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Age and the Explanation of Crime[1]

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One of the few facts agreed on in criminology is the age distribution of crime. This fact has been used to criticize social theories of crime causation, to provide the foundation for other theories, to justify recent emphases on career criminals, and to support claims of superiority for longitudinal designs in criminological research. In the present paper, we argue that the age distribution of crime is sufficiently invariant over a broad range of social conditions that these uses of the age distribution are not justified by available evidence.

According to a recent criminology textbook (Allen et al. 1981, p. 235), age is the easiest fact about crime to study. In one sense, the statement is true: the age of the offender is routinely recorded, and age distributions of crime covering a variety of contexts over a long period are not hard to find.[2] As a result, no fact about crime is more widely accepted by criminologists. Virtually all of them, of whatever theoretical persuasion, appear to operate with a common image of the age distribution. This distribution thus represents one of the brute facts of criminology. Still, the statement that age is an easy fact to study is decidedly misleading. When attention shifts to the meaning or implications of the relation between age and crime, that relation easily qualifies as the most difficult fact in the field. Efforts to discern the meaning of the large amount of research on the topic in terms supplied by those doing the research have

[2] Most of the available data on age and crime are "official" data—i.e., data on arrestees or prisoners. They are, therefore, subject to traditional criticisms of official data they may reflect biased enforcement rather than the behavior of offenders. The logical forms such bias could take are virtually endless. For example, juveniles may be more likely than adults to be arrested either because they commit a larger portion of their offenses in groups and groups are more easily apprehended, or because they are less skilled in evasion. Or juveniles may be less vulnerable to arrest than adults because of leniency. Empirical examination of such biases, with data from self-report and victimization surveys, suggests that the biases do not account for the relation between age and crime (see, e g., Empey 1978)

turned out to be futile (e.g., Wootton 1959, chap. 5; see also Wolfgang, Figlio, and Sellin 1972, p. 105), as have efforts to explain the relation in statistical terms (Rowe and Tittle 1977).[3]

Faced with this intransigent fact, the response in criminology has been generally scientific and logical. Theorists are frequently reminded that their explanations of crime must square with the age distribution, and theories are often judged by their ability to deal with "maturational reform," "spontaneous remission," or the "aging-out" effect (Matza 1964; Hirschi 1969; Empey 1978; Siegal and Senna 1981). Although some theories fare better than others when the age criterion is invoked, no theory that focuses on differences between offenders and nonoffenders avoids altogether the complaint that it provides an inadequate explanation of the age distribution. Given the persuasiveness of the age criticism of traditional theories, it is not surprising to find recent explanations of crime explicitly tailored to fit the accepted variability in crime by age (Matza 1964; Greenberg 1979; Trasler 1980). In fact, there is reason to believe that age could replace social class as the master variable of sociological theories of crime (see Empey 1978; Glaser 1978; Greenberg 1979).

On the research side, the age effect has been instrumental in the rise of the longitudinal study to its current status as the preferred method of criminological research. The major studies of the past decade, including several still under way, have used this design (Wolfgang et al. 1972; West and Farrington 1973; Elliott, Ageton, and Huizanga 1978; McCord 1979; Wadsworth 1979). This research emphasis gains much of its attractiveness from the association between age and such concepts as "career criminal," "recidivism," and "desistance," all of which are thought to be of considerable theoretical and practical import and all of which are thought to require, by definition, longitudinal designs for their study.

Given the increasing role of age in criminological theory and research (e.g., Elliott, Ageton, and Canter 1979; Greenberg 1979; Farrington 1979; Matza 1964; Petersilia 1980; Zimring 1981) and the widely accepted critique of sociological theories on the basis of the age effect (e.g., Matza 1964; Hirschi 1969; Empey 1978; Siegel and Senna 1981), it seems to us that those in the field should consider the possibility that current conceptions of the age effect and its implications for research and theory are misguided. To that end, in this paper we advance and attempt to defend

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[3] The Rowe and Tittle article is a thorough attempt to account for the age effect in a large sample of respondents 15 and older whose criminality was measured by self-reports. Although the authors report isolating a segment of their sample (of unknown size) in which there is no relation between age and crime, we believe this group is composed largely of respondents whose self-reports are manifestly unreliable. It is consistent with our hypothesis that the relation between age and crime is stronger in the "reliable" portion of the sample than in any of the many other subgroups Rowe and Tittle isolate.

the following theses: (1) the age distribution of crime is invariant across social and cultural conditions; (2) theories of crime that do not explicitly attend to age have no logical or empirical obligation to do so and should not be judged by their apparent ability or inability to account for the age effect; (3) the age distribution of crime cannot be accounted for by any variable or combination of variables currently available to criminology; (4) explanations focusing explicitly on the age effect must be compatible with an apparently direct effect of age on crime; (5) the conceptual apparatus that has grown up around the age effect is largely redundant or misleading; (6) identification of the causes of crime at any age may suffice to identify them at other ages as well; if so, cohort or other longitudinal designs are not necessary for the proper study of crime causation.

We recognize the difficulty in establishing some of our theses, especially those that deny either the significance of variability not yet investigated or the power of explanations not yet advanced. Nevertheless, we find nothing in the available research literature inconsistent with our position, and we find a good deal to support it. Furthermore, we consider our specific theses to be logically connected to such an extent that we are obligated to explore each and all of them; were we to discard the difficult or inconvenient issues, we would in effect grant the untenability of our entire position, something we are not now prepared to do. If currently popular approaches to the age distribution are inadequate, there seems little reason to pursue them along traditional lines without considering an alternative position.[4]

1 THE AGE EFFECT IS INVARIANT

Theoretical and textbook discussions of the age effect often presuppose or flatly assert (the former is more common) variations in this effect over time, place, demographic subgroups, or type of crime (Empey 1978, p. 391; Jensen and Rojek 1980, pp. 70–71; Reckless 1973, p. 81; Glaser 1978; Allen et al. 1981, pp. 234–35). Typically, the current age distribution of crime in the United States as revealed by the Uniform Crime Reports (UCR) is shown and the reader is left with the impression that this distribution is only one of many such distributions revealed by research.

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[4] Collectively, reviewers have recommended that we seriously qualify or abandon the majority of our theses, including the argument that the age effect is invariant, which is the key to the remainder The general complaint is that our assertions are "too strong," "sweeping," or "unsubstantiated." We believe that our assertions are derived from the evidence, although we grant that in many cases they go beyond it. If we are wrong, it should not be too hard to show that we are wrong. The risk to the field of considering our hypotheses would therefore appear to be minimal

Time and Place

Figures 1, 2, and 3 show three age distributions of criminality:[5] one from England and Wales in 1842–44 as reported by Neison in 1857, another from England in 1908 as reported by Goring in 1913, and another from the most recently available UCR for the United States (U.S. Department of Justice 1979). Looking at one of these distributions, Goring (1913) concluded that the age distribution of crime conformed to a "law of nature."[6] The similarity between the three distributions is sufficient to suggest that little or nothing has happened to Goring's law of nature since he first discovered it. The shape or form of the distribution has remained virtually unchanged for about 150 years.[7] Recent data, the basis for many assertions of variability in the age distribution, force the same conclusion: ". . . while population arrest rates have changed in absolute magnitude over time (almost doubling between 1965 and 1976), the same pattern has persisted for the relative magnitudes of the different age groups, with fifteen to seventeen year-olds having the highest arrest rates per population of any age group" (Blumstein and Cohen 1979, p. 562).

We do not know how England and Wales in the 1840s differed from the United States in the 1980s. Presumably the differences are large across a variety of relevant dimensions. We do know, however, that in the 1960s, the age distribution of delinquency in Argentina (DeFleur 1970, p. 131) was indistinguishable from the age distribution in the United States, which was in turn indistinguishable from the age distribution of delinquency in England and Wales at the same time (McClintock and Avison 1968). If the form of the age distribution differs from time to time and from place to place, we have been unable to find evidence of this fact.

[5] The figures presented in this paper are only illustrative of many similar figures that could be constructed from the literature. The following sources contain age-crime distributions for Belgium, Sweden, West Germany, France, Italy, Russia, and Japan, respectively, at various times in this century, largely since World War II: Swedish National Central Bureau of Statistics 1980; Jacquart, n.d.; Mannheim 1965; Rangol 1962; Besson 1961; Ponti 1962; Callcott 1935.

[6] Goring contrasted the observed distribution of age at first conviction among habitual criminals with two theoretical probability distributions (Pearson's Type I and III) and found a poor fit. However, he concluded that exclusion of the youngest age groups (10–20-year-olds) produced a close fit between observed and expected Type I distributions. Arguing that interference by artificial agencies produces the initial incompatibility between the two distributions ("the efforts of the law to postpone its penalties in the case of juveniles"), Goring concluded that "the age-frequencies at first conviction of habitual criminals . . . obey natural laws of frequency . . ." (Goring 1913, p. 211).

[7] This conclusion is supported by data from Quetelet (1969), U.S. prison statistics (U.S. Department of Commerce, Bureau of the Census 1893; Best 1930), and by the UCR over the period in which age statistics have been published (1930s to 1980). Although data from the Soviet Union are sparse, available sources indicate that they are probably comparable: "Soviet writers generally assert that delinquency is concentrated in the 'troublesome' years between 15 and 17" (Conner 1970, p. 286). See also the studies cited in n. 5 above.

Demographic Groups

Most discussions of the age distribution in a theoretical context assume important differences for demographic subgroups. Textbooks often compare rates of increase in crime for boys and girls for particular offenses, thus suggesting considerable flexibility in the age distribution by sex. "Age-of-onset" studies easily suggest that, say, black offenders "start earlier" than white offenders; such a suggestion gives the impression that the age distribution of crime varies across ethnic or racial groups (see, e.g., Wolfgang et al. 1972, p. 131). Figures 4 (sex) and 5 (race) show that such suggestions tend to obscure a basic and persistent fact: available data suggest that the age-crime relation is invariant across sex and race.[8]
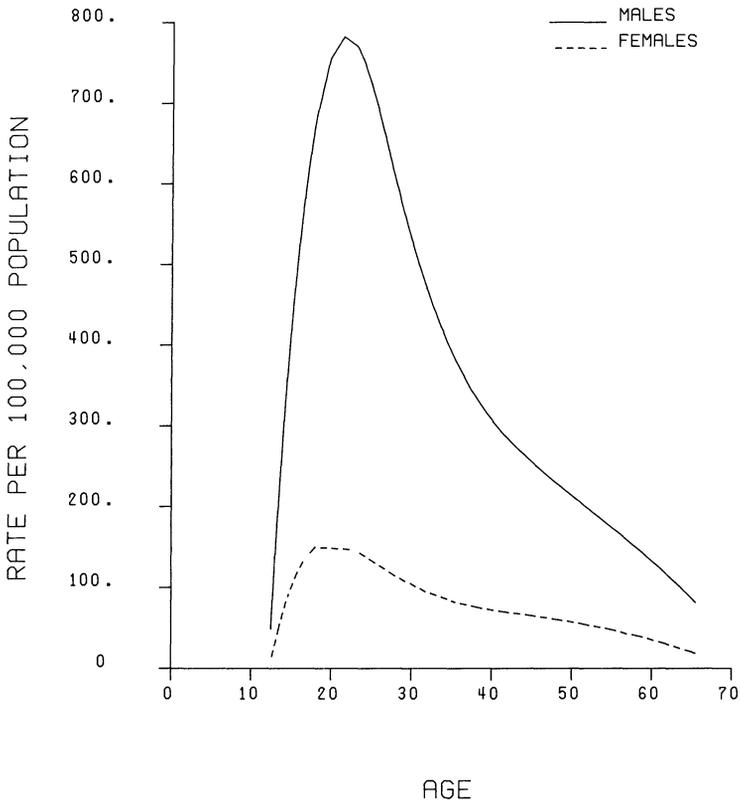
FIG. 1.—Criminal offenders in England and Wales, 1842, 1843, 1844, by age and sex. Rates per 100,000 population. (Source of data. Neison [1857, pp. 303–4].)

[8] See also fig. 1, where the Neison (1857) data for males and females are drawn to the same scale. At first glance, the figure suggests that the age distribution of crime for females is "flatter" than that for males, an impression sometimes reported in the literature. The fact is that it is not flatter. On the contrary, except for differences in level, the curves are virtually the same.

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Type of Crime

A consistent difference in the age distribution of person and property offenses appears to be well-established, at least for official data. In such data, person crimes peak later than property crimes, and the rate declines more slowly with age. The significance of this fact for theories of criminality is, however, problematic. For one thing, self-report data do not support the distinction between person and property offenses; they show instead that both types of offense peak at the same time (see, e.g., Elliott et al. 1978) and decline at the same rate with age (Tittle 1980). The peak years for person and property offenses in self-report data are the mid-teens, which are also the peak years for property offenses in official data. In contrast, person offenses in official data peak in the late teens or early twenties.
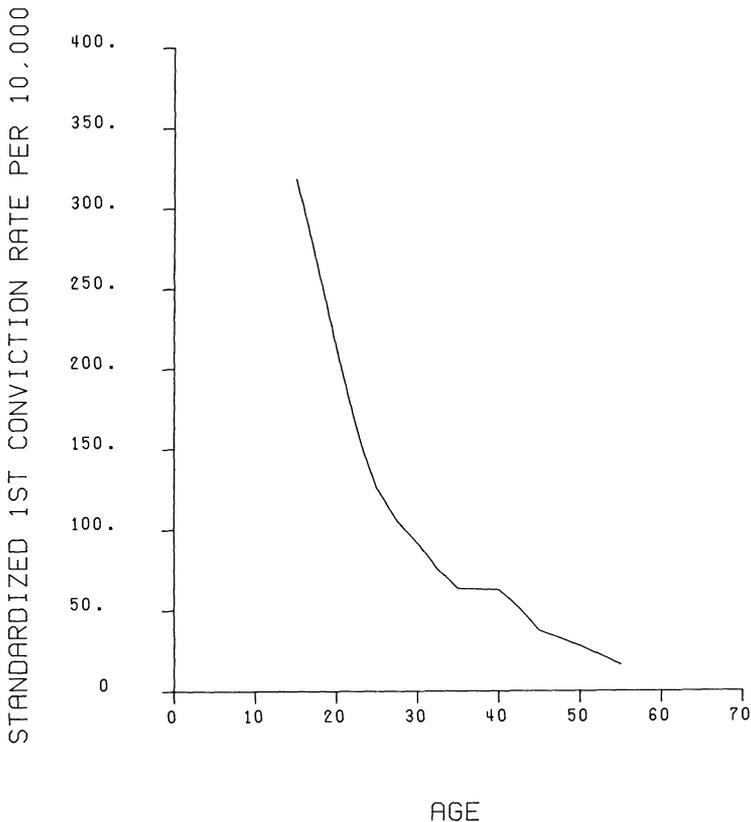

FIG. 2 —Frequencies of age of criminals at first conviction as a percentage of the frencies of age in the general population. Males over 15 years, England, 1908. (Source of ta. Goring [1913, pp. 201–2].)

If the self-report results are taken as indicative of the level of criminality, the difference in the peak years for person and property offenses in official data may be accounted for by age-related differences in the *consequences* of person and property crimes. One of these differences lies in the seriousness of offenses. Wolfgang and his colleagues report that "injury seriousness scores advance dramatically at each offense rank number," while the increase in seriousness for theft offenses is negligible (1972, p. 171). Offense rank is correlated with age (as a group, second offenders are older than first offenders). It should follow that age is positively correlated with the seriousness of injury offenses but not with the seriousness of theft offenses. By extension (and this is consistent with everyday observation), "injury" offenses by the very young are unlikely to be sufficiently serious to attract the attention of officials. Indeed, as long ago as 1835, Quetelet (1969) presented data on the correlation between physical strength and
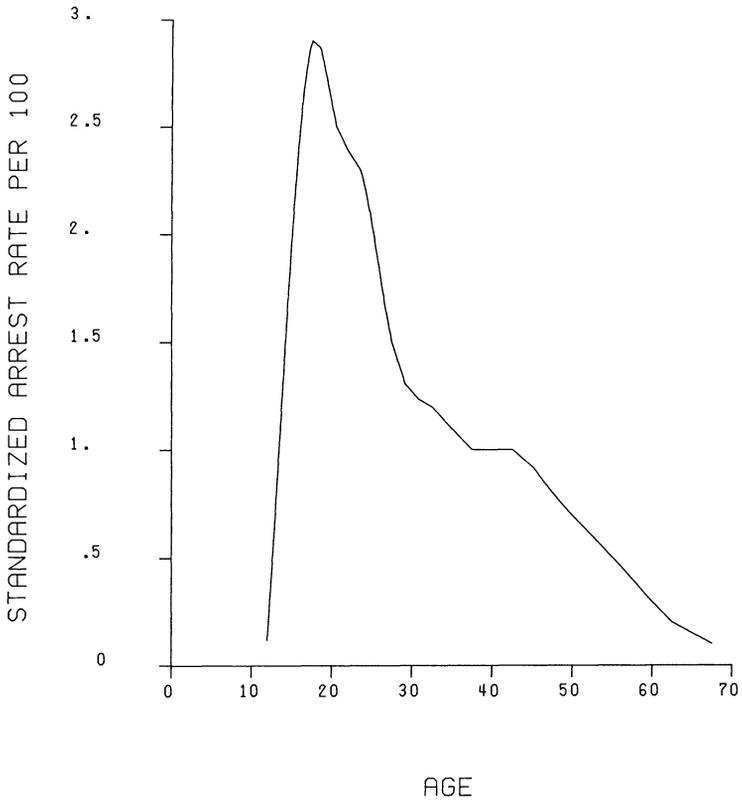

FIG. 3.—Age distribution of persons arrested in the United States for all offenses, standardized for their representation in general population, 1977. (Source of data: U.S. Department of Justice [1979, p. 171]. N.B.. Data are approximate.)

age alongside data on the age distribution of crime, the idea being that some crimes appear only when the strength necessary to inflict injury or coerce others has been attained. Apparently, the tendency to commit criminal acts, as reflected in theft offenses, however measured, and in violent offenses, as measured by self-reports, peaks before the physical ability necessary for serious violent offenses. The peak age for person offenses is thus a consequence of the confluence of the "tendency" and "ability" curves. Since strength continues to increase after the peak age of criminality has been reached, the person-crime curve declines from a later point. For a brief period, increases in the dangerousness of offenders more than offset their declining tendency to commit offenses.

The slower decline of person offenses in official data may reflect the fact that a greater proportion of such offenses involve primary group
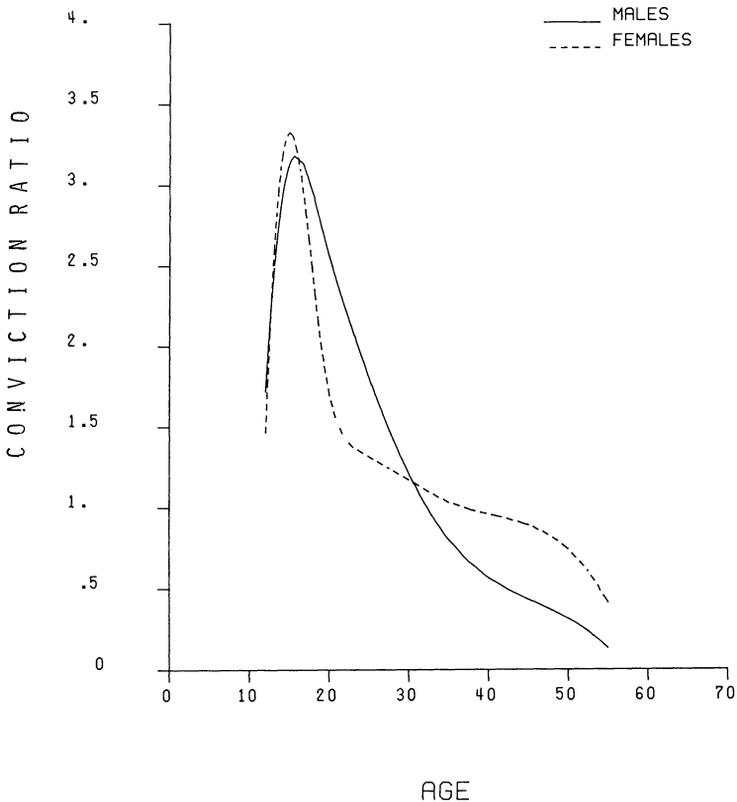

FIG. 4.—Age distribution of persons found guilty of indictable offenses, standardized by sex, England and Wales, 1965. Conviction ratio. percentage of offenders in age group divided by percentage of population in age group. (Source of data: McClintock and Avison [1968, p. 170].)

conflicts. Primary group conflicts may be assumed to be relatively constant over the age span and to produce a relatively stable number of assaultive offenses during the period of capability (i.e., among those neither very young nor very old). If these offenses were subtracted from the total number of person offenses, the form of the age curve for person offenses would approximate more closely that for property offenses. These speculations are consistent with the self-report finding of no difference between person and property crimes with respect to the long-term effects of age (Tittle 1980, p. 92).

Since our thesis is that the age effect is invariant across social and cultural conditions, it may appear that our explanation of the apparent difference between person and property crimes requires modification of our thesis. Actually, in some social conditions, the effects of age may be muted. As people retreat into the primary group context with increasing
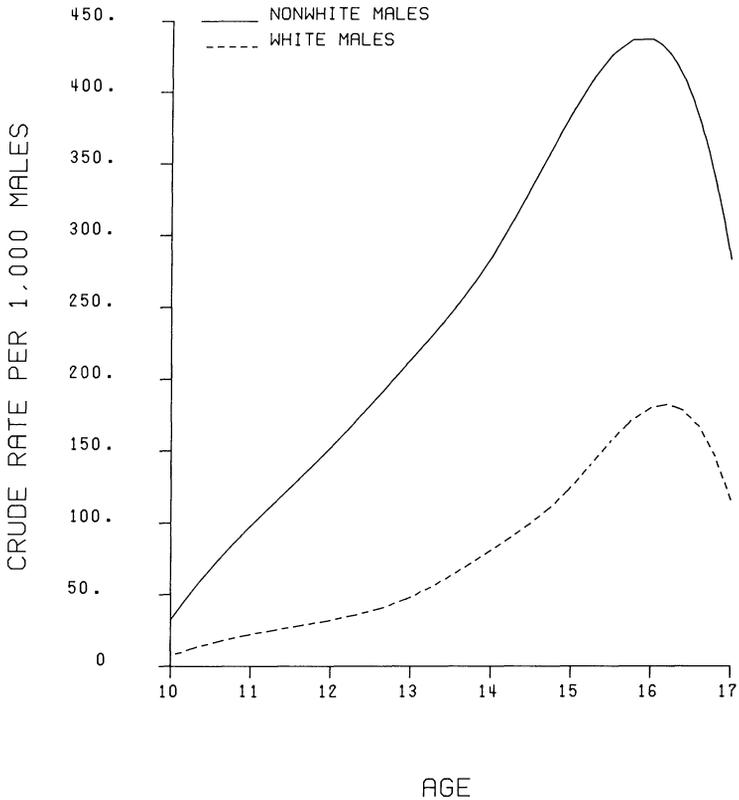

FIG. 5.—Delinquency rates by race and age. (Source of data: Wolfgang et al. [1972, p. 109].)

age, the relatively rare criminal events that occur in this context continue to occur. Outside the primary group context, the effects of age on person offenses show themselves even more clearly. So, while we may find social conditions in which age does not have as strong an effect as usual, the isolation of such conditions does not lead to the conclusion that age effects may be accounted for by social conditions. On the contrary, it leads to the conclusion that in particular cases the age effect may be to some extent obscured by countervailing social processes.[9]

Artificial Conditions and Behavior Analogous to Crime

Theories designed to explain age effects focus on the social position of youth vis-à-vis adults, suggesting that if their situations were identical, the differences in their crime rates would disappear. One way to test such theories would be to construct an artificial environment in which age varies and the forces said to create the age relation are held constant. For example, if differential labor force participation is said to account for the age effect, we could test this thesis by creating an environment in which no one participates in the labor force. All this is more easily said than done. The closest we can come to a natural approximation of an environment that holds at least some of the putative causal variables constant is the prison. For research, prison populations have the advantage of being relatively homogeneous on many crime-causal variables, since they

[9] Our invariance hypothesis was generated by observation of the stability of the age distribution of crime over a variety of conditions. It was intended to be merely an empirical generalization stating a relation between observable variables. Defense of such generalizations in the face of variation across indicators, however minor, requires some degree of conceptualization. In the discussion here, we have substituted "tendency to commit criminal acts" for "crime," and our invariance hypothesis has thus become "the age distribution of the tendency to commit criminal acts is invariant across social and cultural conditions." This revised hypothesis is not strictly at the mercy of the facts and is therefore not necessarily contradicted by the observation that the relation between age and various *indicators* of crime is not precisely the same under all conditions. We would have to grant, for example, that removal of large portions of high-crime-rate groups from a population could reduce the crime rates for those groups (especially if [a] the individuals removed remained in the denominator of the rate or [b] those removed were selected for their higher likelihood of crime), but we would not have to grant that the data produced by such an experiment contradict or falsify our invariance thesis. Our initial inclination was to attempt to defend the age-crime relation as an empirical generalization without benefit of general concepts or theory, in order to stress the similarity of the shape of the distribution under all conditions. We have learned, however, that theory-free facts are no match for fact-free theories. Reviewers and colleagues have repeatedly demanded theory or have derived from theory statements that "contradict" our thesis. The standard way of dealing with this problem is to follow the presentation of facts with a summary of various theories. This procedure allows facts and theory to borrow truth from each other (see Galtung 1967, pp. 453–54) and is eminently satisfying. The simple fact is, however, that this solution is closed to us. We cannot simultaneously maintain that the age-crime relation is beyond current theory and suggest that this relation supports and may be derived from such theory.

are relatively homogeneous on crime.[10] As shown by figure 6, which presents prison infraction rates by age, when "practically everything" is held relatively constant, the age effect is much like the age effect in the free world.[11]

Another way to approach the problem of the confounding of age with other causal variables is to isolate an item of behavior analogous to crime but lacking at least some of its components, such as socially induced motivation. Automobile accidents satisfy some of these criteria. Such accidents do not suggest economic need or exclusion from the means of production; they do presuppose a minimum of ability, an ability that quickly reaches its maximum and then slowly but steadily declines; furthermore, the various dimensions of personality thought to be associated with accidents are not thought to be correlated with age.

Figure 7 shows the motor vehicle accident rate in New York State among those eligible to drive, by age. As is evident, these data closely parallel those for crime. We will have reason to discuss the theoretical import of these accident data subsequently.

2. THE AGE CRITIQUE OF THEORY IS UNJUSTIFIED

Most current theories of crime concentrate on the adolescent and late teen years, when the rate of crime is at or approaching its maximum level.

[10] Given that the correlations between crime-causal variables and crime are often weak, prison populations are far from homogeneous in a statistical sense In fact, in this sense, these populations may tend to be more heterogeneous than the population as a whole. (Minorities tend to contribute more than their share to the offender population.) Nevertheless, within groups relatively homogeneous on crime, crime-causal variables tend to lose their ability to predict subsequent criminality. For example, Glueck and Glueck (1970, pp. 174–80) report that very few of the "traits and factors" that differentiated delinquents from nondelinquents in adolescence continued to differentiate offenders from nonoffenders among the delinquents followed into adulthood.

[11] Similar relations between age and rule breaking in prison have been reported repeatedly (Ellis, Grasmick, and Gilman 1974; Wolfgang 1961; Zink 1958; Mabli et al. 1979). Flanagan (1981, p. 3) reports that "the most adequately established correlate of misconduct among prison inmates is age." It is difficult to obtain age-standardized prison infraction data. The data in fig. 6 were constructed in the following fashion: the age bases represent the population of male inmates by age category in New York State prisons, as of December 31, 1975 (source: New York State 1976, table 2G). The infraction data are derived from a sample of releasees, drawn to be representative of all male releasees from New York State prisons in 1973–76. The number of releasees sampled was 758; the number of infractions recorded was 4,293. Thus, the data are limited in two respects: the infraction data reflect the experience of a sample of releasees, while the base data are for all persons confined, and the time periods are not identical. The infraction data represent codings from the institutional history file of the sampled inmates. Any recorded adjudication by either of two disciplinary bodies— the prison adjustment committee and the superintendent's proceedings—were coded. These are, essentially, all formally adjudicated infractions. Data on ages of persons in prison were available only for the catgories 16–18, 19–20, 21–29, 30–39, 40–54, and 55 and older. The midpoints of these categories were used as the basis of the figure presented in the text. As far as we can see, the biases in this procedure would tend to affect the location of the distribution rather than its form.

The general strategy is to identify or construct high- and low-rate groups, to differentiate between delinquents and nondelinquents. However differentiation is accomplished—whether by labeling, exposure to definitions favorable to delinquency, lack of legitimate opportunity, reinforcement of incipient delinquent conduct, or lack of social restraint—the result is identification of groups unusually likely to commit criminal acts.

   Standard research procedure in testing such theories is to compare the actual crime rates of the groups they identify. Although in practice the theories may be difficult to test because of ambiguity or inconsistency, there is in principle little disagreement about how they should be tested. If differential opportunity is said to be the key to delinquency, one defines opportunity operationally and compares the delinquency rates of those having more with those having less. Up to the actual initiation of tests, there appears to be no necessary empirical defect in such theories. And since they are at least in principle testable, there would appear to be no necessary logical defect in them either.
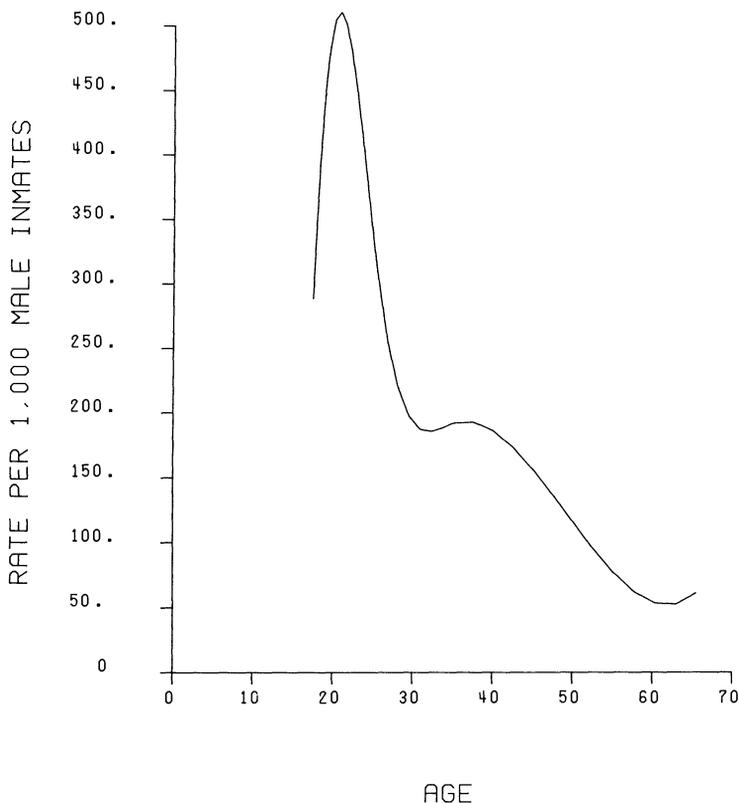
FIG. 6.—Prison infractions per 1,000 inmates, New York State, 1975. Graph presents nalysis of raw data from Flanagan (1979) and New York State (1976).

Enter the brute fact, the age distribution of crime. Just at the point where the criminal group has been created, it begins to decline in size. "Maturational reform" or some equivalent unexplained process takes over. The theory is then said to be able to explain the onset of crime but unable to explain desistance from crime. Since "desistance" is equal in theoretical significance to "onset," this failing of the theory is considered to be a failing sufficiently serious to bring its explanation of the onset of crime under a cloud of suspicion: "Since most delinquents do not become adult criminals, can we assume that somehow their social bonds eventually are strengthened? How is this possible? Control theory does not adequately answer these and similar questions" (Siegel and Senna 1981, p. 139). And: "Social process theories do not account for the 'aging out' factor in delinquency. This is a fault of the . . . social structure approach as well" (Siegel and Senna 1981, p. 147).
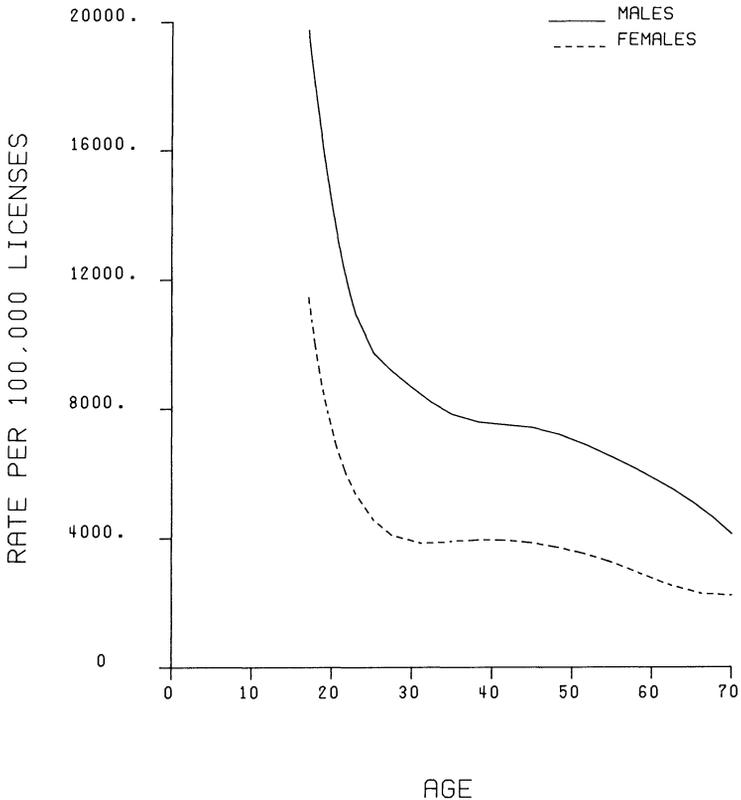
FIG. 7.—Motor vehicle accidents by age and sex. Rates per 100,000 licenses in force, New York State, 1977. (Source of data: New York State [1979].)

This by now traditional criticism should be understood for what it is: a theoretical argument dressed as a logical and empirical argument. The empirical fact of a decline in the crime rate with age is beyond dispute. The requirement that theories account for facts is also beyond controversy. But it does not follow that a theory that adequately differentiates criminals from noncriminals will also account for the effects of age. What makes the argument theoretical is that it *requires* that the age distribution of crime be accounted for by the variables explaining crime rate differences at a given time. This amounts to an assertion that the age effect on crime cannot be independent of the variables employed by an accurate theory of crime. Yet it could be that a given theory, in which the rate for the low-rate group is simply a constant proportion of that for the high-rate group, holds true at all age levels. Figure 8 illustrates this possibility. It shows a true theory unaffected by "maturational reform." This theory differentiates offenders from nonoffenders throughout the life cycle. Its failure to account for the "aging-out" factor in crime cannot therefore be taken as a "fault" of the theory since the aging-out effect occurs constantly in each group. Clearly, until evidence against this plausible hypothesis
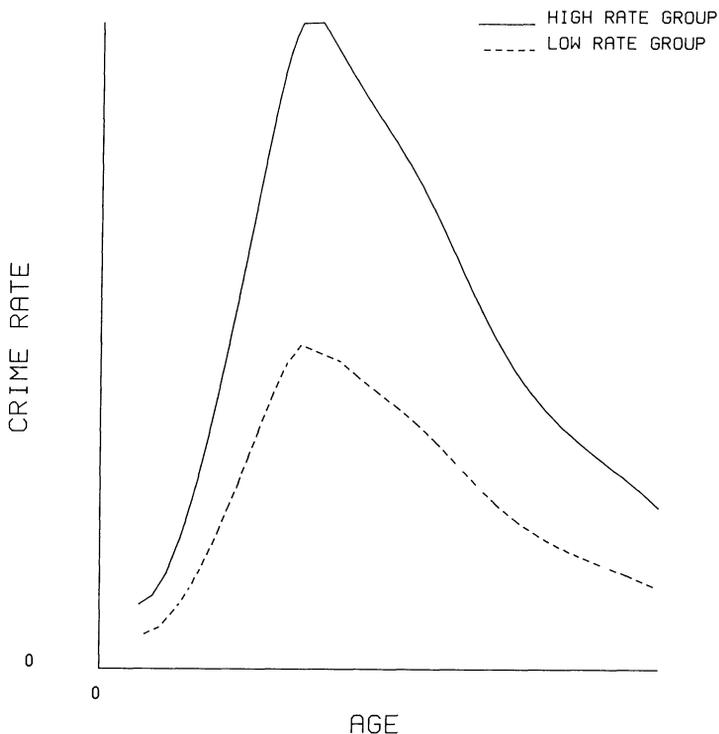

FIG. 8.—True theory unaffected by age

has been located, there is no justification for using age as a critical weapon against any current theory of crime.

This point may be illustrated by application of the logic of age-based critiques of social theories of crime to the motor vehicle accident data displayed above. No one would argue that the impact of driver training on accidents is inadequate as an explanation of variation among drivers because it fails to account for the age effect. Indeed, insurance companies that routinely give premium discounts for persons with driver training do not neglect the age variable. More generally, it is beyond question that age affects the likelihood of motor vehicle accidents regardless of the social characteristics of drivers. It should be mentioned also that the natural costs of accidents are usually far greater than the social costs and/or formal penalties. There is, therefore, no reason to believe that social control can account for the shape of the age distribution of accidents.

Thus, if the possibility depicted in figure 8 describes the actual situation, efforts to bring theories into line with the age distribution, to encompass the effects of age, will lead the theorist into assertions contrary to fact.

For example, Sutherland and Cressey (1970, p. 126) argue that the theory of differential association can account for the apparent effects of age. Presumably this means that age is correlated with exposure to particular constellations of definitions favorable to violation of law, and that in groups where there is no change in definition, there will be no change in the likelihood of crime over the life cycle. Yet research shows that, in accord with our thesis, "even with equal exposure to criminal influences, propensity toward crime tends to diminish as one grows older" (Rowe and Tittle 1977, p. 229).

Again, this fact does not invalidate the theory of differential association. On the contrary, it is exactly what we would expect were the theory (or any theory) true *and* independent of age. The reader will note that figure 8, though hypothetical, closely approximates actual subgroup differences reported elsewhere in this paper. It therefore seems safe to say that (1) the argument that theories of crime must take age into account is itself a theory of crime, and (2) the theory underlying this argument is contrary to fact.

3. THE AGE EFFECT CANNOT BE EXPLAINED WITH AVAILABLE
   CONCEPTS

Our third thesis is a corollary of the thesis that the age distribution of crime is invariant across social conditions. If the age effect cannot be even partially explained by historical trends or cross-cultural comparisons, if it is unaffected by introduction of such gross correlates of crime as sex and race, if it appears when other known causes of crime (including

crime itself) are held constant, and if it shows itself in phenomena analogous to crime that lack many of the elements typically encountered in explanations of crime, then there is reason to believe that efforts to explain the age effect with the theoretical and empirical variables currently available to criminology are doomed to failure.

The exceptional plausibility of theories of crime that focus on age is, nonetheless, hard to deny. Ironically, a major source of their plausibility appears to be the robustness or universality of the relation between age and crime. Since no one doubts that age (unlike, say, social class) is an important and powerful predictor of crime, it follows that theories capable of explaining this relation are also important and powerful (and probably true). The fact that social or cultural theories that explain the age effect automatically predict variation in the age-crime relation does nothing to diminish their plausibility. On the contrary, predicted variation serves to enhance the plausibility of the theory predicting it, whether or not such variation is actually observed. Other sources of plausibility include the ability of age theory to address historical trends as well as current differences in crime rates and its ability to use a variety of sociological perspectives in a noncompetitive fashion (different perspectives are used to account for different features of the age distribution).

Given the plausibility of age-based explanations of crime, it should be instructive to examine the logic and empirical adequacy of the most prominent theory built explicitly on age, that presented by David Greenberg (1979) in "Delinquency and the Age Structure of Society."

Greenberg first focuses on theories that address within-age variation. These theories are said to "shed little light on the relationship between crime and age" (1979, p. 589). The theories of Miller, Cohen, and Cloward and Ohlin are all argued to be defective vis-à-vis the age distribution of crime. This leaves the door open for theories that take the age distribution as the direct focus of attention. Theories said to be partially adequate in this regard are those of Bloch and Neiderhoffer (age status problems) and Matza (drift accompanied by reduction in male status anxiety). As one would expect, these partially adequate theories have difficulty explaining *within*-age variation, but they are taken by Greenberg as pointing in the right direction.

In the end, Greenberg proposes a theory combining strain and control which, he says, provides "a very plausible account of age and other systematic sources of variation in delinquent involvement . . ." (1979, p. 591). The strain comes from exclusion of youth from access to the means of production, with its resulting material deprivation and masculine status anxiety. Control enters through increasing legal penalties and increasing social integration ("stakes in conformity") with age. Greenberg focuses on four "facts" about the age distribution of crime: (1) the peak age of

crime is late adolescence, (2) the peak age declines over time,[12] (3) the peak age varies by type of offense, and (4) offenses decline in number with age. The strain component of the theory is said to explain the first three of these facts, the control element the fourth. Greenberg's argument is plausible to the extent that the cross-sectional theories on which it is based are plausible. But whatever the plausibility of the source theories, there is no empirical reason to believe that any combination of them explains the age distribution of crime. Let us turn first to the strain component in Greenberg's theory.

Adolescent Deprivation Replaces Class Deprivation

Greenberg catalogs the needs of adolescents ("clothing, cosmetics, cigarettes, alcoholic beverages, narcotics, phonograph records, transistor radios, gasoline for cars and motorcycles, tickets to films and concerts, meals in restaurants, . . . gambling" [1979, p. 593]) and argues that the deterioration of the position of adolescents in the labor force has "progressively eliminated" legitimate employment as a source of the funds required to meet these needs. The argument, then, is that adolescents are a deprived class and that their (relative) deprivation has increased steadily in recent years: ". . . teenagers [are] less and less capable of financing an increasingly costly social life whose importance is enhanced as the age segregation of society grows. Adolescent theft then occurs as a response to the disjunction between the desire to participate in social activities with peers and the absence of legitimate sources of funds needed to finance this participation" (1979, p. 594).

The plausibility of "poverty" explanations of crime is beyond question (see, e.g., Blau and Blau 1982). Whether applied to differences among adolescents or between adolescents and adults, these theories make a great deal of sense. We should note, however, that there is no more evidence for Greenberg's theory than for the general strain or motivational theories from which it is derived. These general theories have consistently shown themselves to be less than adequate in dealing with basic cross-sectional facts about crime (Kornhauser 1978). For example, they automatically predict that employed teenagers will be less likely than unemployed teenagers to steal, that teenagers with access to legitimate funds will be less likely to steal, and so on. The data from delinquency research simply do not support these predictions (West and Farrington 1977; Hir-

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[12] Daniel Glaser (personal communication, 1982) predicts an increase in the peak age and a flatter age distribution over time Both of these predictions, he notes, "contradict our theme of a constant age-crime relationship." Both of Glaser's predictions also contradict those advanced by Greenberg. As we have noted, neither set of predictions is consistent with the evidence. Therefore neither "contradicts" our theme.

schi 1969, pp. 188–89). In fact, they show a relation between individual affluence and delinquency opposite to that predicted by strain-poverty-deprivation theory.

If a theory mispredicts cross-sectional differences, there is little reason to think that it is adequate as an explanation of age differences in crime. But there is direct evidence on the age aspects of the theory. Tittle (1980, p. 92) shows that the age distribution of self-reported offenses is unaffected by controls for sex, race, marital status, socioeconomic status, state of residence, size of place, religion, place of childhood residence, family background, or, most important in the present context, labor force status. In none of the 36 demographic categories identified in Tittle's analysis does the negative correlation between age and deviance drop below $-.40$ (gamma) (1980, p. 92). (Critical to Greenberg's theory is the fact that two of Tittle's items measure theft.)

A complex strain theory is hard to test. Rarely, in fact, do such theories restrict their attention to combinations of variables currently available in the research literature. For this reason, even the rare data analyzed by Tittle (and by Rowe and Tittle 1977) are not sufficient to the diverse claims of Greenberg's presentation, a major feature of which is that the age distribution of crime in modern capitalist society differs from the age distribution of crime in earlier periods, or, by extension, in less industrialized, less capitalistic societies.

But the similarity between the age-crime distributions through time (1835–1980) and across place (Argentina, the United States, France, Sweden, Japan, England, and Wales) is remarkable (see figs. 1–3 and n. 5 above). In shape or form, they are virtually identical. The major or only difference is in the location of the curves along the age axis. In early 19th-century England and France, crime peaked at a later age than it does in late 20-century America. What is the significance of this fact?

Variation in the age of maximum criminality has been widely noted (e.g., Sutherland and Cressey 1970, p. 122). The peak age varies across time and place and by type of offense. Person offenses tend to peak later than property offenses and have done so for some time (Quetelet 1969, p. 93).

Interestingly enough, Greenberg begins his discussion of the *crime-type* difference in peak age with what may be seen as an explanation of the *over-time* difference: "Over time, the 'democratization' of the family has reduced the age at which given levels of autonomy are acquired" (1979, p. 596). But the theoretical problem is to explain the later peak age of person offenses—rape, robbery, homicide, assault—and the generally slower decline of such offenses after the peak age has been attained: the answer Greenberg provides is "masculine status anxiety" engendered by "the contradiction between the school's expectations of docility and submission

to authority, and more widely communicated social expectations of masculinity" on the one side and "inability to fulfill traditional sex role expectations" (1979, pp. 604–5) on the other: "One would expect masculine status anxiety to appear with greatest intensity and to decline most slowly in those segments of the population in which adult male unemployment is exceptionally high. This conforms to the general pattern for violence offenses . . ." (1979, p. 605). There are difficulties with this explanation, too. As we argued above, if the self-report data (Tittle 1980; Tittle and Rowe 1977) showing no difference in peak age for property and person offenses are taken at face value, the difference in the peak years for person and property offenses in official data is a function of differential response by age rather than a function of differential causal factors operating to produce person and property offenses. Evidence of differential causal factors by type of crime is nonexistent. Indeed, the persistent search by criminologists for "types of offenders" has met with repeated empirical failure (see, e.g., Wolfgang et al. 1972; Peterson and Braiker 1980). Since the data strongly imply that the same individuals are involved in both "types" of offenses, explanations such as Greenberg's that posit a shift in the operative causal factors with age confront empirical (as well as logical) difficulties.

Social Control Explains the Decline in Crime with Age

Having cataloged the various motivational factors in crime that may reach a peak at the point of maximum criminality, Greenberg turns to a discussion of the cost or social control aspects of criminality. He begins by rejecting the notion that internal controls or moral inhibitions increase with age sufficiently to account for the age-related decline in delinquent conduct. External costs are, however, another matter: "Parents and teachers are generally willing to write off a certain amount of misbehavior as 'childish mischief,' while enormous caseloads have forced juvenile courts in large cities to adopt a policy that comes very close to . . . 'radical nonintervention' for all but the most serious cases. . . . [However], as teenagers get older, the potential costs of apprehension increase; victims may be more prone to file a complaint, and police to make an arrest. Juvenile court judges are more likely to take a serious view of an older offender . . ." (1979, pp. 606–7). Nor is the justice system the only source of increased costs with increasing age: "Just as the costs of crime are escalating, new opportunities in the form of jobs, marriage, or enlistment in the armed forces create stakes in conformity and . . . may also relieve problems of masculine status anxiety" (1979, p. 607). (Differences in the age distribution between blacks and whites, lower and middle class, and

by type of offense are all said to be consistent with this thesis [1980, pp. 607–8].)

There are several difficulties with this argument about the effects of external social control. (1) No data on the age distribution of crime show the discontinuous decline in the rate of crime one would expect were formal sanctions responsible for the decline. Since penalties for crime depend on age, one would expect to observe in the data the transition of people from one set of sanctions to another. But the data do not reflect such movement. Instead, they evidence a continuous, monotonic decline in crime rates with age once the peak has been attained (whatever this peak age may be). In fact, Rowe and Tittle (1977, p. 231) show that all four types of deviance they investigated (theft, gambling, assault, tax cheating) are negatively correlated with age when delinquency of ac-quaintances in childhood, social integration, utility (of the behavior), moral commitment, and sanction fear are partialled out. They conclude that their results "clearly show that the explanations offered, when each is considered alone, are inadequate to account for the persistent age/crime relationships" (1977, p. 232). (2) The factors Greenberg adduces to explain desistance offer a plausible account of crime rate differences between late adolescence and early adulthood, between, say, 19 and 24, but they do not provide a plausible account of the similar decline in crime rate be-tween, say, 29 and 34. Yet in all of the data, including the data provided by Greenberg, the decline in this five-year period is as steep as in the earlier five-year period. (3) Greenberg's account of the situation of ju-veniles vis-à-vis the criminal justice system clearly suggests that the true crime rates in early adolescence may be higher than those in late adoles-cence, that, in other words, the peak age of "criminality" may be even earlier than crime statistics suggest. If so, the economic need, the mas-culine status anxiety, and the formal-external control hypotheses are called into question. Fourteen-year-olds do not need money for cigarettes, al-coholic beverages, narcotics, gasoline for their cars, and for gambling—unless, as Greenberg's description of their "needs" often suggests, they are already delinquent—or if they do need such things, it is clear that they are likely to need them less than, say, 17-year-olds. For, the fact is, the use of cigarettes, alcohol, narcotics, and gasoline increases steadily throughout the teen years, while the rate of clearly criminal offenses (especially, in official data, theft offenses) peaks in middle adolescence and then begins to decline.

The best that can be said is that contemporary arrest data peak a few years earlier than conviction or incarceration data from earlier periods. For purposes of discussion, let us assume that the maximum age of crim-inality has indeed declined slightly in the past 60 years. (Contemporary

incarceration data may even cast this assumption in doubt.) The assumption of a declining age of maximum criminality is taken to reflect change in the social position of youth. The social position of those at the current maximum is not identical to the social position of youth at the same age some time in the past. Therefore, it seems to follow that the relation of age to crime has changed. But what if we compare the social position of youth at the current maximum with youth at the historical maximum? What do we conclude? Apparently, the social situation of the two groups is similar—otherwise, each time period would require a unique explanation of the period of maximum criminality (and any age group could, in principle, occupy this position).

Thus, an explanation that argues that high rates among contemporary youth are a product of exclusion from legitimate means of satisfying peer and media engendered needs (Greenberg 1979) would have to argue that 25-year-olds in Wales in 1842 were also so excluded and were also so vulnerable to peer and media consumption pressures. Thus, even if a nontrivial difference in peak age were present in the data, an explanation of the current pattern that did not recognize the existence of a virtually identical form to the age distribution in earlier periods would encounter serious logical and empirical difficulties. It is much simpler and more consistent with the data merely to assume that the age effect is virtually invariant over the range of social conditions for which data are available, and that small changes in the peak age may reflect either (1) the earlier emancipation or (2) the earlier physical development of youth. That is, with the exception of minor fluctuation in mode, the essential feature of the age distribution is extraordinary stability.[13]

In sum, Greenberg's argument concerning the theoretical importance of age is explicit and rests on two principles:

1) age variation may help to test delinquency theories constructed to explain other sources of variation, such as class or sex. Since these other sources of variation can be explained in many ways, the adequacy with which

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[13] Whether the peak age is 17 or 19, or whether this peak age varies somewhat by offense or by sex, may be quite insignificant in contrast to the stability of the major parameters of the age-crime distribution. In other words, variation in the location of the curve on the age axis across time, place, and such demographic factors as sex or even type of offense may say little or nothing about the impact of age on criminality. If age affects criminality within all of the groups so identified (and available evidence indicates that it does), then it is, to say the least, unusual scientific logic to conclude that it therefore has no effect on criminality. Most textbook discussions of the age distribution in a theoretical context rely on assumed differences for demographic subgroups. Theoretical discussions of the supposed interaction of sex and age on crime are good examples. Textbooks perhaps typically compare "rates of increase" for boys and girls on particular offenses, thus suggesting considerable flexibility in the age distribution by sex. Elaborate explanations of the changing structure of society, or of role anxiety, are then constructed to account for these fluctuations. All of which tends to obscure the basic fact that the age-crime relation does not vary by sex.

> rival theories explain age variation may help us to distinguish among them . . . [and]

> 2) any explanation of age variation in criminality based on psychological reactions to physiological changes accompanying adolescence would be difficult to reconcile with the great variation in delinquency involvement among juveniles as well as the lateness of peak involvement in violence offenses. If age is relevant to criminality, the link should lie primarily in its social significance. [1979, pp. 588–89]

Thus, Greenberg explicitly denies the relevance of nonsocial explanations of the age distribution, relying mainly on variation in criminality within age groups as evidence against such explanations. However, if, as we have previously suggested, it is illegitimate to use age variation against social theories, it is equally illegitimate to use within-age variation against nonsocial theories. We must then disagree with Greenberg on both counts. No rule of logic requires that explanations of one correlate of crime also be explanations of other correlates. If the effect of age on crime does not interact with other effects, explanations of the age effect may not explain the effects of other variables. Once again, then, we see delinquency theorists dressing theoretical arguments as logical and/or empirical arguments, a most distressing habit, since it implies that alternative theories are clearly defective on both logical and empirical grounds when they may be defective on neither.

The robustness of the relation of age and crime—across time, place, and social condition—grants a high degree of plausibility to explanations of crime, such as Greenberg's, that focus on age. Ironically, the very fact that gives such theories their plausibility also falsifies them. A ubiquitous relation falsifies explanations the moment they are advanced, and the ubiquity of the age relation to crime is phenomenal.

4. AGE HAS A DIRECT EFFECT ON CRIME

A direct-effect hypothesis makes sense only in the context of a restricted set of competitive or intervening variables. We would not argue that no mechanism can be found to account for the effects of age; we argue only that no such mechanism is to be found in current criminological research or theory. Since this argument follows directly from what has been said before and is based on data previously discussed, further elaboration seems unnecessary.

5. CONCEPTUALIZATION OF THE AGE EFFECT IS LARGELY REDUNDANT OR MISLEADING

An increasingly prominent focus of contemporary criminology is the career criminal. Statistics to the effect that a small percentage of offenders ac-

count for a large percentage of offenses are routinely cited in support of this concept. Taken at face value, the career criminal notion suggests division of the criminal population into discrete categories: those who offend occasionally or sporadically for a typically brief period of time and those who offend regularly over an extended period of time. If the division proves valid, two general age-crime distributions could be extracted from the data. For the first group, the age distribution might well approximate the now familiar positively skewed, unimodal distribution in evidence throughout this paper. For the second group, the career criminals, several distributions are possible, depending on how such careers proceed, and several such distributions have in fact been postulated. The most common asserts that career length is a function of "age of onset"—the younger the age of onset, the older the age of desistance. "Boys first convicted at the earliest ages tended to become the most persistent offenders as adults" (Farrington 1979, p. 12; see also Shannon 1978; Department of Justice 1981–82). (The underlying model here is that the more serious or persistent the offender, the earlier in life will this fact be evident.) An alternative conception suggests a fixed length to the criminal career, so that those who start early will also finish early. This suggests that all careers are of approximately equal length. This distribution is often associated with the concept of "burnout." Other models are possible within these generic types. For present purposes, the question is whether the age distribution of crime can shed light on this way of looking at the phenomenon.

Let us focus directly on the major elements of career descriptions: "age of onset," "age of desistance," and their derivative, "length of criminal career."

Age of Onset

The age-of-onset terminology appeared early in the criminological literature (see Goring 1913; Wootton 1959, chap. 5). As originally conceived, the concept involved the isolation of a group of offenders and subsequent identification of the age at which their criminality first appeared. The logic for investigating the etiological significance of age on the basis of the age-of-onset notion was borrowed from etiological studies of disease:

> . . . as a subject for independent inquiry, the present age of any sample of criminals appears to have no more direct and special statistical import, in relation to criminological questions, than the present ages of a sample of tubercular subjects would have direct statistical bearing upon problems of tubercular disease. . . . And just as in the one case a solution of this etiological problem depends upon the analysis of statistics relating, not to the ages of subjects during the course of disease, but to their ages at the time of its onset, so, to elucidate the etiological relation of age to crime,

> the statistics we require are not the ages of criminals at any period of their career, but statistics of the age distribution of criminals at the time of their first offense. [Goring 1913, p. 201]

When research is based on a sample of continuing offenders and retrospective procedures, onset notions make sense. However, Goring himself used an alternate procedure to study age of onset. Although this alternate procedure is repeatedly used in contemporary research, it is inconsistent with the logic of the original idea: Goring distinguished habitual from first-time offenders and compared these groups with respect to age of onset. When the research design was altered to avoid exclusive concentration on persistent offenders and retrospective accounts of their "careers," the terminology and logic of onset were retained. But when samples include those who persist in offending and those who do not, age of onset becomes a problematic idea, and the effects of age-of-onset differences are easily confused with differences in rates of offending.

For example, a common mistake in traditional thinking about age of onset has been to confuse the finding that a given group is more likely to produce offenders with assertions about the characteristics of its offenders. Thus, if a larger proportion of blacks than whites commit offenses at an early age, this is read as meaning that blacks begin offending at an earlier age than whites. The question then becomes, Why do blacks start earlier than whites? If blacks start earlier than whites, there is, given career notions, reason to believe that blacks may be more serious offenders than whites and that, at any given age, they will be "further along" in their careers than white offenders. This logic then leads to comparisons of black offenders with white offenders when, in fact, there may be no evidence of differences between the two that require explanation.

Figure 5 shows delinquency rates for white and nonwhite males in the Wolfgang et al. (1972) birth cohort. Note that in these data the higher rate for nonwhites at early ages persists throughout the age range available in the sample, and that there is no difference in the form of the age distributions of delinquency for nonwhites and whites. Note too that higher rates for nonwhites at ages 10–12 may be taken as evidence that nonwhite delinquents "start earlier" than whites. Wolfgang and his colleagues address this question directly:

> Nonwhites generally incur their first police contact at an earlier age than whites. The respective mean ages of onset are 13.4 for nonwhites and 14.3 for whites. Moreover, the percentage of nonwhite youth arrested for the first time is higher than the corresponding percentage of white youth at each age of onset category between 7 and 13 years, whereas a higher proportion of white than nonwhite youth fall into the 14–17 age categories. Thus, up to and including age 13, 48.7 percent of the nonwhite youth, compared to 30.8 percent of the white youth, have their initial police con-

> tact. The trend is reversed between 14 and 17 years. [1972, p. 135; see also
> U.S. Department of Justice (1981–82), which reports similar findings from
> subsequent cohorts studied by Wolfgang]

Figure 9 compares prevalence rates for nonwhites and whites calculated from the Wolfgang et al. data. These rates appear to be perfectly consistent with the age distributions of criminality presented in figure 5. The proportion of nonwhites "beginning" delinquency, like the proportion committing delinquent acts, is greater than the proportion of whites at all age levels. If we calculate a measure of association between race and onset at each age from seven to 17, the results are as follows: .73, .62, .68, .67, .67, .67, .64, .59, .54, .46, .43.[14] These results are consistent with the Wolfgang et al. discussion just quoted. They show that when delinquency is measured by the first offense, something happens to the
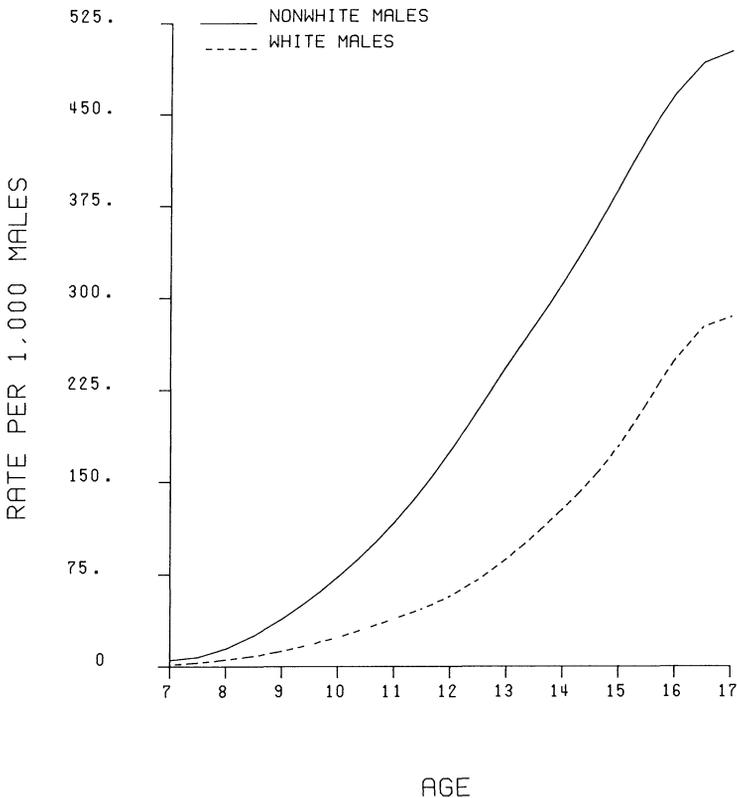

FIG 9 —Prevalence rates by age and race. (Source of data· Wolfgang et al [1972, p. 90].)

[14] The measure of association is NW − W/NW, where NW and W are the nonwhite and white delinquency rates.

race/"delinquency" relation at about age 13. The difference between non-whites and whites begins to decline. Why? As figures 5 and 9 and the measures of association show, nonwhites are progressively more likely than whites to be and to become delinquent throughout the age range covered (at least to age 16). At about age 13, however, the proportion of nonwhites who have already become delinquent becomes sufficiently large that the relative increase in nonwhite first offenders cannot keep pace with the relative increase in white first offenders. For nonwhites to keep pace with whites between ages 13 and 17, about 55% of the entire non-white population (almost three-quarters of those eligible) would have to commit a *first* offense during this period. The resulting difference in relative increase, a logical necessity given differences in rates of delin-quency, produces all of the age-of-onset differences described by Wolfgang and his colleagues. (In fact, at age 12 and before, the mean age of onset for nonwhites and whites is identical!)

The fact is that the longitudinal design with its age-of-onset terminology unduly complicates our explanation of its findings. In (relatively inex-pensive) cross-sectional terminology, this major finding of longitudinal research may be expressed as follows: at all ages, nonwhites have higher rates of crime than whites. At any given age, therefore, nonwhite of-fenders are more likely than white offenders to have committed prior offenses (to be career criminals?). Since the proportion of nonwhite of-fenders with prior offenses approaches the theoretical limit faster than the same proportion for white offenders (because of the higher crime rate for nonwhites), at some point the proportion of "new" offenders among white offenders will be larger than the proportion of "new" offenders among nonwhite offenders. This will suggest to the unwary that whites "start later" than nonwhites. If whites start later than nonwhites, it fol-lows that nonwhites start earlier than whites. But we have already es-tablished that the latter statement is in no meaningful sense true, just as the former statement, being a statistical necessity, is in no theoretical sense useful.

In short, it seems clear that age-of-onset terminology can be highly misleading; it can obscure otherwise straightforward facts and produce in their stead "findings" that defy rational explication. In the present case, it suggests differences between nonwhite and white offenders that are not supported by the data. As with all "career" notions, the age-of-onset question seems to lead to comparisons of offenders across groups, when the proper comparisons include those who do not offend as well.[15]

[15] A similar conclusion has been reached by those studying violence prediction. For example, Monahan argues (1981, p. 111) that "the relevance of race in a person with an extensive record of violence appears minimal or nonexistent. Whatever their race, people with such records have a higher probability of future violent behavior. Such findings lead one to 'emphasize the unimportance of race as a determinant [of future violence] once the individual has been identified as a delinquent' (Hamparian, Schuster, Dinitz, and Conrad, 1978:133) "

Age of Desistance

Implicit in the concept of career is some notion of behavioral stability. In most careers, end points tend to be fixed by convention or by the physical demands of the profession. Since conceptions of the natural length of a criminal career are hard to come by, the approach to this question in criminology has tended to be radically empirical. A career lasts as long as the offender continues to offend. When, then, do offenders stop offending? One way to answer this question is to follow a group of offenders over time, recording their criminal offenses. When a set period of time elapses without an offense having been committed, the offender is said to have abandoned his career. Age at last offense marks the end point of a career in crime. When age of desistance is charted, it turns out that there is considerable variation. What accounts for the fact that some offenders quit earlier than others?

This kind of question is assumed to be of singular significance and to be different from the kinds of questions criminologists uninterested in careers are likely to ask. It therefore requires a peculiar set of independent variables. Historically, the major variable in this set has been age of onset. (Age of onset serves two functions: it defines and it may explain the length of a criminal career. Separation of these two functions is often difficult.) Consider the following: "A central issue for criminal careers research is the identification of factors that discriminate between people who do and people who do not continue criminal activity after their initial police contacts. . . . Studies have found the characteristics of juvenile criminality to be the most reliable predictor of an adult criminal career. Those who engage in serious crime at an early age are the most likely to continue to commit crimes as adults" (Petersilia 1980, pp. 346–47). In the career criminal literature, this empirical relation is easily translated into a pressing theoretical issue: "But what explains an early onset of juvenile crime? Is it peer and family relationships?" (Petersilia 1980, p. 374).

Given what we know about age of onset, what would we predict about its relation to age of desistance? Recall that high-rate groups have high rates at all ages. From this it follows that high-rate groups will have high rates of recidivism (repeated offending). Since repeated offending defines a criminal career, it follows that high-rate groups will have high rates of "career offenders." It follows further that there is nothing of particular etiological significance in the concepts of "age of onset," "age of desistance," and "criminal career"—that there is nothing in these concepts not found in the concept of criminality itself.

A possible empirical objection to our conclusion would be a finding that rates of desistance are a function of rates of initial offending such that those who "start earlier" "quit earlier." Invariant age distributions

preclude this possibility. No group with relatively high rates early and relatively low rates late has been discovered.

6. THE LONGITUDINAL STUDY IS NOT REQUIRED TO STUDY THE CAUSES OF CRIME

Age, career, and associated notions are considered so important by contemporary criminologists that the longitudinal or cohort study is approaching required status. Longitudinal logic is sometimes said to be necessary for adequate theory (Elliott et al. 1979), and longitudinal research is often said to be "superior to cross-sectional if one is primarily interested in drawing causal inferences" (Petersilia 1980, p. 337).

If our position is correct, the emphasis on this research design is unjustified and potentially misleading. We readily grant that the bulk of the evidence against exclusive reliance on longitudinal studies has been provided by longitudinal studies themselves. These studies repeatedly show the value of cross-sectional research and the risks of longitudinal analysis. For example, Shannon (1978) analyzes and compares 1942 and 1949 cohorts in Racine, Wisconsin. The purpose is to determine "which categories of people: are most likely to engage in delinquent behavior, will cease delinquent behavior as they grow older, or will continue into adult criminal activity" (1978, p. 3). We have argued that the first question leaves nothing for the others. Shannon appears to disagree, asserting that "age at first police contact is the best predictor of juvenile delinquency seriousness scores, these scores in turn the best predictor of intermediate seriousness scores, and finally, intermediate scores the best predictor of adult seriousness scores" (1978, p. 5). He also says that historical differences account for an "earlier onset of careers" (1978, p. 7) in the 1949 cohort and that the existence of "a relationship between more frequent and more serious [police] contacts early in life and continuity in careers cannot be denied" (1978, p. 11). Let us examine these statements within our perspective. "Age at first police contact" must mean "police contact before a given age" (otherwise Shannon would be attempting to predict juvenile delinquency seriousness scores for offenders whose first contacts were in adulthood). This statement thus reduces to the assertion that delinquency predicts delinquency. The "earlier onset of careers" in the 1949 cohort simply reflects a higher rate of crime in this cohort (and tells us the source of the idea that delinquents today "start earlier" than they once did). The relationship between frequent and serious police contacts early in life and "continuity in careers" can be similarly explained.

The correlates of "police contact at an early age" identified by Shannon in his longitudinal study (1978, p. 5) are consistent with the correlates of delinquency and criminality repeatedly reported in cross-sectional re-

search, whether or not such research had access to age-of-onset infor-
mation. This is true because "delinquency at an early age" is nothing
more than delinquency, and age does not interact with any known causal
variables in its effect on crime. Therefore, identification of the causes of
crime at one age suffices to identify them at other ages as well, and little
substantive benefit accrues to the longitudinal design for the study of
crime causation.

Life-Course Explanations

Age is correlated with important events thought to be related to crime,
such as leaving school, marriage, and gainful employment, but its effects
on crime do not appear to depend on these events. Age affects crime
whether or not these events occur. We described the data on employment
status earlier. Good research indicates that marriage does not affect de-
linquency either:

> Marriage has often been invoked as the reason for the observed decrease
> in convictions after age 18, and indeed as the most effective treatment for
> delinquency. The Cambridge study found that both official and self-reported
> delinquency decreased between 18 and 21. Men who married during this
> period were compared with those who stayed single, to see if the married
> group decreased more. The groups didn't differ in official or self-reported
> delinquency at age 21, even after attempts were made to match them up
> to the date of the marriage. [Farrington 1979, p. 314; see also Tittle 1980]

Although not designed as direct tests of life-course questions, studies
of crime during military service are, in our view, also consistent with the
argument that life-course change cannot account for the age effect (Glueck
and Glueck 1968, chap. 12). The persistence of the age effect in incar-
cerated populations casts doubt on the assumption that such status changes
as marriage, parenthood, or employment are responsible for decreases in
criminality associated with age. Perhaps more fundamentally, the stability
of the age effect across societies and demographic groups would not be
expected were life-course factors responsible for an "apparent" age effect.

Theories that try to explain the age effect by relying on life-course
events will always sound plausible. Their plausibility stems from the fact
that the age effect is confounded with the effects of its correlates. (For
example, marriage and "settling down" do go together because age pre-
dicts them both.) Age is correlated with beliefs and practices themselves
correlated with crime—for example, respect for authority, punitiveness
toward offenders, church attendance—but we believe that these correlates
are not responsible for the age effect. Although crime-relevant beliefs and
practices indeed vary greatly over the life cycle, the data suggest the

effects of age will be found in all categories of these beliefs and practices. Once again, the plausibility of explanations of the age effect based on such correlates results from the universal tendency to assign the effects of age to its correlates. The statistical difficulties inherent in this tendency are obvious once it is realized that none of these correlates can compete with age in predicting criminality.

Implications

Age is everywhere correlated with crime. Its effects on crime do not depend on other demographic correlates of crime. Therefore it cannot be explained by these correlates and can be explained without reference to them. Indeed, it must be explained without reference to them.

Although correlated with crime, age is not useful in predicting involvement in crime over the life cycle of offenders. For predicting subsequent involvement, to know that a child of 10 has committed a delinquent act is no more useful than to know that a child of 15 has done so. The implications of this fact for contemporary research practice are profound. It denies, for example, the suggestion (at the heart of the longitudinal survey and the career criminal notion) that "prevention and treatment efforts should be concentrated on those boys who begin their criminal careers early in life" (Farrington 1979, p. 301).[16]

Our argument also implies that the traditional division of the etiological problem into juvenile and adult segments is unlikely to be useful. Because

[16] We are grateful to Alfred Blumstein for stubbornly arguing this point until we were at last convinced that he was correct. Research evidence apparently contrary to this position is frequently reported. For example, "based on a preliminary analysis of the subjects in the 1959–1965 cohort who have already reached adult status, the researchers [Wolfgang et al. 1972] find that the age of onset of criminal activity is a key predictor of the offender's subsequent career as an adult. . . . The most active offenders in the Philadelphia sample commit their first offense around the ages of 10 or 11, and the least active commit their first—and generally—only offense at age 17" (U S. Department of Justice 1981–82, p. 11). Let us try once more to clarify this issue. We say age is irrelevant in predicting subsequent criminality. Longitudinal researchers appear to say age is the key predictor They present evidence. What does this evidence look like? Once again, the evidence is a relation between delinquency as measured at various points in time, say t-1, t-2, and t-3. If early onset is crucial, does this mean that t-1 delinquency predicts t-3 delinquency better than does t-2 delinquency? The answer, consistent with common sense and the results of longitudinal research, is obviously no. Is an offender who commits five and only five offenses at age 17 less likely to commit offenses as an adult than an offender who commits five and only five offenses at age 12? Again, the obvious answer is no. What, then, does it mean to say that age of onset is a key predictor of adult criminality? It probably means that *looking back* over the careers of offenders, one finds that those who have committed many offenses over a long period of time have also committed offenses when they were quite young. If so, it is (1) not clear how things could be otherwise and (2) not clear why longitudinal studies are required to look back at the records of offenders. Coupling onset and career notions with longitudinal research clearly suggests the possibility of identifying career offenders at the onset of their careers. As far as we can determine, no longitudinal study to date has identified, or even attempted to identify, the onset of a career in crime at the time of onset.

the causes of crime are likely to be the same at any age, the choice of sample should depend on the complexity of the theoretical argument and the causal analysis it presupposes. Resources should not be devoted to establishing the effects of a variable whose influence on crime is noncontroversial and theoretically uninteresting, especially when, almost by definition, examination of the effects of this variable precludes adequate examination of the effects of theoretically intriguing variables.

Funding agencies seem convinced by researchers that the longitudinal study is necessary for the proper study of crime (see, e.g., National Institute of Law Enforcement and Criminal Justice 1979). The alleged necessity of the longitudinal study is apparently based on a combination of substantive and methodological considerations. The major substantive consideration appears to be the age effect. The methodological considerations derive from the experimental model, because of which it is claimed that the longitudinal design is unique in its ability to resolve the question of causation. Our critique here focuses on the substantive justification for the longitudinal study. At the same time, however, we are not convinced that the longitudinal study offers solutions to causal questions commensurate with its costs. As we have shown, the conceptual apparatus generated by longitudinal thinking has been very misleading. This design has been oversold to criminology at high substantive and economic costs.

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