BOOK REVIEW

Donald Black (ed.) Toward a General Theory of Social Control; Vol.I Fundamentals; Vol.II Selected Problems. Orlando, Academic Press, 1984 (pp. xiv + 363; xiv + 310).

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The notion of social control covers a rather wide range of meaning (Wolff, 1964). It may focus on constraints experienced by the individual from his social environment, on a species of interaction, on mechanisms regarded as functional in maintaining structure and order in society, on the power, manipulation and self-interest of those in control. The general theory toward which these volumes intend to make a contribution focuses on social control as normative interaction. The volumes contain twenty-two essays by twenty authors. Naturally it is impossible to review each essay separately, and I apologize beforehand if I do not do justice to everybody.

Social control is defined by Black (others, like Ekland-Olson, II: 209 f. use slightly different definitions) as including "all of the practices by which people define and respond to deviant behavior", that is "conduct that is regarded as undesirable from a normative standpoint, conduct that ought not to occur" (I:5). The concept of social control is thus of considerable scope. While originally referring, as Black says, to practices intended to influence people to conform to social order (I:4), it now comprises any kind of reaction to undesirable behavior, any behavior by which people express grievances: "... social control is present whenever and wherever people express grievances against their fellows" (I:5). In other words, the idea that social control is a function of social order, as well as the question whether social control actually controls anything, are not part of the theory. Therefore, we have not only social control from above, but also from below (Baumgartner, I:303ff.), when subordinated people pursue grievances in various ways, such as rebellion, non-cooperation, appeals to the powerful or to public opinion, flight and distress.

I am afraid that the concept of social control, when it is made to include every kind of behavior expressing grievance or disapproval - such as scowling, ridicule, shaming, weeping, mourning, song-matches, gossip, sorcery, political protest, riot, strike, guerilla, war, crime, suicide, self-mutilation, spirit-possession and mental illness (and why not adultery, divorce, alcoholism and drug addiction?) - will soon lose all meaning and usefulness as an analytical tool for social scientific research. Americans have always had a great reputation for pragmatism, but I wonder what practical purpose is served by labeling almost everything "social control" which can be seen as reflecting or expressing a judgment about someone else's behavior. A case in point is Black's essay "Crime as Social Control" (II: 1-27). There is no need to argue that people often see no other way to express a grievance than to commit a crime, e.g., killing an adulterous partner or stealing from an oppressive boss. So in Black's definition crime is often social control. But one cannot leave the matter at that, as Black virtually does. First, evidently not all crimes express a grievance, so we need criteria for distinguishing those that are social control from those that are not. Secondly, what are the implications of labeling a crime as social control? Should it call for treatment instead of punishment, or remain without response altogether? Black does not say a word about this.

From a theoretical point of view Black's essay falls short of insights elaborated in Yngvesson's interesting essay, "What is a Dispute About?" (II: 235-259). No act is a crime per se. It is defined as such in the course of its treatment by officials. The police officer, for instance, has the power to decide whether a beating is a "crime" or a "family matter". Moreover, the act may be defined differently in different universes of discourse without implying any contradiction. What is discussed as "crime" in the police office or the court, may be discussed as "family matter", an "affair of honor", or "just a lot of fun" in other circles, and may get still other analytical labels in the discourse of social scientists. But "essentially" it is neither the one thing nor the other.

Black believes that "the theory of social control provides a radical alternative to theories of deviant behavior of every kind" (I:20, note 19). But it really is no more than their dialectic counterpart. Whether you define protest, theft or suicide as deviant behavior or as a response to the deviant behavior of others, the underlying normative supposition is the same: normal people in a decent society do not protest, steal or commit suicide. Black does not make that supposition problematic. It is not as easy as Black assumes to get rid of the connotations of "social control" in its traditional sense. "Deviance" and "order"

are a dialectical pair in the structural-functionalist paradigm of "social control". You can't just put the concept of "order" in brackets. Whenever you speak of "deviance", you speak implicitly about something from which is deviated. So it is no wonder that the focus of Black's theory of social control remains on normative reactions (punishment, demands for compensation, scolding, etc.), to conduct which is generally perceived as defying the social order, as well as on the procedures through which such normative reactions can be channeled (pacification, mediation, arbitration, adjudication, repression) (I:21). Social control remains a structural-functionalist concept, even if one in disguise.

Black's shift from "law" to "social control" may be inspired partly by the wish to find a category that is more amenable to empirical research than "law", but this can only prove to be an illusion. There exists no empirical phenomenon that corresponds to the analytical notion of "normative behavior". Whistling, for instance, can be a form of social control, but whether it is depends inter alia on the intentions of the whistlers. And they may disagree among themselves as to its exact meaning. But the labeling of their behavior by the actors themselves cannot be decisive for purposes of social science. A researcher may analyze particular behavior as a kind of social control although the actors hardly identify it as such, and vice versa. And other social scientists may disagree with both. Social control is not an empirical phenomenon: whether some kind of behavior can be called "social control" is a matter of interpretation and evaluation.

It is therefore nonsense to speak about the quantity of social control and to formulate all kinds of quasi-exact variations and correlations. You cannot count or measure social control (or law, for that matter). Let me give only one example. One of Black's well-known generalisations is that "Law appears to vary inversely with other social control, for instance, to be stronger when normative life of other kinds is weaker and vice versa" (I:15). This certainly reflects a conservative popular belief, but is it true? Naturally you can only speak in an exact sense about such an inverse relation on the assumption that the total amount of control (legal and otherwise) in any given society is a definite, measurable magnitude. But this is absurd. And Black nowhere makes plausible the idea that it could be done. There are, moreover, indications which go against the supposed inverse relationship. Law and social control often compete with each other. In the Middle Ages adultery fell under the jurisdiction of church and state, but could also be punished by village justice or "rough music". The history of "rough music" in Europe is the history of a sustained fight of church and state against other social control. Social control can develop without there necessari-

ly having previously been any legal regulation. In the case of the enforcement of marriage promises in Staphorst we found, contrary to popular belief, no identifiable relation (inverse or otherwise) between former legal remedies and recent village justice (Van den Bergh et al., 1980). On the other hand, law may retire without other social control stepping in (adultery in modern urban society). There are, moreover, cases with which social control cannot cope effectively (pollution across international borders), and others which the law cannot rule effectively (solidarity in the family and neighborhood).

I have never believed in definitions of law as a species of social control, however popular these may be. My doubts have only increased while reading these volumes. One problem immediately springs to the eye. You cannot extend the concept of social control to all behavior expressing or responding to grievances and at the same time maintain that law is a species of social control. Criminal law may be so conceived, but procedural law hardly can, and constitutional law, codified contract law, social security law, etc. can only be conceived of as responses to grievances in a very indirect, historical sense.

Seeing law as a species of social control leads to a one-sided attention to dispute processing, conflict resolution and regulation, that is, to legislative, adjudicative and executive action. Those who define law as a species of social control usually exclude all expressive, symbolic and ideological functions of law. But law is much more than a variety of normative behavior. It is a form of discourse about society, a language of emancipatory aspirations, an educational project, the symbolic representation of the historic justification of the existence of the state, the legitimation of existing power relations, the process by which we try to balance power and freedom. Law is an ongoing critical discussion and an intellectual enterprise.

Secondly, the paradigm of conflict management, with its one-sided focus on legislative, adjudicative and executive functions tends to ignore a series of institutions which form an important part of the legal system and are mainly responsible for the maintenance of its symbolic and ideological functions: law schools, law firms, codes, law reports, legal periodicals, legal publishing companies. As a result, it tends to produce a very limited sociology of law. In the third place the belief in conflict as a central form of social interaction and the mainspring of the clockwork of society is something like a self-fulfilling prophecy. The neglect of the study of trouble-less cases is only one aspect of this ideosyncrasy.

Fourthly, the definition of law as a species of social control inevitable leads into a dead end. Essentially it is a definition per genus et differentiam, but as such it remains incomplete so long as we cannot define the differentia specifica between law and other social control. It has proved impossible to do this in a satisfactory way (as is argued by Griffiths, I:43ff.). Black solves the problem in a rough and ready way: law is governmental social control. But this begs the question: it implies that there is no law outside the state, it reflects a purely positivistic point of view, one that is at variance with the way in which most people - and even most lawyers and social scientists - are used to talking about law. There is absolutely no empirical foundation for such an arbitrary definition and it is contradicted in the same volume not only by Griffiths, but also by Rieder, who strongly stresses the "legal" character of vengeance (I:132ff). There are numerous studies of all kinds of societies without government where people define the things they do as "law". And in many other cases we find practices which analytically seem to be indistinguishable from what we would call "legal" in our own culture.

Griffiths, who gives a series of very convincing arguments against Black's definition of law as governmental social control, believes that the problem of defining the differentia specifica can be avoided. He clearly sees the problem: "law" is a folk concept "suitable only for use within an internal perspective by the participants in a given system of institutionalized social control" (I:38). So the concept of law is unfit for use as an analytical concept for the purposes of social science. The "taxonomic" search for a definition of law as a distinct species of social control is hopeless because it is fundamentally misdirected. Instead of speaking about "law" we should regard relative "legalness" as a dimension of variation: the degree of "division of social control labor" (I:38). But here Griffiths is guilty of causing new confusions instead of resolving the old ones. Every normal person, and indeed every social scientist, will tend to associate "legalness" somehow with "law". If this association is to be avoided, what, then, does the word mean? If "legalness" is no more than a synonym of "specialization in social control", why use it at all? What added meaning does the term convey (or smuggle into our discourse)? Why should greater specialization be more "legal"? Is specialization a distinctive characteristic of "legal" systems? Is social control more "legal" if it conforms to the principles of the trias politica? Is specialized social control more governed by preformulated rules? Does it conform more to the ideals of due process and fair trial? I am afraid that all these questions only lead us back into the "taxonomic cul-de-sac". Moreover, there is enough empirical evidence from great bureaucratic organizations to suggest that greater

specialization does not necessarily produce greater "legalness" in any of the senses of "legal" just mentioned. The concept of "legalness" must be suspected of being an ideological device.

Griffiths is not the first to use "division of labor" in this context. He points to Weber, who saw as a distinctive feature of law, that it is applied "by a staff of people holding themselves especially ready for that purpose" (I:53). But in fact the concept goes back still further, to one of the founders of the Historical School, Von Savigny. He taught that all religious, moral, legal, cultural and other functions originally resided undivided in the people. By a process of division of labor the legal function has devolved upon lawyers, who now represent the people in its legal capacity. Here the ideological function of the concept of "division of labor" stands out nicely. The whole theory serves to legitimize the social position of learned professionals as legal authorities, as makers and authoritative interpreters of the law. Griffiths proposes a cruder version of this idea: specialization is not a kind of delegation by the original incumbent of its legal powers, division of labor in and of itself makes control more "legal".

Apart from this it should be remembered that the concept of division of labor is extremely ill-defined. Following Griffiths we might conclude that village justice in Staphorst (see Van den Bergh et al, 1980; see also Griffiths 1984) scores rather high on the scale of legalness. The leaders of the community decide that something must be done; three gossip centers, lead by elderly women, specialize in producing the necessary consensus by the manipulation of extensive gossip networks; bands of unmarried boys take care of the organization and execution of the verdict. These boys can unquestionably be regarded as a staff "holding itself specifically ready for action meant to guarantee obedience" (Griffiths, I:53). They have the spare time required and their specialization is quite functional as well: unmarried boys are marginal to the established social relations of the village and can therefore carry out a socially dangerous task without risking durable harm to existing social relationships.

I am not sure that Griffiths' theory would be adequate to deal with such questions. Nicely revealing his own folk concept of law, he says that social control labor can be divided "for heuristic purposes" into legislative, adjudicative and executive labor: "Rules must come into being, be ascertained, interpreted and applied, and be carried out" (I:60). In the case of Staphorst, however, the status of the rule itself is rather doubtful and a distinct specialization of legislative and adjudicative functions cannot be observed. And what would we think of the "legalness" of an institution which scores high with regard to

the legislative, but low with regard to the adjudicative function? Griffiths seems well aware of this problem, but as far as I can see does not provide us with an answer.

My main objection to this kind of theorizing is that it treats law as an empirical phenomenon, which it is not. There are numerous persons and institutions specialized in the production, management and distribution of law. In as much as these (and their behavior) belong to the phenomenal world, they can be counted and measured. But law itself is not a phenomenon but an interpretive and evaluative concept. You cannot count or measure law. With law it is the same as with news. There are all kinds of persons and institutions engaged in the production, management and distribution of news. You can count journalists and presses, headlines and columns, minutes of radio and television broadcast, words, periods and sentences. But "news" itself is not a phenomenon that can be counted and measured. What is news is a matter of interpretation and evaluation.

I strongly believe that it is wrong to treat normative concepts as if they were references to empirical phenomena. It is not just that scientistic quasi-exactness is bad science. It is dangerous as well, because it may lead to statements like those recently made in the Netherlands by conservative police-officers and prosecutors: Law cannot do everything to make society safe, social control must increase again. This is an invitation to vigilante activities, spying, invasions of privacy and all the rest. This is what you get when you identify the rule of law with empirical social control. Normative concepts can only be discussed meaningfully in a normative discourse.

I am convinced that any attempt at the creation of a purely "scientific" study of the normative aspect of social life is doomed to be self-destructive. There are only two possibilities. Either we take a strictly empiricistic view and accept that a sociology or anthropology of law cannot study "law" itself, but only observable behavior and normative expressions of people and institutions who say that they are occupied with "law". In that case, we must recognize that the concept "law" is a folk concept unfit for analytical purposes. Social scientists should stop using it, just as biologists long ago gave up to talk about "life", and natural scientists in general about "causality". This approach implies that a social science of law is not about "law" and has no direct relevance for lawyers. Or we must take a more urbane view of methodology and accept that in sociology and anthropology of law not only analytical concepts and methods are necessary, but also interpretive and evaluative, in short hermeneutical ones. In my opinion the second opinion is the more realistic and fruitful one, and I do not see what can be said against it

from the point of view of scientific methodology. It is a fairy tale that "really scientific" sciences use only analytical methods. Most sciences - and even natural ones like astronomy and physics - show a mixture of analytical and hermeneutical methods (Albert, 1976: 60). What else can theories about the expanding and contracting universe be than interpretations? From a strictly "scientific" point of view they are not analytically established facts, nor can they be verified or falsified in any exact sense.

But most importantly it is a matter of intellectual hygiene not to use the language of an exact science, with "behavior", "quantity", "inverse relation", "variable", "direct function", while in fact we are interpreting of social phenomena with a combination of analytical and hermeneutical methods. We should not forget that all the ethnographic material which is used in these volumes to build a general theory of social control is at best the result of participant observation, which is a clearly hermeneutical method. And a large part of the theories formulated by classical authors, such as Weber and Durkheim, which are constantly invoked in these volumes, are mainly based on speculation.

There are a few minor points which I cannot refrain from making. If you cannot think of more than two sets of two variables, you can always construct the well-known quadrant (Koch, I:99; Horwitz, I:213; Ekland-Olson, II:216) to get something seemingly significant:

	A	B
X	ax	bx
Y	ay	by

But if you have more than, say, four variables, the temptation seems irresistible to construct an "evolutionary" sequence. Naturally this has nothing to do with scientific evolution theory. The sequence is simply based on an author's intuitive assessment of relative "simplicity", "authoritativeness" or whatever. No scrap of evidence is usually produced to make the sequence historically plausible. A case in point is Rohrl's scale of the evolution of compensation (I:196). As an antidote one should read the pages in which Rieder convincingly criticizes the popular belief that vengeance is replaced by formal law in the course of historical development (I:134f.).

Nader (I:71f.) is quite right in stressing that one-sided attention to dispute-processing made legal anthropology overlook a very important "legal" institution in modern industrial society: the management of complaints. But why should this useful insight be encumbered with a blurred discussion of legal evolution?

If we take the general evolutionary scheme (hunters and gatherers - pastoralists - horticulturalists - industrial society) for granted and if in a special case we have enough evidence to situate a given society unequivocally on that scale, there may be some point in labelling specific legal institutions we find in that society correspondingly. But there is no reason why, for instance, the use of a third party in disputing is by itself more developed than dyadic dispute settlement. We find the latter in every society, from the most primitive to the most developed. Sweeping statements such as "the demand for disputing mechanisms increases with an expanding society" (Nader I:73) are either truisms or not true.

Collier's interesting if highly speculative essay "Two Models of Social Control in Simple Societies" (II: 105-140) shows another remarkable feature. Several times she thinks she is able to correct the findings of other researchers on whom she relies without independent field research of her own (II:121, 132 note 21). This reminds one of the baron of Münchhausen. And what to think of this enigmatic note: "This account of why juniors work for seniors is both oversimplified and ultimately wrong, but it is adequate for the limited purpose of this paper" (II: 126 note 15)?

Humphries and Greenberg's essay "Social Control and Social Formation; a Marxian Analysis" (II: 171-208) is as disappointing as any exercise in scholastics, though some of their material is interesting. Beginning with the hope that they can show "how Marxian theory can contribute to the development of a general theory of social control" (II: 171), they end by acknowledging that "a general Marxian theory of historical change in social control cannot be framed ..." (II: 200). No rabbit in the hat, after all.

Reading both volumes is, I must confess, a rather tiring experience. Continuous repetition of the same kind of argumentation, generalization and theory-formation, based on the same set of epistemological presumptions, sociological speculations (especially Durkheim seems to be a favorite) and ethnographies makes for monotonous fare. And in the end one often remains with too many direct functions to construct a manageable theory. Let me give some examples. In Black's introductory essay we find the following propositions: "Normative variation is a direct function of social diversity" (I:17) and "Normative variation is a direct function of the quantity of social control" (I:20). How can we combine these statements? How can a variable be a direct function of two different factors? This is only possible when both factors are identical or vary in an identical way. If so one of the two is redundant. The same can be said of the following two

propositions: "the authoritativeness of settlement behavior is a direct function of the relational distance between the settlement agent and the principals" (I:22) and "the autoritativeness of settlement behavior is a direct function of the relative status of the third party" (I:24). Here one of the two propositions is clearly redundant. If the second proposition is correct, only vertical distance between principals and settlement agent, not relational distance by itself is decisive.

Apart from too many direct functions there are also too many truisms in these volumes. A nice example of a pompous collection is Felstiner's essay on the logic of mediation (I: 251-269), which contains such statements as proposition 20: "Success in mediation is dependent on the degree to which its structure is appropriate to its function." Is this not just as true of chimpanzees and automobiles? What specific content does it have regarding mediation?

As I said at the beginning, I cannot venture to discuss all the essays in detail. Inevitably, there are some very good ones (Rieder on vengeance, I:131f.; Merry on gossip and scandal, I:271f.; Baumgartner on social control in suburbia, II:79f.; Yngvesson on the political interpretation of social control, II:235f.; Nelson on history and social control, II: 283f.). There are also some rather poor ones (Koch on liability, I:95f.; Rohrl on compensation, I:191f.; Gross on social control under totalitarianism, II:59; Ekland-Olson on social control and relational disturbance, II:209f.). But unsubstantiated verdicts like these always remain arbitrary and unsatisfactory. No doubt other readers will give other marks in many cases. I would like to emphasize that the critical remarks I have made should not be misunderstood. Anyone interested in phenomena of social control cannot afford to ignore these volumes and will learn a lot of them, as I did myself by studying them carefully and critically.

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