CAUSATION IN CRIMINOLOGICAL THEORIES AND RESEARCH[1]

Virtually all criminological theories are causal in that they focus on variables purportedly responsible for crime. Although some crime theorists eschew the term "cause," they substitute such terms as "influences," "leads to," "affects," "determines," "structures," "prevents," "creates," "depends on," "brings about," "increases (or decreases)," "results in," "is due to," "produces," and "forces" (Glenn, 1989). It is important that criminological theories are causal because non-causal or covariational theories lack policy applications. One of the principal reasons to construct causal theories in the social sciences, whether they are about crime or any outcome variable, is to apply them, that is, to use them to identify effective intervention policies for individuals or populations (Freedman, 1997; Glymour, 1997; Hart and Honore, 1985; Marini and Singer, 1988). In the case of crime, the idea is that if X causes crime, then we may be able to identify an intervention to effectively change X, thereby reducing, if not eliminating, crime.

Criminological theories tend to recognize multiple causes, as do theories about most other outcome variables in the social sciences (Ragin, 2000). There is recognition of multiple causes in theories outside the social sciences as well, including the experimental sciences. However, in theory testing in the experimental sciences, the effects of particular causes can be separated from the effects of other causes through randomization. For example, if W, V and X are purported to be causes of Y, the independent causal effects of X can be estimated by randomly assigning cases to values of X, manipulating X, and then observing the values of Y. Randomization will help to ensure that the effects of W and V on Y are controlled, leaving only the causal effects of X on Y to be observed. Randomization is often impossible in the social sciences, and this is especially true when it comes to tests of criminological theories. For example, if a theory attributes lawbreaking by juveniles to such causes as child abuse, parents' divorce, and school failure (as does Agnew, 1992), it is morally unacceptable to randomly assign juveniles to abusive and non-abusive families in order to estimate the independent causal effects of child abuse (Glenn, 1989). Without the possibility of randomization, tests of criminological theories usually rely on statistical controls (or partialing) of variables with multiple regression or a similar statistical technique. However, applied statisticians have long questioned use of these techniques for drawing causal inferences because of the need to make strong a priori *theoretical* assumptions that cannot be checked or validated (Clogg and Haritou, 1997; Ragin, 2000; Glenn, 1989; Smith, 1990). Among the more important for testing criminological theories, use of these techniques assumes that the causal effects of a set of variables are (1) uncorrelated with the effects of left-out causes (omitted-variable bias) and (2) the same across all cases (causal homogeneity).

The purpose of this paper is to examine these kinds of assumptions and other issues

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about constructing and testing theories of crime causation. This is done by considering: (1) causal versus spurious effects, (2) independent versus shared causes, (3) reversible versus irreversible causes, and (4) basic versus superficial causes. Then, attention shifts to the issue of causal homogeneity, which presents an especially serious challenge to constructing theories of crime causation. The paper is more epistemological (methodological) than ontological out of a belief that attention to a wide range of epistemological issues can improve criminological theories and provide a systematic basis for more effective intervention policies. While the causal issues considered here lend themselves mainly to quantitative considerations, causal inferences are no less problematic in qualitative criminological research on crime. However, those problems are different from the ones that challenge quantitative theory and research and, thus, are beyond the scope of this paper.

BRIEF DEFINITIONAL CONSIDERATIONS

There is no accepted definition of "cause," and it is widely believed that any attempted definition is destined to fail. There is no desire to join the definitional debate here. For the purposes of this paper, H.L.A. Hart's (Hart and Honore, 1985:29) definition will suffice: "a cause is essentially something which interferes with or intervenes in the course of events which would normally take place" so as to change that course of events (also see Freedman, 1997:116; Stinchcombe, 2005:255). As such, a "cause" is different from both a necessary and sufficient condition. Suppose that a fire results when someone drops a lit cigarette on combustible material. Although oxygen in the air is a necessary condition for a fire, normally we would not think of it as causing the fire; instead, we would think of the lit cigarette as the cause (Hart and Honore, 1985). Now suppose that a man shoots another man and kills him. Although deprivation of his blood cells of oxygen is a sufficient cause of the man's death, we would not tend to think of it as the cause; instead, we would think of the shooting as causing the man's death. Although deprivation of blood cells of oxygen is a sufficient condition for death, we are more interested in the "cause of death under circumstances which call for an explanation" (Hart and Honore, 1985:39).

BELO HORIZONTE DATA

Discussion of causal issues can be clarified by referring to concrete illustrations. With that in mind, I am going to briefly summarize the multiple regression results of research that Dr. Rodrigues and I have been conducting on the relationship between religious denominational identification and delinquency and drug use. The research uses survey data collected by researchers in CRISP at the Federal University of Minas Gerais from over 3,500 5th -11th grade students in 65 schools in Belo Horizonte and the surrounding metropolitan area. The surveys were administered in classrooms by research assistants who informed students that the surveys were anonymous and that no one from the school would see their responses. Teachers remained in the classrooms, but they completed teacher surveys while the student surveys were being administered.

To measure delinquency, students were asked to self-report about whether they had ever (1) brought a firearm to school, (2) brought another weapon to school, (3) been involved in a gang fight, (4) stolen in school, and (5) committed robbery. They also were asked whether in the past three years they had (6) hit anyone in school. The response options were "none," "1 or 2 times," "3-5 times," and "more than five times," and responses were added to construct a delinquency scale (alpha=.745). Similarly, a drug scale (alpha=.741) was constructed by adding students' responses to questions abut whether they had ever tried (1) alcohol, (2) cigarettes, (3) laughing gas, (4) marijuana, (5) solvents, (6) ecstasy, (7) injectable drugs, (8) cocaine, and (9) crack. The response options were "never tried" and "tried," coded as 0s and 1s. In the U.S. and many other countries, such self-reports of offending are widely considered more accurate than police and court records for measuring delinquency and drug use. One of the key benefits is that they allow researchers to collect data from both non-offenders and offenders, including offenders who have never been caught by legal officials for their lawbreaking. The Belo Horizonte self-report data are among the first collected in Brazil.

The key independent variable is religious denominational identification, which was measured by asking students whether they were "Catholic," "Evangelical," "Other" (which included Spiritualist or Espirita), or "No Religion." Dummy variables were created for each type of religious denominational identification (1=Catholic, 0=not Catholic; 1=Evangelical, 0=not Evangelical, and so on). "No religion" was omitted from the multiple regression analysis as the reference variable. About 50 percent of the surveyed students were Catholic; about 33 percent were Evangelical, and about 10 percent said they had no religion. According to a recent Brazilian census (IBGE, Demographic Census, 2000), Catholics make up about 75 percent of the total population in Brazil and Evangelicals make up about 15 percent, so the distribution of religious denominational identification of the students was slightly different from that among Brazilians as a whole.

The other independent variables were included in the analyses as "controls." "Age" is a continuous variable, ranging from 10 to 17. There are four race dummy variables, "Black," "Mixed," and "Other Race" (including indigenous and mixed-race students), with "White" being the omitted reference variable in the multiple regression analysis. "Gender" was dummy-coded with 1 for males and 0 for females. "Work" is a dummy variable created from students' responses to a question about whether they "worked for money" (0 = no, 1 = yes), and "Gang" is a dummy variable reflecting whether students currently belonged to a gang (0 = no, 1 = yes). To measure socioeconomic status (SES), each student was given a list of 14 types of household items and services, including radios, televisions, videocassettes and DVDs, air conditioners, computers, and household help, and asked how many there were of each type in his/her household (e.g., a student might have answered two radios, three televisions, one computer, and so on).   An additive scale was constructed from the standardardized values of the fourteen variables (alpha=.839). Parental involvement ("Parental") is an additive scale (alpha=.639) constructed from students' responses to these three questions about

the persons responsible for them (usually parents): "Over the last 12 months, did the person(s) responsible for you (1) go to school meetings, (2) help with homework, and (3) participate in school activities open to the community?" The response options were "never," "sometimes," and "always." Although the results are not reported here, the analysis also included dummy variables for each of the 65 schools as controls, with one of the school dummies omitted as the reference variable in the multiple regression analysis.

TABLE 1

Regression of Delinquency and Drug Use on Religious Denominational Identification and Other Independent Variables

Independent Variable	Delinquency		Drug Use	
	b	B	b	B
CATHOLIC	-.335*	-.093	-.382*	-.141
EVANGELICAL	-.293*	-.077	-.503*	-.174
OTHER DENOMINATION	.046	.005	.043	.006
AGE	-.004	-.004	.176*	.232
BLACK	.173	.030	-.083	-.019
MIXED	.062	.017	.128*	.047
OTHER RACE	.245*	.039	.140	.029
GENDER	.702*	.196	-.046	-.017
WORK	.217*	.053	.207*	.066
GANG	2.622*	.326	.763*	.124
SES	.018*	.080	.009*	.053
PARENTAL	-.029	-.026	-.085*	-.101
R2	.223		.203	

* p<.05, two tailed
Note: The regression analyses also included dummy variables for the schools attended by the students.

Table 1 reports multiple regression results for both delinquency and drug use, with unstandardized regression coefficients (b's) and standardized regression coefficients (B's) for the independent variables, along with significance levels.   Since the purpose of this paper is not to discuss what predicts delinquency and drug use, but to examine causal issues in criminological theory and research, a brief summary of findings will have to suffice.

Findings

Students who identified themselves as Catholics and Evangelicals reported significantly less delinquency and drug use than students who identified themselves as having no religion (the omitted reference variable). In contrast, students who self-identified with a religious group other than Catholic or Evangelical did not differ significantly from the "no-religion" students in their reported delinquency and drug use. These findings are similar to what has been found in research in other countries, including the U.S. (e.g., Ellis, 2003; Johnson et al., 2000).

Age was not significantly related to delinquency, but it was significantly positively related to drug use; hence, there was more reported drug use among the older than the younger students. Only two of the six race dummy variables were significant. Black students did not report significantly more delinquency and drug use than white students. Mixed-race students reported significantly more drug use than white students, but they were not significantly different from whites in their self-reported delinquency. "Other-race" students reported significantly more delinquency than white students, but they were not significantly different from whites in their self-reported drug use. Males reported significantly more delinquency than females, but they did not report significantly more drug use. Working for money, gang membership, and socioeconomic status (SES) were significantly positively related to delinquency and drug use, and parental involvement was significantly negatively related to drug use, but not delinquency. Like the findings about religion, these findings are similar to what has been reported in studies in other countries (Empey, Stafford, and Hay, 1999).

CAUSAL VERSUS SPURIOUS EFFECTS

Causal terms were deliberately avoided in the foregoing summary of findings. However, consider the statement that being Catholic or Evangelical *caused* students to commit less delinquency and use less drugs than not identifying with a religion. Of course, that only begs the question: *why* or *how* is being Catholic or Evangelical negatively causally related to delinquency and drug use? Evans et al.'s (1995:197) answer is representative of the kind of causal theoretical answer often provided: some "denominations … broadcast proscriptive moral messages, inculcating their members with moral values that inhibit crime [i.e., *cause* them to refrain from committing crime]." Observe that when a causal term is used in one part of a theory, causal terms often are used in other parts of the theory. However, other causal issues arise immediately, one of them having to do with the distinction between spurious and causal effects.

In a review of recent research, Johnson et al. (2000:37-38) caution that multivariate analyses are necessary to draw "acceptable" causal inferences about the relationship between religion and delinquency. Such cautions are standard in the criminological literature and reflect a belief that the relationship between any variable and crime might be spuriously attributable to other variables; that is, the relationship may be non-causal. Such a belief is ostensibly why many researchers control for such demographic variables as age, race, gender, and socioeconomic status (SES) in analyzing criminological data (for a general discussion, see Glenn, 1989:130). Such demographic variables could be causally related to crime and its covariates. In the case of the religion-delinquency relationship, it is believed that such variables as work and parental and peer influences may be sources of spuriousness (Benda and Corwyn, 1997; Evans et al., 1995). For example, religion and delinquency could be related only because both variables are caused by involvement of parents in the lives of their children. If this is the case, as conventional reasoning goes, the regression coefficients for "Catholic" and "Evangelical" should not have been significant when parental involvement was included in the multivariate analysis. According to this reasoning, the findings in Table 1 should give researchers the confidence to conclude that there is a causal relationship between religion and delinquency. However, such a conclusion may be erroneous. One reason is "omitted-variable bias" (Clogg and Haritou, 1997). Even if a statistical analysis includes a wide range of variables as controls to detect spuriousness, it is possible that the *true* source of spuriousness is omitted from the analysis, perhaps because it is unknown. There is no statistical technique, however sophisticated, that can identify an unknown source of spuriousness, since that is a theoretical, not a statistical issue. The more general issue is summarized by Clogg and Haritou (1997:106): If it can be known with certainty that a model about the relationship between X and Y is "causally" right when Z is included and "causally" wrong when Z is omitted, "then of course the casual effect can be identified." The problem is that this can never be known with reasonable certainty about any purported causal relationship in non-experimental research.

OTHER CAUSAL ISSUES

The issue of "causal versus spurious effects" has to do with whether X actually causes an outcome variable. However, even when it can be assumed that X *is* the outcome variable's cause, there are important causal issues regarding (1) independent versus shared causes, (2) reversible versus irreversible causes, and (3) basic versus superficial causes.

Independent versus Shared Causes

Researchers often attempt to identify variables that are independently causally related to crime, that is, do not share causal effects with other variables (for a general discussion of the issue in the social sciences, see Glenn, 1989:133). To illustrate, consider a simple theory that crime is caused by both race and socioeconomic status or SES. Since race and SES are likely to be associated, a statistical analysis may show that neither is independently related to crime. This may be a reason why the race coefficients in Table

1 mainly were non-significant. Among the Belo Horizonte students, SES was positively related to being white and negatively related to being black or mixed-race. Hence, the race variables could have become non-significant when SES also was included in the multiple regression analysis. In fact, this *was* not the case. The race variables were not significantly related to delinquency and drug use when considered apart from SES or any of the other independent variables in the analysis. However, even if a variable is not independently related to an outcome variable when other variables are considered in a statistical analysis, the variable may be no less important to consider (Turner, 1997). Sekhon (2003:24) gives an example of a statistical analysis of the relationship between race and uncounted election ballots in the 2000 U.S. Presidential election, which shows how shared effects sometimes are more important than independent effects:

> If one were able to estimate a regression model ..., which showed that there was no relationship between the race of a voter and her probability of casting uncounted ballots … when one controlled for a long list of covariates, it would be unclear what one has found … Before any regression is estimated, we know that if we measure enough variables well, the race variable itself … will be insignificant. But in a world where being black is highly correlated with socioeconomic variables, it is not clear what we learn about the causality of ballot problems from a showing that the race coefficient … can be made insignificant.

Similarly, Marini and Singer (1988:356-57) also illustrate how researchers may be interested only in the:

> disjunctive plurality of causes that may produce an effect … If an individual is identified as having high susceptibility to several causes of death and dies shortly thereafter, this information offers some explanation of why the individual died but does not single out the actual cause of death. It may be irrelevant to know which of several possible causes produces an effect.

Many theorists ignore the distinction between independent and shared causes, choosing instead to let researchers disentangle the issue, but most theories of crime causation would benefit from explicitly recognizing that most causes of crime are probably shared. For example, in his strain theory, Agnew (1992) argues that, among individuals, delinquency is caused by anger and other negative emotions that are, in turn, caused by negative life experiences, such as child abuse, failure in school, divorce of parents, and loss of a girlfriend/boyfriend. It may be interesting in an abstract sense to learn which negative life experiences are independently related to delinquency. However, many negative life experiences covary as when an abused child also fails in school, and this makes them no less important causes of delinquency. Moreover, in the case of policy applications, it would be incredulous to address only those negative life experiences that are independently related to delinquency and drug use and ignore the rest.

Reversible versus Irreversible Causes

As previously indicated, it is important that most criminological theories are causal because of their potential policy applications. However, causes lend themselves to effective intervention policies only if they are reversible. If an increase in X causes an increase in Y, then X is a reversible cause of Y if a decrease in X also causes a decrease in Y. However, if an increase in X causes an increase in Y, then X is an irreversible cause of Y if a decrease in X does not cause a decrease in Y. Many causes of crime probably are irreversible. According to Gottfredson and Hirshi (1990), crime is caused by low self-control, coupled with criminal opportunity. Low self-control forms in early childhood as a function of ineffective parenting and is immutable after that. Intervention cannot increase people's self-control and thereby decrease their propensity to commit crime. Similarly, Moffitt (1993) argues that neuropsychological impairments, which can be inherited or caused by such factors as maternal alcohol and other drug use, poor prenatal nutrition, and brain injury, cause persistent offending across the life course for some offenders. These life-course persistent offenders have poor "verbal skills … and …[weak] self-control" and cannot be rehabilitated (Moffitt, Lynam, and Silva, 1994:280).

There are many examples of irreversible causes outside of criminology. Among populations or for any population over time, the incidence of lung cancer increases when many people smoke, and it decreases when many people stop smoking. Hence, smoking is a reversible cause of cancer at the population level. However, at the individual level, smoking is an irreversible cause of lung cancer, since cessation of smoking will not eliminate it (though it will reduce its likelihood among smokers who have not yet acquired it). Hence, among individuals, the cause of lung cancer is different from what causes its cure (Hart and Honore, 1985:36). Uggen and Piliavin (1998) argue that a similar situation exists with crime because the reasons why people initiate criminal behavior probably are different from the reasons they may later desist from it. Moreover, they argue that it may be easier to translate causes of desistance into effective intervention policies:

> Our ability to isolate the true causal effect of critical etiological factors such as parents, schools, and neighborhoods is constrained by our inability to manipulate the selection mechanisms guiding their allocation. For both social scientific research and for policy purposes, manipulation of these factors is unacceptably invasive in a democratic society. The researcher conducting a desistance study has a more legitimate and expansive *license to intervene* in the lives of participants (Uggen and Piliavin, 1998:1412-13).

The Belo Horizonte findings in Table 1 show how a cause of crime may later become a cause of desistance from it. In particular, work may cause Belo Horizonte students to commit delinquency and use drugs (observe the estimated positive relationship

between work and delinquency and drug use), but their later desistance may be caused by work or work-related variables.

Basic versus Superficial Causes

According to Lieberson (1985), social scientific theories have tended to focus on superficial rather than basic causes, partly because of adopting an experimental-science model that relies on studying variation. Speaking of the classic image of Sir Isaac Newton sitting under the proverbial apple tree, Lieberson (1985) argues that social scientists probably would identify something other than gravity as the cause of the apple's fall to the ground because gravity is not a variable quantity in earthly situations. Viewed this way, a basic cause is akin to Aristotle's formal cause, which involves the very essence of a thing (Marini and Singer, 1988:363). Theories of crime causation have sometimes posited basic causes. Perhaps the best example is Merton's (1957) theory of anomie, which states that crime in the U.S. is caused by a combination of a basic and a superficial cause. The basic cause is adherence to the American Dream, which according to him, is a goal universally shared by people in the U.S., and the superficial cause consists of opportunities to achieve the American Dream, which some people have more than others. There is little wonder why researchers have tended to focus on the superficial cause more than the basic cause because, according to Merton (1957), the American Dream is a constant that falls outside the scope of conventional research methodologies.

It is likely that many basic causes of crime are variables, not constants and, hence, do fall within the scope of conventional research methodologies, *at least in principle*. The qualification is important because there may be serious difficulties with incorporating variable basic causes in testing theories of crime causation, even though it may be possible to do so "in principle." An example comes from Sampson and Laub's (1993) life-course theory in which they argue that certain life experiences, such as marriage or employment, will cause offenders to desist from crime or at least reduce their offending. However, suppose that both marriage and desistance from crime are causal effects of a "desire to change." The problem is not that marriage and desistance from crime are said to be spuriously attributable to a "desire to change." In fact, this illustrates the previous point that not all spurious relationships can be dismissed as unimportant. The problem is that a "desire to change" is very difficult to measure and, hence, to incorporate in tests of life-course theory even though it is a basic cause and marriage is only a superficial cause.

CAUSAL HOMOGENEITY

In an ideal situation, the causes of crime would be the same across all populations. If X causes crime in one population, say among U.S. residents, then X would cause crime in all other populations, say among Brazilian residents as well as among resdidents of all other countries. However, the situation is far from ideal; the causes of crime often differ among populations. For example, being Catholic and being Evangelical were more

strongly related to delinquency (but not drug use) among Belo Horizonte students in public as compared to private schools and in religious as compared to non-religious schools. Hence, there was evidence of causal heterogeneity. These findings reflect the fact that the relationship between any independent variable and crime will be affected by the joint distribution of other variables related to crime, and this joint distribution can vary among populations. If so, the association between the independent variable and crime likewise will vary among populations (Marini and Singer, 1988:383).

Heterogeneity in the causes of crime may be even more complex than this, involving either disaggregation by type of crime or different units of comparison. To illustrate the first possibility, Parker (1989) reported that different types of homicide may have different causes. In a study of U.S. cities, he disaggregated homicides into: (1) robbery homicides, (2) other-felony homicides, (3) primary non-intimate homicides or those that occurred between friends and acquaintances not known to have a sexual relationship, and (4) family-intimate homicides or those that occurred between spouses. A multivariate analysis included four independent variables: (1) a poverty index of such variables as percent of families with less than $3000 in income and percent of children living with only one parent, (2) income inequality, (3) a dummy variable for southern region, and (4) percent black. Neither income inequality nor the southern-region dummy variable was significantly related to variation in any of the homicide types. However, the poverty index was significantly positively related to other-felony, primary non-intimate, and family-intimate homicides, and percent black was significantly positively related to robbery and primary non-intimate homicides.

Heterogeneity in the causes of crime also may involve different units of comparison. As previously indicated, socioeconomic status (SES) was significantly positively related to delinquency and drug use among the Belo Horizonte students, which is to say there was more self-reported delinquency and drug use among upper SES students than among lower SES students. Although this finding may be contrary to a popular belief that SES is strongly negatively related to crime, it is consistent with research in the U.S., which tends to show either a positive or a weak negative association among individuals between SES and delinquency (Tittle, Villemez, and Smith, 1978). However, there also is considerable evidence of a strong positive relationship *among* U.S. *territorial units*, such as cities and metropolitan areas, between crime and such income variables as the percent of families that live below the poverty line and income inequality. Parker's (1989) research on city-variation in homicide rates is relevant here, as is Blau and Blau's (1982) research that revealed a strong positive relationship among U.S. metropolitan areas between income inequality and rates of violent crime.

The situation is not one where the causes of crime differ from one population to the next. Even for the same population, the causes of crime may differ from one unit of comparison to the next. It may be useful to consider an analogy about the washing of hands. Even if there is a causal relationship among individuals in a particular country between hand washing and disease, there may be no causal relationship between the two variables among cities in the same country. Variation in disease among individuals

might not be affected by factors affecting city-level variation, such as clean water and adequate nutrition. It is not that the one of the causal relationships is right and the other is wrong. They are just different, and that difference should bedevil theorists, researchers, and policymakers.

CONCLUSIONS

Theorists and researchers need to be aware of the possibility of drawing unwarranted conclusions about causation from multivariate analysis of crime data. However, there is no need for despair. Even given the limitations of non-experimental research, evidence for causation can be convincing, it not conclusive, when generated from diverse studies, both quantitative and qualitative. As Glenn (1989:123) states: "certainty may be an illusive goal never to be reached, but the cumulative evidence from studies conducted with different methods may often bring us … close to certainty."

There are other equally, if not more, daunting issues that theorists and researchers need to consider about the causes of crime. The complexity of crime (and perhaps all human behavior) requires a complex treatment of causation, including but not limited to the possibility that crime may involve independent and shared causes, reversible and irreversible causes, and superficial and basic causes. The alternative to these more complex views of causation is likely to be ineffective intervention policies.

Finally, there is considerable evidence that the causes of crime may be heterogeneous rather than homogeneous, with the heterogeneity dependent on type of population, type of crime, and unit of comparison. An ideal theory would apply to all populations, types of crime, and units of comparison. However, there is no existing theory that achieves that ideal. At this time, all theories of crime must be considered partial, and researchers should continue to search for ways of integrating them.

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