


Fall 1960

## Ten Years of Unraveling Juvenile Delinquency

Sheldon Glueck

Follow this and additional works at: <https://scholarlycommons.law.northwestern.edu/jclc>

 Part of the [Criminal Law Commons](#), [Criminology Commons](#), and the [Criminology and Criminal Justice Commons](#)

---

### Recommended Citation

Sheldon Glueck, Ten Years of Unraveling Juvenile Delinquency, 51 J. Crim. L. Criminology & Police Sci. 283 (1960-1961)

This Article is brought to you for free and open access by Northwestern University School of Law Scholarly Commons. It has been accepted for inclusion in Journal of Criminal Law and Criminology by an authorized editor of Northwestern University School of Law Scholarly Commons.

The Journal of  
**CRIMINAL LAW, CRIMINOLOGY AND POLICE SCIENCE**

VOL. 51

SEPTEMBER-OCTOBER 1960

NO. 3

**TEN YEARS OF UNRAVELING JUVENILE DELINQUENCY**

**An Examination of Criticisms**

SHELDON GLUECK

The author is Roscoe Pound Professor of Law in Harvard University. Both Professor Glueck and his wife, Dr. Eleanor T. Glueck, who is a Research Associate in Criminology at Harvard Law School, have contributed frequently to this JOURNAL over a period of many years.

UNRAVELING JUVENILE DELINQUENCY, a major work by Professor and Mrs. Glueck, was published ten years ago. The product of eight years' work with the assistance of a sizeable staff, the book presented findings and analyses based upon a research project involving about a thousand boys living in underprivileged areas of Greater Boston. UNRAVELING attracted a great deal of attention both here and abroad and provoked a large number of reviews and articles, some of which were highly critical. In the present article, Professor Glueck not only answers the critics of UNRAVELING but also presents significant post-publication data concerning various validation experiments relating to the Gluecks' delinquency prediction tables.

The author prepared this article at the special request of the Board of Editors in commemoration of the Journal's fifty years of publication.—EDITOR.

Ten years have elapsed since the publication of UNRAVELING JUVENILE DELINQUENCY<sup>1</sup> by Dr. Eleanor T. Glueck and the writer. The work on that book spread over eight years, and to accomplish the project we had to call upon the aid of a staff of social investigators, a psychiatrist-physician, two physical anthropologists, six psychologists, two Rorschach test analysts, a statistical consultant, two statisticians, and several secretaries.

The book attracted a great deal of attention, both here and abroad. Shortly after publication of UNRAVELING, the Supreme Court of Japan ordered its translation into Japanese; thereafter, a distinguished board of representatives of the several disciplines involved completed the translation, 3,000 copies of which were distributed to judges, prosecutors, probation officers, and others in Japan. A new edition of the Japanese translation is now in process. Parts of the book have also been translated into Hebrew, and a Spanish translation is almost completed. The more popular work founded on UNRAVELING, DELINQUENTS IN THE MAKING,<sup>2</sup> has been rendered into French and

Italian. Spanish and Japanese versions are in process; a German one is contemplated.

The book has greatly influenced research here and abroad and is well known to students of criminology, psychiatrists, anthropologists, juvenile court judges, and others.

We are grateful to all who wrote reviews and articles about UNRAVELING. It should be realized that it was a pioneering effort. We, as well as our critics, can profit from its shortcomings—a happy consequence in the tradition of the history of science.

UNRAVELING has been honored by three symposia of reviews: one in the Harvard Law Review,<sup>3</sup> a second in the Journal of Criminal Law, Criminology, and Police Science, a third in Federal Probation.<sup>4</sup> It has been very widely reviewed in other journals both here and abroad. It was the subject of two extremely critical articles in the American Journal of Sociology.<sup>5</sup> On the other hand, most of the reviews have been favorable.

<sup>3</sup> A Symposium on *Unraveling Juvenile Delinquency*, 64 HARV. L. REV. 1022-41 (1951).

<sup>4</sup> *Unraveling Juvenile Delinquency—A Symposium of Reviews*, 41 J. CRIM. L., C. & P. S. 732-59 (1951); 15 FED. PROB. 52-58 (No. 1 1951).

<sup>5</sup> Rubin, *Unraveling Juvenile Delinquency. I. Illusions in a Research Project Using Matched Pairs*, 57 AM. J. SOC. 107-14 (1951); Reiss, *Unraveling Juvenile Delinquency. II. An Appraisal of the Research Methods*, *id.* at 115-20.

<sup>1</sup> GLUECK, S. & E. T., UNRAVELING JUVENILE DELINQUENCY (Foreword by E. N. Griswold) (New York, The Commonwealth Fund, Cambridge, Harvard University Press, 1950, Third Printing, 1957).

<sup>2</sup> GLUECK, S. & E. T., DELINQUENTS IN THE MAKING (New York, Harper and Bros., 1952).

The most confident and severest critics have been a group whose writings have the tone of fire-breathing chevaliers eager to do battle for that purest queen of the exact sciences, Sociology, to which the authors of *UNRAVELING* allegedly did not pay adequate tribute.<sup>6</sup>

We have waited for most of the reviews to appear before attempting a reply to the principal points raised. These may be summarized under the heads of (I) The Sample, (II) The Techniques of Investigation, (III) Causative Analysis, (IV) The Role of Culture, and (V) The Prediction Method.

## I. THE SAMPLE

A. *Areas from which the sample of delinquents and that of nondelinquents were drawn.* The simplest way to attack any piece of research involving a complex biosocial problem is of course to deny at once the "typicality of the sample." Now it is elementary that *any* sample, especially one involving a social problem, is in some respects atypical of various factors and forces that might be relevant to the issues sought to be illuminated by the inquiry. Unless one included all the juvenile delinquents and nondelinquents in the world and their detailed makeup and background (not to stress the history of the universe), he would, by such facile criticism, be wasting his time. It is particularly difficult to obtain absolute similarity in respect to the myriads of factors and forces in any area from which delinquents and nondelinquents may be drawn.

Some critics fail to fulfill the burden of proving in exactly what respects the samples compared in *UNRAVELING* are atypical. It would seem that they

<sup>6</sup> "... there are occasions on which sociologists can unite—and this exemplifies the sociological principle that attacks from an external enemy tend to unite an in-group that is at conflict within itself." Hartung, *A Critique of the Sociological Approach to Crime and Correction*, 23 *LAW & CONTEMP. PROB.* 704 (1958). (They will no doubt unite to attack the present article.) "Edwin H. Sutherland may be said to have entered the lists against *Unraveling Juvenile Delinquency* in advance." "One of the great values of (Terence) Morris's book is his lengthy historical chapter on research during the nineteenth century, which helps to establish the legitimate claim of sociology to the field of criminology." Hartung, *id.* at 707, 726. In addition, Hartung several times quotes approvingly expressions of great regret by Sutherland at the way nonsociologic criminologists have been able to get research funds from foundations, even quoting the word "seduced." For the record, let it be said that the Gluecks have always had to work hard to obtain subsidization of their researches and they have never used seduction, deception or highway robbery to obtain this hard-won financial aid.

should wait until at least one study similar to ours—employing the same definitions and verifying the raw data with equal care—was done; then there would be a much more substantial basis for judgment of typicality of the samples than the mere *ipse dixit* of the critics.

In the meantime, let us examine the more specific criticisms advanced with reference to the samples compared in *UNRAVELING*:

It has been urged that the *areas* from which the delinquents were selected were not absolutely similar to the areas from which the nondelinquents were chosen. In taking both the delinquents and the nondelinquents from economically and culturally deprived regions, our aim was to "control" major underprivileged area influences and thus set the stage for more detailed comparison of the two groups in respect to a great many more specific traits and factors—anthropologic, medical (health), psychiatric (temperament), personality-character structure (Rorschach test), and various social-psychologic factors of what we call the *under-the-roof culture* of the home and school, as well as certain influences and conditions in the boys' neighborhoods.

Professor E. N. Burgess supports his claim that the regions from which the delinquent and nondelinquent boys came were not similar by emphasizing *Table VIII-1- Type of Neighborhood in Which Boy Lives* (*UNRAVELING*, p. 79).<sup>7</sup> In that table we present a breakdown of the neighborhoods in terms, largely, of *Blighted slum tenement area (deteriorating socially and economically)* and *Interstitial area (merging of business and residential)*. The table shows that 55 per cent of the delinquents' families lived in the first-mentioned type of neighborhood, while 34 per cent of the nondelinquents' did; and 31 per cent of the former lived in the second type of neighborhood, while 49 per cent of the latter did.

However, this is but one type of subclassification of the neighborhoods. Professor Burgess unfortunately fails to call attention to the other, more significant, comparisons in the book, those of a nature much more relevant to the central aim of the control of the general culture for our expressed purpose of comparing delinquents and nondelinquents in respect to more specific characters. For example, he fails to mention *Table IV-2* (p. 36) in which the *delinquency rates* of the two sets of areas

<sup>7</sup> Burgess, *Symposium on the Gluecks' Latest Research*, 15 *FED. PROB.* 52-53 (No. 1 1951).

compared are demonstrated to be remarkably similar. Nor does he allude to *Table IV-3* (p. 36), presenting the results of a very careful field appraisal by trained researchers, who know the regions thoroughly, in terms of defined "good," "fair," and "poor" neighborhoods (from the points of view of the presence or absence of vice, crime, gangs, etc., on the one hand, and the presence or absence of reasonably convenient facilities for wholesome recreation, on the other). This expert assessment, like the similarity of the delinquency rates, demonstrates that the crucial neighborhood influences existing around the homes of our delinquents were very similar to those of the non-delinquents.

Moreover, while for certain theoretical sociologic purposes it may be helpful to classify regions into "blighted slum areas" or "interstitial areas," etc., the sociological area-criminologist himself does not make this the significant distinction when he gets down to the study of delinquency in the field. He first defines his areas in terms of the *rates of delinquency* (as we did), then he compares the incidence of certain *economic and social factors* existing in areas or zones of different delinquency rates (as we did). He analyzes the situation in low income areas as opposed to high income areas<sup>8</sup> and relates the other factors to that distinction rather than to whether he is dealing with a blighted or interstitial area. The well-known works of sociologist Clifford Shaw and his associates, for example, do not differentiate between areas on the latter basis for the purpose of ultimate analysis of delinquency causation. Their familiar spot-maps and circular zonal maps cut across regions part of which may be termed either "blighted slum areas" or "interstitial areas," or both. Shaw and McKay emphasize economic status as one of the most generative and pervasive of the conditions under which "the conventional forces in the community become so weakened as to tolerate the development of a conflicting system of criminal values."<sup>9</sup> They say:

"It may be observed, in the first instance, that the variations in rates of officially recorded delinquents in communities of the city correspond very closely with variations in economic status. The communities with the highest rates of delinquents are occupied by those segments of the population whose position is most dis-

advantageous in relation to the distribution of economic, social, and cultural values. Of all the communities in the city, these have the fewest facilities for acquiring the economic goods indicative of status and success in our conventional culture."<sup>10</sup>

Applying such a relevant yardstick, then, as economico-cultural status, our nondelinquents have unquestionably been chosen from areas relatively similar to those of our delinquents, so like in fact that not a few delinquents lived within the same city block or two as the nondelinquents and sometimes even in the very same tenement house.

It happens that Shaw and McKay had plotted *delinquency rates* in Boston shortly before we began our research for UNRAVELING, and their geographic distribution of various delinquency rates is very similar to our own. One of their conclusions was: "Considering the area [of Boston] as a whole, heavy concentrations are noted in old Boston, especially in the areas north and northeast of the Common, known locally as the West End and the North End; and in Charlestown, East Boston, and South Boston. Only slightly less concentrated are the clusters in Roxbury."<sup>11</sup> Both our nondelinquent group and our delinquent group came very largely from these same regions and, in very similar proportions, from the same census tracts. (See UNRAVELING, *Table IV-2* at 36).

As we pointed out in UNRAVELING,

"Since it was our purpose to draw the boys only from neighborhoods in which the environmental conditions were deleterious to the wholesome development of youth, the fact that fewer delinquents than nondelinquents . . . were found to be living in so-called interstitial areas (merging of business or industrial and residential elements) and a larger proportion in blighted tenement areas has no significance. What is of importance for our purposes is the extent to which the general neighborhood environments were alike in both groups."<sup>12</sup>

It is amusing to find certain sociologic criminologists insisting upon the ultra-precision of the physical and chemical laboratory in this type of comparison.<sup>13</sup> We should be grateful to have our atten-

<sup>10</sup> *Id.* at 437-438.

<sup>11</sup> *Id.* at 222-223.

<sup>12</sup> UNRAVELING at 35.

<sup>8</sup> See, for example, SHAW & MCKAY, *JUVENILE DELINQUENCY AND URBAN AREAS* 435 *et seq.* (Chicago, University of Chicago Press).

<sup>9</sup> *Id.* at 437.

<sup>13</sup> Taft says: "Second, the delinquents and non-delinquents were paired in the neighborhood by physical proximity only. Because of this neglect of social influences in the neighborhood, it is not possible to accept the Gluecks' claim that the two groups in the

tion directed to even one study by them which achieves such absolute and detailed identity in a matching of two culture-areas. Certainly the "proof" of the favorite sociologic theory of "differential association" is notoriously imprecise.<sup>14</sup> One cannot help being reminded in this connection of poor little Brother Juniper's burning belief (in Thornton Wilder's delightful novel, *THE BRIDGE OF SAN LUIS REY*): "It seemed to Brother Juniper that it was high time for theology to take its place among the exact sciences, and he had long intended putting it there."

In this connection I should like also to advert to a criticism by a reviewer who confuses our aim to control the *general* neighborhood-cultural complex of influences for statistical purposes with the finding of differences in various detailed sociologic factors which could only have been, and which were in fact, brought out by our factorial comparisons of the delinquents with the nondelinquents. This critic seems to imply that we were engaged in some deep-dyed plot to banish sociologic influences.<sup>15</sup> This is sheer nonsense. Just as we

study were actually paired by identical social influences of the neighborhood." Taft, *Implication of the Glueck Methodology for Criminal Research*, 42 J. CRIM. L., C. & P. S. 300 (1951). It is perfectly obvious that absolute identity of the myriads of influences involved in neighborhoods is impossible, nor did the Gluecks claim that. Reiss's view that "the study largely ignored the influence of primary groups in guiding behavior and in enforcing conformity to sets of norms," the familiar sociological formula in familiar sociological jargon, is a convenient attempt to explain away some of the disturbing findings of *UNRAVELING*; but nobody has defined these supposed narrow-area and extremely subtle differences in the locales from which our two groups of boys were taken; nor has any critic shown exactly how these minute differences could have been determined and how they would affect the outcome. Besides, the argument is quite irrelevant because we found that the great majority of our boys (90%) were already delinquent at under 11 years of age, long before the alleged influence of "primary groups" in supposedly "enforcing conformity to sets of norms" of a delinquent type could make itself felt. A similar comment can be made with respect to the more penetrating suggestion put forth by Gault to the effect that areas may differ in terms of the leaders of a community. Gault, in *Unraveling Juvenile Delinquency—A Symposium of Reviews*, 41 J. CRIM. L., C. & P. S. at 734-35 (1951). The method used in selecting the neighborhoods is discussed in *UNRAVELING* at 30-31.

<sup>14</sup> See Glueck, S., *Theory and Fact in Criminology*, 7 BRIT. J. DELINQ. 92-109 (1956).

<sup>15</sup> "In place of a study which sought steadfastly to eliminate environmental factors as well as to eliminate them from a causal law, the force of social (or environmental) causation of delinquency proves irrepressible." Rubin, *op. cit. supra* note 5 at 107, 113. Hartung repeats this misconception: "First, although the Gluecks sought steadfastly to eliminate environmental influences, the great influence of social causation proved to

did our utmost to match each delinquent with a nondelinquent for age, I.Q., and ethnic derivation, so did we control the general cultural area situation in respect to delinquents and nondelinquents as a basis for the subsequent detailed comparison of the incidence of the psychologic and socio-cultural traits and factors in the home, school, and neighborhood.<sup>16</sup> As to many of these characters, statistical comparison proved the status of the delinquents to be significantly different from that of the nondelinquents. The differences were very clearly brought out, taking full account of the possible influence of chance and they established factually the point we made in the introductory chapter of *UNRAVELING* (p. 5) with respect to the difference between the typical sociologic "area" or "subculture" study of delinquency and our own eclectic approach—a point which forms a major aspect of our conception of etiology:

"The area-studies establish that a region of economic and cultural disorganization tends to have a criminogenic effect on people residing therein; but the studies fail to emphasize that this influence affects only a selected group comprising a relatively small proportion of all the residents. They do not reveal why the deleterious influences of even the most extreme delinquency area fail to turn the great majority

be irrepressible in their data." Hartung, *op. cit. supra* note 6 at 706. The Gluecks neither "sought steadfastly to eliminate environmental factors" from the research nor to "eliminate them from a causal law"; nor does "the force of social (or environmental) causation of delinquency prove irrepressible" from analysis of our materials, which establish a *multicausal* influence neither exclusively social nor exclusively biologic.

<sup>16</sup> "Socio-cultural level. A primary approach to the analysis of crime causation is the exploration of the forces and circumstances of the family background and the physical and social environment of the home; for character and conduct may be said to consist of the result of the interplay between innate constitution and environmental forces. The selection of the delinquents and the nondelinquents from similar neighborhoods of underprivilege carried with it, of course, no necessary assumption that poverty and its correlates or the cultural forces of urban delinquency areas play no role in the causation of delinquency. However, just as the matching of delinquents with nondelinquents on the basis of general intelligence permits us to examine the constituents and patternings of intelligence, so matching by residence in underprivileged neighborhoods makes it possible to control the general factor of area culture and to determine whether subtler differences exist in respect to the more intimate socio-cultural factors of home and school and the attitudes and practices of parents, teachers, and companions as bearers of the home and school culture." *UNRAVELING* at 16.

of its boys into persistent delinquents. They do not disclose whether the children who do not succumb to the evil and disruptive neighborhood influence differ from those who become delinquents and, if so, in what respects. Until they take this factor into account, they cannot penetratingly describe even the culture of the delinquency area. For to say that certain bacteria have a fatal effect on some individuals but no such effect on the majority, without describing or explaining the differential influences, is to describe that infective agent inadequately."

How do some sociologic critics answer this? By the following *reductio ad absurdum*: "First, it is generally recognized that there is a gap of unknown size between 'actual' and 'known' delinquency. Some students think that *few if any adolescent boys in high-rate areas escape delinquency*."<sup>17</sup> It will be seen below that, where convenient, these critics assume that only 10 per cent or less of slum area boys are delinquent (that is, those supposedly subjected to "differential association" with delinquent persons or patterns); but when necessary to mount what they conceive to be a really devastating attack on the kind of basic argument presented in the quotation above, they conclude that *all* boys in high delinquency areas are delinquent!

B. *The sample of the delinquents (and of the nondelinquents) is atypical, because: It did not include all ethnic groups.* Our samples dealt largely with matched representatives of such ethnico-racial derivation as Italian, Irish, English, and Slavic. (See Table IV-6 p. 38, and Appendix B of UNRAVELING). The results have been criticized in some quarters because the samples did not include representatives of other ethnic groups and because the number of Jews was small. The reason for our sample not including more Jewish boys is the very simple one that it was exceedingly difficult to find enough Jewish delinquent boys in the areas in question. As for the other complaint, our samples do not include Chinese, West Indians, Hottentots, Eskimos, etc., although there may well be a few boys descended from these ethnic groups in Boston. If the objection be made that then we had no right

to "generalize" about a "tentative causal law" of delinquency, we invite the reader's attention to Chapter II (especially p. 15) and Chapter XXI (especially p. 272) of UNRAVELING, where we fully recognize the limitations of our conclusions arising out of the necessity of controlling, at the outset, certain generalized sets of variables (including ethnic derivation). The extent to which our findings will ultimately prove to hold good for other samples of cases (ethnic, economic, etc.) can only await testing through further research. But, as will be shown below, our social prediction table based on certain sociocultural factors of family life has proved to have very high prognostic force when applied to samples of Jewish, Negro, French and Japanese delinquents.

C. *The sample of delinquents is atypical because: The boys selected were largely those who had been committed to an industrial school and who were, thereby, presumably "institutionalized."* On pages 13-14 of UNRAVELING we give our definition of delinquency for the purposes of the research in question. We have spent a great many years in the investigation and study of delinquents and criminals and on the basis of that experience we point out (UNRAVELING p. 13) that children who, once or twice during the period of growing up in an exciting milieu, steal a toy in a ten-cent store, sneak into a subway or motion picture theatre, play hooky, and the like, but *soon outgrow* such peccadilloes, are not true delinquents even though technically they may have violated the law.<sup>18</sup> We state that "in order to arrive at the clearest differentiation of disease and health, comparison must be made between the unquestionably pathologic and the normal."

Certainly, if a laboratory technician is studying cancer, he must first make sure he is dealing with a malignant and not a benign growth. It is for that good reason that we decided to include in our sample only boys whose misbehavior could be said to be truly delinquent in that it was persistent, rather than either accidental or very occasional and followed by early abandonment of misconduct.

<sup>17</sup> Hartung, *op. cit. supra* note 6 at 728. The specific reference is to Hartung's reply to criticisms of A. Cohen's theory of a delinquency subculture and hypothesis that "middle-class" male delinquency is "a consequence of the middle-class adolescent boy's anxiety concerning his masculine role." But the quotation in the text would apply equally (and I have several times heard the argument seriously advanced) to the specific point made in the text.

<sup>18</sup> "Indeed, it is now recognized that a certain amount of petty pilfering occurs among many children around the age of six or seven and is to be expected as part of the process of trying their wings. Children appear to be no worse for very occasional and slight experimental deviations from socially acceptable norms of conduct. Since they voluntarily abandon such behavior, their misconduct or maladaptation cannot be deemed either habitual or symptomatic of deep-rooted causes." UNRAVELING at 14.

And it is precisely for this reason that we have grounds to believe that our prediction tables, designed to "spot" probable future delinquents at school entrance, will be able to differentiate *true* prospective delinquents showing difficulties of adjustment in the early years from pseudodelinquents who display *similar external behavior* but whose future is in fact more promising.

One severe critic makes a great to-do about the "institutionalization" of our delinquents as invalidating the findings with reference to delinquents in general, findings he nonetheless is quick to accept in another part of his critique as proof of his claim that, despite our supposed ruling out of sociocultural influences, "the force of social (or environmental) causation proves irrepressible." He produces no proof for his contention about "institutionalization."<sup>19</sup>

Here are the relevant facts:

Our boys spent, on the average, 7.12 months in correctional institutions, 61.8 per cent having been there less than six months. (UNRAVELING, Table A-10, p. 296). By contrast, they had had an average of 10.84 months on probation, over half of them six or more months. (UNRAVELING, Table A-9, p. 296). The institutions to which they were committed are open industrial schools with a regime of education, athletics, recreation, religious guidance, etc. No lockstep and bars are involved. Surely nobody with even an elementary first-hand knowledge of psychology or the conditioning of delinquent attitudes and behavior could seriously claim that the brief stay in such an institution would crucially overbalance all the other experiences of a young lifetime gained in the home, school, and community! We solemnly assure this critic that our delinquent lads did not have the "prison pallor," did not talk out of the corner of the mouth, did not glance apprehensively over the shoulder.<sup>20</sup>

<sup>19</sup> Rubin, *op. cit. supra* note 5 at 108-109. "Two outstanding facts emerge from *Unraveling Juvenile Delinquency*: . . . (2) Institutionalized children differ from children who have not been institutionalized." *Id.* at 113. A similar criticism, made in more temperate terms, is that of P. P. Lejins in the Symposium in 15 FED. PROB. at 58 (No. 1 1951).

<sup>20</sup> Rubin pontificates: "The most elementary caution in criminological research is the recognition that an examination of institutionalized offenders (or delinquents) will provide information about *institutionalized offenders* and *not* about offenders in general. An institutionalized offender is characteristically, in great part, an institution product." Rubin, *op. cit. supra* note 5 at 108-109; Symposium in 15 FED. PROB. at 55 (No. 1 1951). Rubin confuses an institutionalized

Besides, the vast part of our data concerns the personal makeup and early conditioning factors operative *years before* these boys were "institutionalized."

To the extent that relevant factors exist for comparison, we have found little difference between the general run of delinquents who appear in a juvenile court and our sample. Strong inferential proof of this fact, by neutral investigators, is to be seen in a study significantly entitled, *The Close of Another Chapter in Criminology*, by Doctors William Healy, Augusta E. Bronner, and Myra E. Shimberg.<sup>21</sup> Another sample of juvenile delinquents had been used by us in an investigation prior to UNRAVELING, and that research involved a follow-up of delinquents who had passed through both the Boston Juvenile Court and the Judge Baker Guidance Center (Clinic). The findings of recidivism reported in that investigation, ONE THOUSAND JUVENILE DELINQUENTS,<sup>22</sup> had been questioned on the ground that, since the cases were those that had been sent by the judge to the clinic, they must have been the most *serious* ones and this must have accounted for the high failure rate we discovered in tracing these boys during a five-year follow-up period after court appearance. On the basis of a check group of a thousand *other* cases—the *general run of the mill* of the same court (*i.e.*, those who "passed through the . . . court, but were not referred . . . to the" clinic)—the authors of the above-named check study (two of them at that time directors of the clinic in question) concluded, after their own careful follow-up of these cases, that "the results of the two studies [the Gluecks and theirs] are amply corroborative."<sup>23</sup>

It is further significant on the issue of sampling, a point worthy of repetition, that many prediction-validation studies (see below) on samples of boys of a variety of ethnic derivation, religion, culture, etc., are also "amply corroborative" of major findings in UNRAVELING.

---

delinquent with a delinquent who happens to have been sent for a brief time to a correctional school. Besides, 80% of the boys were 12 years or older by the time of their first commitment to an institution, and their basic character structure was by then quite firmly fixed. UNRAVELING, Table A-5 at 294.

<sup>21</sup> Healy, Bronner & Shimberg, *The Close of Another Chapter in Criminology*, 19 MENTAL HYGIENE 208 (1935).

<sup>22</sup> GLUECK, S. & E. T., ONE THOUSAND JUVENILE DELINQUENTS (Introduction by Felix Frankfurter) (Cambridge, Harvard University Press, 1934).

<sup>23</sup> Healy, *et al.*, *op. cit. supra* note 21 at 211.

## II. THE TECHNIQUES OF INVESTIGATION AND TESTING

A. *Interviewing and investigating.* Some critics claim that the facts we were able to obtain through interviewing the parents of nondelinquents are very probably not similar to those we could obtain through interviewing those of delinquents, on the ground that the latter are under compulsion to give information, the former not.<sup>24</sup> While this view is understandable, we believe these critics to be mistaken. Neither set of families was under compulsion to give us information. We are not officials, and our field investigators made their special research interest and nonofficial status very clear to the parents. There is a technique, carefully developed over many years of experience with this type of research, that enables us to obtain data both intensive and verified; the method is described in the first chapters of UNRAVELING and in an unpublished (mimeographed) account of the building up of a typical case history by our two chief investigators.<sup>25</sup> Long experience in investigating families, the scrupulous care with which parents and relatives were interviewed, and, especially, the intensive search for verifying data through *pre-existing records* (in private social agencies and public offices, and made by others for purposes entirely different from those of our research) render it most unlikely that the intensity and range of the investigations were significantly different in the case of nondelinquents from that of delinquents. It must further be borne in mind in this connection that the same syllabus of data to be gathered and verified, and the same statistical schedules, were used for nondelinquents as for delinquents.

### B. *Reliability of tests and measurements.*

(1) *The method of anthropometry and the Sheldonian technique of somatotyping are supposedly not reliable.* One critic<sup>26</sup> first admits he is no specialist in anthropology, then presumes to question the

<sup>24</sup> Taft, *Implications of the Glueck Methodology for Criminological Research*, 42 J. CRIM. L., C. & P. S. 305-306 (1951). See also, Gault and Anderson, *Unraveling Juvenile Delinquency, A Symposium of Reviews*, 41 J. CRIM. L., C. & P. S. at 734, 746 (1951).

<sup>25</sup> McGRATH & CUNNINGHAM, *THE CASE OF HENRY W., ILLUSTRATING METHOD OF SOCIAL INVESTIGATION*. Copies of this mimeographed statement, too bulky to have been included as an appendix to UNRAVELING, as originally planned, have been distributed to principal libraries.

<sup>26</sup> Taft, *op. cit.* *supra* note 24 at 303 n. 1.

validity of the anthropometric and somatotyping work performed by the two highly experienced physical anthropologists who did the measuring and morphologic classification of types for UNRAVELING. Another critic, also not an anthropologist, attacks the anthropological work in reviewing *PHYSIQUE AND DELINQUENCY*.<sup>27</sup> Why? Because the late Professor Edwin H. Sutherland, whom both critics admire greatly, "with his wonted thoroughness, gave the Sheldon contribution to criminology a terrific going over." However, Sutherland, also, was no anthropologist. Moreover, the "going over" referred to a book by William Sheldon in the field of criminology, which had nothing whatsoever to do with our own work. We ourselves had criticized Sheldon's book, *VARIETIES OF DELINQUENT BEHAVIOR*,<sup>28</sup> because, although the somatotyping is excellent, the sociologic data and interpretations are, in our opinion, questionable. But this is irrelevant to the employment of (and the basic improvement upon) Sheldon's categorization of somatotypes (in turn an improvement over Kretschmer's) in our study. We are convinced that the anthropometry and analysis of the somatotype data in UNRAVELING are exceptionally meritorious. A distinguished authority in the field, who is a trained anthropologist, Professor C. Wesley Dupertuis, is lavish in his praise of this work.<sup>29</sup>

Another critic attacks the *statistical* basis of *PHYSIQUE AND DELINQUENCY*, which employed the somatotype findings of UNRAVELING in intercorrelating morphologic types with numerous traits and factors. He is concerned with the fact, for example, that careful anthropometry, by experts, showed that 60.1 per cent of the delinquent group compared to 30.7 per cent of the nondelinquents are *mesomorphic* (with "relative predominance of muscle, bone and connective tissue") and, at the other extreme, 14.4 per cent of the delinquents compared to 39.6 per cent of the controls were found to be *ectomorphic* (with relative predominance of linearity and fragility and, in proportion to their mass, with "the greatest surface area and hence relatively the greatest sensory exposure to the outside world"). (UNRAVELING, pp. 193,

<sup>27</sup> Hartung, Book Review, 48 J. CRIM. L., C. & P. S. 638 (1958), reviewing GLUECK, S. & E. T., *PHYSIQUE AND DELINQUENCY* (1956).

<sup>28</sup> SHELDON, *VARIETIES OF DELINQUENT BEHAVIOR* (Harper & Bros., 1950). Our review appears in 86 SURVEY 215 (1950).

<sup>29</sup> Dupertuis, *Physical Anthropology*, 64 HARV. L. REV. 1031 (1951).



344, 54.) This critic, after correctly stating that we presented 109 tables "in an effort to ascertain which traits and factors supposedly exert a significant differential influence on the delinquency of four body types," incorrectly concludes that the statistical treatment is questionable. The argument is that "There are at least forty-six tables in which the number of delinquents, or controls, or both is less than 100," and that "None of the 109 tables deals with the entire sample of delinquents and nondelinquents."<sup>30</sup>

This is a fallacious argument. Of course none of the 500 delinquents and 500 nondelinquents had *all* the traits and factors embraced in UNRAVELING.<sup>31</sup> To have made that possible it would have been necessary to match myriads of thousands of cases in respect to over 400 items included in the study. But apart from this, the purpose of the comparisons in both UNRAVELING and PHYSIQUE AND DELINQUENCY was to determine the traits and factors in respect to which the incidence significantly differed between the delinquents and the controls, *in the cases where those traits and factors did exist*. If, as was true of many characters, the incidence was found to vary significantly among the two groups compared, this certainly did not indicate that, because a particular trait or factor was not present in *all* 500 cases of the delinquents and in *all* 500 of the control group, it should be eliminated.

When, for example, it is found that, in relation to all 12 socio-cultural factors of the home contributing selectively to delinquency, mesomorphs had the highest incidence in comparison with the three remaining body types but especially in contrast to ectomorphs, and this response is exactly what one would expect of the solid bone-muscle mesomorphic type as opposed to the fragile, sensitive ectomorphic type of person (PHYSIQUE AND DELINQUENCY, 240), are we to be told that because the factors in question did not exist in all 500 delinquents and all 500 nondelinquents the result should be ignored or is due to pure chance?

In all this it should be borne in mind that where necessary we have consulted statistical authorities.<sup>32</sup>

<sup>30</sup> Hartung, *op. cit. supra* note 6 at 705.

<sup>31</sup> "... it is rare indeed in the social, as in the meteorological field, that we can find an attribute or small group of attributes invariably associated in the past with the occurrence or non-occurrence of the phenomenon whose future behavior we wish to predict." Wilson, *Prediction*, 64 HARV. L. REV. 1040 (1951).

<sup>32</sup> It should be mentioned that, unlike the critics, our

And as to the anthropologic soundness of PHYSIQUE AND DELINQUENCY, which is based on UNRAVELING, not only the anthropologist Dupertuis but also one of the world's leading biologists, Julian Huxley, made this judgment: "This is an interesting and indeed important book... as demonstrating beyond any doubt the importance of specific and readily detectable genetic factors determining psycho-physical type, in predisposing boys to delinquency."<sup>33</sup>

Under the circumstances, can we be blamed for preferring the judgment of those with expertise in the relevant fields to that of those sociologists who are neither recognized anthropologists nor statisticians?<sup>34</sup>

In fairness it should be pointed out that not all sociologists have gone off the deep end in an attack on somatotyping. For example, Professor Albert Morris, a sociologist and cultural anthropologist who is both well read and experienced in criminologic research, has said, after setting forth a perceptive appraisal of the difficulties involved in the types of research we are doing:

"These difficulties, usual in research in the behavioral sciences, have been understood and intelligently met. The techniques for doing this have elsewhere been clearly discussed. The result, of course, falls short of perfection; but, perhaps, only those who love certainty more than truth will be misled by unawareness that

statistical consultants, Professors Edwin Bidwell Wilson, Jane Worcester, Carl R. Doering, and Mrs. Rose Knezek, with whom it has been our practice to discuss statistical problems from time to time, *are* highly experienced in statistical theory and technique. See "Explanation of the Statistical Method," by Carl R. Doering, Professor of Biostatistics, Harvard University, in UNRAVELING at 75-76 and "Explanation of Statistical Method," by Jane Worcester in PHYSIQUE AND DELINQUENCY.

<sup>33</sup> Huxley, Review of PHYSIQUE AND DELINQUENCY, 3 INT'L J. SOCIAL PSYCHIATRY 71, 72 (1957). A recent research in England confirms the presence of excessive mesomorphy among delinquents in a Borstal institution. See GIBBONS, NEW FORMS OF JUVENILE DELINQUENCY 13, 50 (World Health Organization, mimeographed, June 6, 1960).

<sup>34</sup> TAFT refers to the fact that when he sent the authors of UNRAVELING a draft of his proposed review, "Professor Glueck properly chides me with failure to explain the basis of my questioning the validity of these methods and failure to name the critics"; yet Taft adds: "I leave the brief comment unchanged, because as indicated I am no specialist in these matters." Taft, *op. cit. supra* note 24 at 303 n.1. It would seem that this fact is a pretty good reason why Taft (and Hartung and the other nonexperts in the fields involved) should *not* have dogmatized on the anthropologic and statistical aspects of UNRAVELING.

the statistician has been given only the opportunity to develop precise summaries of good approximation. . . . They [the authors] wisely recognize that the soundness of their effort is best judged by the clinical 'good sense' and the internal consistency evidenced in the integrated result."<sup>35</sup>

(2) *The Rorschach test is supposedly invalid.* Another criticism of a field in which the sociologic critic is no authority has to do with the Rorschach test. One such cavalier announces that "an eminent psychologist tells me that the Sheldon and Rorschach techniques have both been demonstrated to be invalid."<sup>36</sup> The unreliability of the criticisms of the Sheldonian somatotypes has been discussed. As to the other, I should like to be informed by whom and just how the Rorschach test has been "demonstrated to be invalid"; and invalid for what? Nobody claims that the Rorschach test (or, for that matter, any "projective" test or any intelligence test) is perfect. Yet nobody with even an elementary acquaintance with psychology and psychiatry would seriously assert that the Rorschach test has been "demonstrated to be invalid."

As a matter of fact, until UNRAVELING nobody had made a systematic study of a large sample of true delinquents as compared with a carefully matched control group of nondelinquents; nor had the Rorschach results been previously systematized into a rational clinical set of categories as they have been in UNRAVELING (Appendix E). It is to be emphasized that the boys in that research were subjected to the tests by different psychologists (in Boston) from those who interpreted the tests (in New York). The critic whose judgment is under review glides over, *en passant*, the fact that the New York experts on the Rorschach test (Dr. Ernst Schachtel and the late Dr. Anna Hartoch Schachtel) were not informed by us which of the "protocols" sent them for interpretation were the test-responses of delinquents and which were those of nondelinquents. The critic then unwarrantedly proceeds:

"Delinquents were assumed and then discovered to have less fear of authority and dependence upon it than 'nondelinquents' . . . De-

linquents were assumed, and then discovered to be, more unstable and impulsive in their behavior. . . . Delinquents were assumed and then discovered to be more aggressive and destructive. The logical relationship of these characteristics to delinquency is so obvious that a cynic might add that delinquents might be assumed to be law-breakers and then proven to be such by elaborate Rorschach devices!"<sup>37</sup>

This is patently prejudiced as well as absurd. In the first place, Rorschach responses cannot be successfully manipulated by a subject taking the test. In the second place, there are certain standards of interpretation of the Rorschach results. In the third place, the Rorschach experts who interpreted the protocols in New York were not the same psychologists who had given the tests in Boston; as pointed out, the interpreters did not know which protocols came from delinquents, which from nondelinquents. In the fourth place, this critic is silent about the Rorschach traits in respect to which it was found that delinquents do *not* differ from nondelinquents. (UNRAVELING, pp. 223, 226, 228, 233, 238, 272). Finally, any expert in Rorschach psychology can testify that the traits and factors brought out in the Rorschach materials of UNRAVELING are there in the protocols completed by the testees; they certainly were not "assumed and then discovered" by either the Rorschach experts or the authors of UNRAVELING.

One could turn the tables on this critic by saying that he assumes the predominance of general cultural influences, and then finds them to predominate; except that, contrary to his insistence that cultural forces are all important in delinquent etiology,<sup>38</sup> he now discovers that the various psychologic traits he cites bear an "obvious" and "logical relationship to delinquency"—something he evidently did not know until he read the findings of UNRAVELING.

But another critic seizes upon the Rorschach aspect of UNRAVELING to prove that, far from its being bad, it is the best part of the study! This generous judgment fits in neatly with his misconception of a statement by Dr. Schachtel, one of the experts who in New York interpreted the

<sup>37</sup> Taft, *op. cit.* *supra* note 24 at 304.

<sup>35</sup> MORRIS, Review of *PHYSIQUE AND DELINQUENCY*, 62 HARV. L. REV. 753-758 (1957).

<sup>36</sup> Taft, *op. cit.* *supra* note 24 at 303 n.1. The use of the Rorschach Test was also questioned by Rubin and Monachesi, among others. See Symposium in 15 FED. PROB. at 55, 57 (No. 1 1951).

<sup>38</sup> Taft's textbook on criminology attempts "to integrate a strong *cultural emphasis* with a *synthetic approach*." (The italics are his.) TAFT, *CRIMINOLOGY, AN ATTEMPT AT A SYNTHETIC INTERPRETATION WITH A CULTURAL EMPHASIS* vii (New York, The Macmillan Co., 1492).

Rorschach test protocols of the boys who had been tested in Boston. He says that Schachtel

"particularly emphasizes the importance of class culture and the milieu in which the delinquent lives as important factors in delinquency. Specifically, in making his judgments he states: 'The judgments would have been different if the socio-economic background had been different.' For the judgments were made by 'asking myself whether *his character structure, as I saw it on the basis of the Rorschach test*, was of a type likely to resist the inducements toward becoming delinquent offered by poor socio-economic circumstances and by the neighborhood.'" (Emphasis added.)<sup>39</sup>

This critic's interpretation of the quotation is amusingly typical of the way a few sociologists, eager to place their discipline at the head of the procession in the interpretation of the etiology of delinquency (and indeed denying there is any procession), misinterpret the findings of others to suit their preconceptions.<sup>40</sup> This critic omits altogether the crucial statement regarding *character structure* and speaks exclusively about the general culture. The significant thing in the above quotation is not culture but the *variations in character* which bring about a *differential response* to a similar *milieu*. (The role of culture is discussed below.)

Another criticism of the sample from the point of view of the Rorschach test has recently emerged: It is insisted that to list individual traits is all wrong; that the Rorschach is a "holistic technique, the entire pattern of responses is what is significant, not one response factor alone," and that the Schachtels evidently did not know their business in checking on individual traits.<sup>41</sup> To show that contradictory criticisms do not prevent the critics

<sup>39</sup> Reiss, *op. cit. supra* note 5 at 118.

<sup>40</sup> One reviewer of the most recent Glueck work has this diagnosis about the attitude of certain sociologists: "Since the foreign experience demonstrates these critics to be so wrong, the question arises as to whether major portions of their criticisms may not be traced to damaged professional pride due to the failure of the Gluecks to include a sociologist on their research staff." Fox, *Review of PREDICTING DELINQUENCY AND CRIME*, 40 B. U. L. REV. 157 n. 13 (1960).

<sup>41</sup> DATTA, *SOCIOLOGICAL THEORY IN CONTEMPORARY AMERICAN SOCIOLOGY* (unpublished thesis in University of West Virginia Library, 1955), quoted by Hartung, *op. cit. supra* note 6 at 705. In contrast to this student's amateur status, it is not altogether irrelevant to point out that the Schachtels, whose work she criticizes so cavalierly, had had many years' experience with the Rorschach Test and were trained by Rorschach himself.

from being mutually wrong, it should be pointed out that not only did the Schachtels list traits but before doing so they interpreted the Rorschach protocols in terms of whole character structure (the "psychogram"), taking into account the "test situation." This is what enabled them to make such a good record in deciding, without being told in advance, which of the protocols came from delinquents, which from nondelinquents. Appendix E of UNRAVELING describes the Schachtel method of analysis in detail and presents sample records and trait analyses.

When it is considered that, despite the fact that the psychiatrist and the Rorschach test interpreters, although neither was permitted by us to see the results of individual examinations by the other, achieved a very high incidence of agreement in diagnoses (we found what seemed to be inconsistencies between the two sets of diagnoses in only 74 out of some one thousand cases and in only 6 was there continued disagreement after the two experts went over the cases and straightened out semantic misunderstandings),<sup>42</sup> it must be conceded that both the Rorschach test and the psychiatric interview yielded extraordinarily reliable results.

So much, then, for the criticisms of the samples from the point of view of the Rorschach test.

(3) *The psychiatric interview was inadequate.* It has been pointed out that, since the psychiatrist interviewed most of the boys only once, there may be some weaknesses in the psychiatric findings. We anticipated that this criticism would be made. We ourselves would have preferred more contacts;<sup>43</sup> but who can say how many more interviews would have been sufficient—two, ten, or a complete psychoanalytic technique requiring years? Moreover, it must be borne in mind that the aim of the psychiatric interview was diagnostic rather than therapeutic.

The psychiatrist of the research, Dr. Bryant Moulton, is an expert with long experience in interviewing maladjusted children (having been for twelve years with the Judge Baker Guidance Center) and with a special gift of winning *rapport* with boys. But apart from this fact there is an internal test of the reliability of the psychiatrist's findings; that is, the evidence thrown up by a very

<sup>42</sup> See UNRAVELING at 242-43 n. 8.

<sup>43</sup> UNRAVELING at 60. Criticism of the single psychiatric interview was made, among others, by J. Satten, a psychiatrist, in the Symposium in 15 FED. PROB. at 55 (No.1 1951).

significant aspect of the design of the research, namely, the fact that care was taken to avoid infection of the findings of one scientist by those of another investigator. Thus, as already pointed out, the psychiatrist was not given access to the Rorschach test findings, nor were the Rorschach interpreters aware of the psychiatric diagnoses. Yet when we compared the diagnoses on the boys as determined by these single psychiatric interviews with the findings evolved through independent interpretation of the Rorschach test protocols, a remarkable similarity emerged.<sup>44</sup> So, also, when we compared the highly predictive factors (that is, those most markedly differentiating delinquents from nondelinquents) which resulted from the psychiatrist's interviews with those derived from the Rorschach tests, they were, taken as a pattern, quite consistent.<sup>45</sup>

### III. CAUSATIVE ANALYSIS

A. It has been claimed that, since our investigation "was eclectic and not guided by any theory," the interpretations of the findings "were necessarily *ex post facto*."<sup>46</sup> We have several things to say about such a judgment:

In the first place, no unilateral theory advanced by sociologists—whether culture conflict, the "ecologic characteristics of the interstitial area," "differential association," or "delinquency subculture"—is sufficiently comprehensive, sufficiently specific or sufficiently close to the realities of both individual and social psychology and the actualities of behavior to have been used as a guide to our research. Differential association, for example, is a very thin and distant abstraction without capacity to guide practical research.<sup>47</sup> And the delinquency subculture did not fall from the heavens; it was made, and is being sustained, by delinquent people, and to attribute etiologic exclusiveness or priority to it is to reason in a circle.

In the second place, our eclectic approach, broad and comprehensive as it is, was much better designed than any existing sociologic theory to get at the specific facts that differentiate delinquents from nondelinquents. In this connection, no re-

search, either before or after UNRAVELING, has so comprehensively and pointedly dug up traits and factors that are very probably etiologic.

In the third place, there was nothing *ex post facto* about the interpretations; on the contrary, every precaution was taken to avoid "reading in," and any careful student of UNRAVELING will find this to be so. The process was the familiar scientific one of comparison of a representative and substantial sample of true delinquents with an equal sample of true nondelinquents. (The charge of *ex post facto* reasoning is, by the way, an interesting illustration of the psychoanalytic concept of "projection"; it is certain sociologists, not we, who, desirous to "prove" their theories, manage to omit factual data running counter to their preconceptions, such, for example, as the biologically-rooted changes in interests, attitudes, and behavior that tend to occur with changes in age-spans no matter what the general culture or subculture may be.)

Discussion of the etiologic implications arrived at, not through preexisting preconceptions about this or that individual theory, cause, or discipline, but inductively, on the basis of the evidence emerging from comparison of over 400 factors at different levels of inquiry, is given in pages 281-282 of UNRAVELING and developed further in Chapter XV, "Riddle of Delinquency," of our more popular book based on UNRAVELING, *DELINQUENTS IN THE MAKING*.<sup>48</sup>

B. Our position as to *etiology* may be stated in these terms:

At the outset we had to reexamine the implications of the entire history of the study of the problem of crime causation, especially the causes of juvenile misconduct; for as Dostoevsky long ago shrewdly observed in his famous novel, *THE HOUSE OF THE DEAD*, "With ready-made opinions one cannot judge of crime. Its philosophy is a little more complicated than people think." The same may be said of ready-made theories.

It is well known that throughout the centuries there have been favorite, unilateral explanations—call them facts or theories—of criminalism. Some insight into the causation of delinquency or crime can be obtained from almost any approach that may seem to bear a relationship to the problem. Even meteorology can contribute; investigations have been made which show seasonal and climatic variations in crime and delinquency. Numerous studies have been conducted, especially in Europe,

<sup>44</sup> UNRAVELING, notes at 242-43, 252-53.

<sup>45</sup> *Psychiatric*: Adventuresomeness, suggestibility, stubbornness, extroversion in action, emotional instability; *Rorschach*: social assertiveness, suspiciousness, defiance, destructiveness, emotional lability.

<sup>46</sup> Hartung, *op. cit.* *supra* note 6 at 706, citing Reiss.

<sup>47</sup> Glueck, S., *Theory and Fact in Criminology*, 7 BRIT. J. DELINQ. 92 (1956).

<sup>48</sup> Harper and Bros., 1952.

of fluctuations in various indexes of economic conditions (prices of basic commodities, business activity, and the like) as related to the ups and downs of crime or delinquency. Many researches have been made, especially in Chicago, on the relationship of neighborhood conditions to crime. Numerous investigations have studied specific factors of environment and culture, such as the conflict of cultures between foreign-born parents and native-born children or between residents of different sections of the same region, the effect of bad companions, the dearth of adequate recreational facilities, and the like. More recently, old wine has been poured into new bottles under such general titles as the "differential association theory," the "delinquency subculture," etc.

There have also been many biologically-oriented investigations of the causes of delinquency and crime, from the famous and now questionable works of the Italian anthropologic criminologist, Lombroso, involving variations on the theme of the "born criminal," atavism or hereditary "throw-back," or degeneration, and certain forms of epilepsy, to the traditional psychiatric studies which emphasize psychoses, "psychopathic personality," and psychoneuroses. Finally, in more recent years, there have been a few psychoanalytic investigations, and numerous studies (based on inadequate samples and lack of comparable control groups of nondelinquents) of various individual psychologic, emotional, or characterial elements or patterns alleged to be causal of a tendency to criminalism.

Examination of existing researches in juvenile delinquency disclosed a tendency grossly to over-emphasize a particular science or explanation. Proponents of various theories of causation still too often insist that the truth is to be found only in their own special fields of study. Like the fabled blind men examining the elephant, each builds the entire subject in the image of that piece of reality which he happens to have touched.

Obviously then, a *many-sided approach* to this highly complex problem of human nature—a problem which has puzzled philosophers, scientists, parents, and clergy for hundreds of years, and judges and probation officers more recently—is, we were and are convinced, the only type of first step that gives promise of yielding useful insights leading to inductively arrived at theories and hypotheses which can then be subjected to more fruitful tests.

It is such a many-sided study, and for the rea-

sons noted, which Mrs. Glueck and I attempted in UNRAVELING.

To avoid as many pitfalls as possible, we designed an investigation in which we compared numerous persistently delinquent boys ranging in age from 11 to 17, with as many truly nondelinquent boys, matching them, boy for boy, by age, by ethnic (racial) derivation, by general intelligence (I.Q.), and by residence in underprivileged "slum" areas of Boston. This matching was necessary because the factors that had to be analyzed in order to dig closer to the roots of delinquency are so numerous that a way had to be found to avoid confusion. In order to study a large enough sample of variables, certain other sets of factors had first to be "held constant." In deciding which such factors to equalize among the two groups, through the pairing off of delinquents with nondelinquents as a preliminary to their later detailed study, we were guided by several methodologic aims: First, it was felt that since the ultimate comparison ought to deal with subtle processes of personality and environment, the more general or cruder factors should be kept as nearly as possible alike in the matching; secondly, those traits which typically affect a whole interrelated range of factors could also usefully be held constant; thirdly, those general characteristics which had already been explored sufficiently by other investigators and about which there was considerable agreement (such as the partial, indirect role of poverty, or of residence in a socially deteriorated urban area) could also be usefully held constant in the matching, to the extent possible in such an inexact field.

It was only after this careful matching of persistent delinquents with true nondelinquents that the two sets of boys were systematically and minutely compared in respect to the percentage-incidence of numerous traits and factors, measured or assessed at the following levels:

(1) *Family and personal background.* This is a comparative picture of the kinds of homes and families these two sets of boys came from, based on home visits and on the reconstruction, from numerous recorded sources, of the history of delinquency and criminalism, alcoholism, intellectual and emotional deficiency, physical ailments, economic and educational conditions and achievements, not only of the members of the boys' immediate families but also of grandparents, uncles, and aunts. The aim was to determine the conditions under which the parents of the two groups of boys had themselves been

raised, a situation that must have influenced the ideals, attitudes and practices which they, in turn, brought to the task of child-rearing so far as our boys were concerned.

The cultural, intellectual, and emotional conditions in the homes of our boys were of course likewise subjected to intensive study.

(2) *Boys' habits and use of leisure.* Attention here was especially directed to the age at which deviating behavior began and the nature of the earliest signs of antisocial conduct. There was also secured a detailed history of each boy's progress in school, as well as an assessment of the various forms of school misbehavior.

(3) *Physique types.* The bodily structure and form of the boys was determined by the use of photographs and their anthropologic measurement and classification in respect to fundamental physique patterns or somatotypes. After various measurements, the boys were classified anthropologically according to the predominance of one of the three root-components that entered into their development: *endomorphy*, in which softness and roundness of the various regions of the body predominate; *mesomorphy*, in which solid muscle, bone and connective tissue, and bodily compactness predominate; and *ectomorphy*, characterized by the relative emphasis of linearity and fragility of body form. A *balanced* type (relative similarity of all three components) was also identified. The importance of comparing delinquents and nondelinquents in terms of physique-type lies in the fact that the dominance of one or another of the three root components of body form may well imply basic variations in energy output, temperament, affect, and differential response to "differential association."

(4) *Health.* A significant comparison of delinquents with nondelinquents obviously had to include a medical examination, since poor health is often alleged to be a significant causal factor in delinquency. Through this we got a comparative picture of the general health and of the gross evidence of various diseases in the two sets of boys.

(5) *Intelligence.* It is commonly believed that the hows and whys of human behavior are largely governed by the degree and quality of intellectual power. One of the bases of original matching was general intelligence as summarized in the I.Q. This equating of global intellectual capacity gave us a chance to compare the delinquents with the controls in respect to various constituent elements of

intelligence; two boys of like I.Q. can have quite varying intellectual qualities in terms of specific traits, talents and deficiencies.

(6) *Temperament and character structure.* In recent years, those who have studied the human mind and behavior have concluded that the temperamental and affective life of the individual, and especially the feeling-laden experiences and trauma of the first few years in the home, have a great deal to do with the development of personality, the structuring of character, and the channelling and habituation of behavior-tendencies. To reach the main features of character-structure, various "projective" tests have been evolved. The Rorschach (or ink-blot) Test, which the psychologists applied to both sets of boys, is one of the most revealing of such probes of the foundations of personality and character.

As pointed out, there was also an interview by a skilled psychiatrist with each boy. This revealed the more obvious personality traits of the boys, uncovered their many emotional stresses and conflicts and how they resolved them and provided some clues to the reasons for the persistency of misbehavior of the delinquent lads and the conventional conduct of the control group.

Such an extensive and intensive exploration at so many levels involved years of careful investigation, tireless verification of data from many sources, entering of the numerous factors on statistical schedules according to carefully prepared definitions, and meeting many delicate problems of public relations. Several chapters in UNRAVELING render full account of these various techniques, so that the interested reader can judge for himself the reliability of the raw materials that went into the numerous tables and statistical computations from which the basic conclusions of this comprehensive study were derived.

Such, oversimplified in the above description, are the philosophy and technique of UNRAVELING JUVENILE DELINQUENCY.

C. How do these fit into our practical view of "causation"?

(1) There has been altogether too much seemingly profound but essentially superficial writing on the theme (borrowed from the pure sciences) that one must never use the term "cause," but only such evasive expressions as "associated factor," or "decision theory," etc. But the issue is a pragmatic, not a semantic, one. Where a considerable number

of factors that "make sense" from the point of view of common and clinical experience are found to characterize delinquents far more than nondelinquents (the difference not being attributable to chance) it is highly probable that what is involved is an etiologic *connection* between them; in other words, the delinquency not only follows the traits and factors that have been found to precede it, but follows *from* them. The soundness of such a conception of "causation" from a practical point of view is provable by (a) the fact that the concatenation of different traits and factors yields high predictive power in the sense of identification of delinquents and nondelinquents when applied to a considerable variety of populations, and (b) the fact that when such patterns of assumedly criminogenic traits and factors are absent from or are eliminated from a situation, delinquency usually does *not* exist or is greatly diminished.

The possibility that some day variations in the behavior of people may be explainable largely in the more ultimate terms of differences in, say, endocrine gland function, or of microscopic physico-chemical reactions does not, in the meantime, prevent effective action on the basis of existing cruder assessments of reality, any more than the recent development of nuclear science prevented effective coping with many problems of nature through employment of prenuclear chemistry and physics. In the meantime, it can serve no useful purpose for criminologists to keep wringing their hands about the inadequacies of the etiologic researches thus far produced. Medicine made therapeutic strides in several fields long before the specific etiologic agents in certain diseases were discovered (in the treatment of malaria, for example).

Nor should too much time and energy be expended in the armchair search for some grand, all-unifying theory. Fruitful theories and hypotheses usually spring only from soundly gathered facts which suggest clues or influences leading to further investigations. The multidisciplinary evidence of our comparisons of delinquents with nondelinquents convinces us that, taken in the mass, if boys in the underprivileged urban areas have in their makeup and early conditioning certain identifiable traits and factors found markedly to differentiate delinquents from nondelinquents, the boys are very likely to turn out to be delinquent. In this general sense of *high probability* of persistency of antisocial conduct related to the presence of a sufficiently weighty combination of

differentiative characteristics of person and *milieu*, a rough etiologic relationship was established.

(2) It should be emphasized that throughout the work in analyzing the data for UNRAVELING, we insisted on the fundamental importance of *sequence in time*. That is why we ruled out gang membership (frequently emphasized as a cause of delinquency) and other influences which were found to have occurred long after definite proof of antisocial behavior. The onset of persistent misbehavior tendencies was at the early age of seven years or younger among 48 per cent of our delinquents, and from eight to ten in an additional 39 per cent; thus a total of almost nine-tenths of the entire group showed clear delinquent tendencies before the time when boys generally become members of organized boys' gangs. The leading authorities on the subject recognize the gang as "largely an adolescent phenomenon"; for example, of some 1,200 cases of gang membership studied in Chicago, only 1.5 per cent of the boys were six to twelve years old, while 63 per cent were classified as adolescents.

(3) We further decided that "cause" (we would have preferred to use a less controversial term) involves a *totality* of conditions necessary to bring about the delinquency result; individual traits or factors are only parts of the total cause. That is why our *nondelinquents* sometimes had in their makeup and background *some* of the traits and factors found among the delinquents. Not all the characteristics arrived at inductively and included in our general etiologic formulation will be found in all delinquents and be absent in all nondelinquents.

Consider, for example, the fact that twice as many delinquents as non-delinquents were found to be of the closely knit, muscular, athletically inclined (mesomorphic) type. The very fact that 30 per cent of the *nondelinquents* were also of this physique immediately contradicts any conclusion that *mesomorphy* inevitably "causes" persistent delinquency. Or, consider such a trait as *defiance*, which one would naturally regard as closely related to delinquent behavior tendencies. True, 50 per cent of the delinquents had this characteristic, but 12 per cent of the *nondelinquents* also had it; and the very fact that half the delinquent group did *not* display this trait further reveals the inadequacy of conclusions about causation derived from a single factor.

Thus, a single factor (or even a small group of

factors) may be involved, and even frequently involved, in delinquent behavior and yet will not, of itself, have sufficient weight or potency to tip the scales among boys who remain nondelinquent. In any realistic sense, the cause of an effect is that *totality* of conditions sufficient to produce it. The fact noted that as many as half of the delinquents did *not* have the trait of *defiance* is but one illustration of the absence among a considerable group of delinquents of characteristics which are present in some other large group of delinquents. It reveals the usually unrecognized truism that persistent delinquency is not the potential result of only one specific combination of factors which markedly differentiates delinquents from nondelinquents, but of each of several *different* combinations. Just as death, although always the *same terminal event*, may be the result of *various preceding sequences* of conditions, so the terminal event of persistent delinquency may be the result of a variety of different sequences. For we are dealing with a complex aggregation of internal and external conditions which are associated with socially maladjusted, unlawful behavior, and not all of them are indispensable to the result in any single case.

In criminal conduct, as in most other forms of human expression, every person has his individual breaking point. It is difficult for all members of any society at any one time to conform to the requirements and prohibitions of socially desirable behavior, because this involves a subordination of the natural impulses of sex expression, aggression, and the like, to those conduct norms which the law has declared necessary to the general welfare. But most persons are able (through various combinations of numerous factors of native endowment and elaborate conditioning in home, school, church, neighborhood and supportive elements in the general culture) to meet the requirements of the major legal standards of the age and place wherein they live. If a boy persists in delinquency, it means that his power of resistance to natural impulse, or his desire to obey socio-legal mandates, has been overbalanced by the strength of the other circumstances that incline to antisocial behavior.

(4) Closely related to the preceding concepts is the concept of *probability*. Nowadays, even the physical sciences state their generalizations in terms not of absolute inevitability but only of high probability. The statistical method of comparing delinquents, as a group, with nondelinquents, as a group, is not designed to bring out any point-to-point causal

sequence that will always hold good for each and every case. It is rather intended to disclose whether or not a group of boys having a certain cluster of factors in its makeup and background will much more probably turn out to be delinquent than a group not so loaded down; or, to put it differently, whether the "typical" delinquent is likely to be the result of such a concatenation of factors.

(5) The intellectual fruitfulness of our etiologic conceptions compared to the unilateral theory of the sociologist is shown by an example to be found in *PREDICTING DELINQUENCY AND CRIME*.<sup>48a</sup> Our Social Prediction Table (discussed below) is effective in about 90 per cent of the new cases to which it is applied. Why does it not also select potential delinquents and nondelinquents in the remaining 10 per cent? The answer is that there are cases in which the etiologic-predictive nexus based on the factors of family life shown to be highly discriminative in most cases is not adequate to identify delinquents and nondelinquents among the 10 per cent because in those cases *certain other characteristics* are sufficiently potent to counteract the family influences. In other words, there is a *core type* of delinquent from an etiologic-predictive point of view, and there are one or more *fringe types*. If we examine the characteristics of the 10 per cent who were not successfully identified by the Social Prediction Table, it turns out that these *atypical* delinquents, although they come from *wholesome* families, differ from the typical (core) group in respect to many characteristics, particularly those derived from the Rorschach test and psychiatric examination.<sup>49</sup> Some of these traits suggest a quite consistent pattern of neurotic attributes (*e.g.*, marked submissiveness, enhanced feelings of insecurity, fear of failure and defeat), together with a *low* incidence of such characteristics as destructiveness and adventurousness which were found to be highly differentiative in the core-type of delinquent. Other traits suggest the possibility of there existing still another fringe type. We felt justified in calling this pilot analysis "From Prediction to Etiology," because it is by this method of inductive analysis, imperfect as it still concededly is, that one approaches the determination of true etiologic involvements.

So much for our conceptions of causation.

<sup>48a</sup> GLUECK, S., & E. T., *PREDICTING DELINQUENCY AND CRIME* (1959).

<sup>49</sup> *Id.* at 263-73.



D. We are convinced that the *criticisms* of our tentative etiologic formulation are unsound:

(1) One critic points out that the etiologic "law"<sup>50</sup> is untested.<sup>51</sup>

<sup>50</sup> "The delinquents as a group are distinguishable from the non-delinquents: (1) physically, in being essentially mesomorphic in constitution (solid, closely knit, muscular); (2) temperamentally, in being restlessly energetic, impulsive, extroverted, aggressive, destructive (often sadistic)—traits which may be related more or less to the erratic growth pattern and its physiologic correlates or consequences; (3) in attitude, by being hostile, defiant, resentful, suspicious, stubborn, socially assertive, adventurous, unconventional, non-submissive to authority; (4) psychologically, in tending to direct and concrete, rather than symbolic, intellectual expression, and in being less methodical in their approach to problems; (5) socio-culturally, in having been reared to a far greater extent than the control group in homes of little understanding, affection, stability, or moral fibre by parents usually unfit to be effective guides and protectors or, according to psychoanalytic theory, desirable sources for emulation and the construction of a consistent, well-balanced, and socially normal superego during the early stages of character development. [For footnote to this, see UNRAVELING at 283 n. 9.] While in individual cases the stresses contributed by any one of the above pressure-areas of dissocial-behavior tendency may adequately account for persistence in delinquency, in general the high probability of delinquency is dependent upon the interplay of the conditions and forces from all these areas.

"In the exciting, stimulating, but little-controlled and culturally inconsistent environment of the underprivileged area, such boys readily give expression to their untamed impulses and their self-centered desires by means of various forms of delinquent behavior. Their tendencies toward uninhibited energy-expression are deeply anchored in soma and psyche and in the malformations of character during the first few years of life.

"This 'law' may have to be modified after more intensive, microscopic study of the atypical cases. For example, there are instances in which the delinquents are more ectomorphic than mesomorphic in constitution, and cases in which the delinquents are of the introverted, psychoneurotic temperament. There are also some *non*-delinquents who have been reared in immoral and criminalistic homes. While all these groups are relatively small in number, they deserve further study, and their more intensive consideration may result in modification of the basic analysis. 'A scientific law must always be considered as a temporary statement of relationships. As knowledge increases this law may require modification. Even the natural sciences state all their generalizations in terms of probability.' " (The quote within the quotation is from SELLIN, *CULTURE CONFLICT AND CRIME*.) UNRAVELING at 281-82. The summary is preceded by a careful analysis of the findings of the research. See UNRAVELING, ch. XXI, "Dynamic Pattern of Delinquency."

<sup>51</sup> Rubin, *op. cit. supra* note 5 at 112. He quotes the Gluecks as saying "physical anthropologists have not yet answered a major question, namely, whether or not the somatotype remains constant and, if it does, whether, in the formative years of growth around the age of six or seven, when children normally enter school, the physique type is as yet reliably distinguish-

able." One thing at a time. It was clearly presented as tentative; and no claim whatsoever was made that it is final or perfect. On the contrary, the entire discussion made clear the need of testing the general conclusion by further researches. We should be pleased if a team of specialists similar to those who worked on UNRAVELING, but aided by the presence of a relatively unbiased sociologist, were to make a study similar to ours, benefiting from our mistakes.

(2) This critic further points out that the "law is not limited as to place, age, or administrative policy, to accord with the limitations of the sample."<sup>52</sup>

On page 272 of UNRAVELING the limitations of the general sample are set forth. However, we are now able to say that the results of a number of gratifying applications of the Glueck Social Prediction Table would seem to indicate the likelihood of the findings being essentially repeated on another sample of delinquents compared with nondelinquents.

(3) This critic also calls attention to the fact that some of the differentiative factors affect only small groups of delinquents and nondelinquents.<sup>53</sup>

This matter has already been discussed in con-

able," and asks "what justification is there to include physique in the law?" He overlooks the fact that the boys were much older than six or seven when somatyped for UNRAVELING, their average age then having been about 14½ years. Rubin then points out that we rejected physique for predictive purposes. The reason for this (as plainly indicated in the quotation focused upon by Rubin) was that we were not as yet confident that the physique type was reliably distinguished at age six or seven, when prediction is attempted. There is thus no inconsistency between our use of somatotypes on the boys in our samples (at about 14½) and our failure to use them for prediction (at about 6 or 7).

It should be noted that since publication of UNRAVELING, Mrs. Glueck has worked out a prediction device using somatotype data, because evidence was beginning to appear that somatotyping can in fact be made early in life (at age 7, for example). In an English study, "It has been shown that the somatotype can give some indication of the kind of behaviour to expect in different individuals, and that exact measurements support what has for centuries been appreciated in less precise form, namely, that physical constitution plays an important part in shaping people's lives." Davidson, McInnes & Parnell, *The Distribution of Personality Traits in Seven-Year Old Children: A Combined Psychological, Psychiatric and Somatotype Study*, 27 BRIT. J. EDUC. PSYCHOLOGY 48-61 (1957). See, also, Parnell, *Physique and Family Structure*, 51 THE EUGENICS REV. 75-88 (1959).

<sup>52</sup> Rubin, *op. cit. supra* note 5 at 112. Note the statement regarding the limitations of our tentative formulation in UNRAVELING at 272.

<sup>53</sup> Rubin, *op. cit. supra* note 5 at 112.

nection with a similar criticism of the tables in *PHYSIQUE AND DELINQUENCY*. It should be re-emphasized that the fact that certain differentiative factors affect relatively small groups of delinquents and nondelinquents is proof of the existence of fringe types in which the etiologic syndrome differs, in whole or in part, from that which characterizes the core type of delinquent.

The formulation suggested by the facts of our research yields a point of view toward causation equally applicable to urban and rural regions, to crime in primitive systems and in developed cultures, to "white collar" and black collar crime, to crime in 1960 and in George Washington's day, etc., because it recognizes a variety of etiologic patterns and takes account of the continuous conflict between the individual's tendencies toward the gratification of his urgent egoistic desires, on the one hand, and sociocultural taboos and prohibitions on the other. Until the sociologists can produce a more fruitful and realistic formulation than they have thus far developed, we prefer to adhere to our multicausal analysis rather than to have our revealing factual findings, in Tennyson's words, "Veneer'd with sanctimonious theory."

E. The same critic protests that "none of the law goes beyond mere correlation." He has a naive conception of causation, especially in the biosocial field. High intercorrelation is of the very essence of etiologic implication, provided the factors found to be greatly interrelated with delinquency and not with nondelinquency are those that "make sense" from the point of view of clinical experience and provided it is possible to test the influence of the etiologic factors by removing them (or enough of them) from the personal-social situation in a series of cases and noting whether or not the behavior changes.<sup>54</sup>

#### IV. THE ROLE OF CULTURE

A. Almost all the sociologic critics claim that we have ignored what they conceive to be the most sig-

nificant, if not indeed the *exclusive*, source of delinquency—the cultural *milieu*. Our view as to this (already touched upon above in discussing the control of neighborhood as a prerequisite to the detailed comparison of the delinquents with the nondelinquents) is that individuals differ with reference to the elements of culture that affect them. They tend to choose, or to succumb to, those aspects of a culture which are naturally more congenial. Individuals vary in constitution, temperament, strength of innate drives, and degree of integrative and inhibitory capacity. These differences are the result not only of early conditioning, but also, to an extent as yet unmeasurable, of heredity. Especially when the educative and supportive social agencies are inadequate or in process of rapid change does the reaction of different individuals subjected to a similar culture vary. Some find it impossible to inhibit their primitive impulses in the absence, or even in the presence, of the deterrent influence of external force; others have so efficiently "internalized" the psychologic accompaniments of various forms of authority that they have an efficient superego (conscience) which, despite major changes in cultural controls, still enables them to "toe the mark."

But apart from the evidence of general experience as to nature and nurture, there are several crucial statistics that cast serious doubt upon the view held by some sociologists that such cultural entities as the gang, or the "delinquency subculture," or the "lower-class subculture," or the wider general culture comprise the most potent (if not the exclusive) "cause" of delinquency.

(1) As to *gang membership*, as previously pointed out, it has been established, in *UNRAVELING JUVENILE DELINQUENCY*, that almost nine-tenths of the delinquent youths had already shown clear signs of antisocial behavior when they were under eleven years of age; and the typical "gang age" is well beyond that period, in adolescence.<sup>55</sup>

<sup>54</sup> This type of approach, and not adherence to some *a priori* theory through thick and thin, is the method of science. "The main cause of this unparalleled progress in physiology, pathology, medicine and surgery has been the fruitful application of the experimental method of research, just the same method which has been the great lever of all scientific advance in modern times." Dr. William H. Welch, Argument against Antivivisection Bill, S. Doc. No. 34, 56th Cong., 1st Sess. (Feb. 21, 1900), quoted in 1 CUSHING, *LIFE OF SIR WILLIAM OSLER* 521. Of course, in the field under discussion experiment must take a less precise and more humane path than in the laboratory.

<sup>55</sup> The charge that "the Gluecks ignored their own finding that almost all of the delinquents were members of gangs and that only three of the nondelinquents belonged to gangs" (Hartung, quoting Clinard, *op. cit. supra* note 6 at 705) is simply not true. In the first place, not almost all delinquents were found to belong to gangs, but 56%. (*UNRAVELING*, Table XIII-16 at 163). In the second place, as pointed out in the text the reason for excluding gang-membership in the *etiology* of delinquency is the fact that gang-membership is an adolescent phenomenon and nine-tenths of our delinquents were already manifesting clear signs of delinquency at age 10 or less. "Factors that come into play after persistent antisocial behavior is es-

(2) As to *neighborhood culture*, even in the most marked "delinquency areas" or delinquency subcultures of our cities, not more than a small fraction of the boys (say 10 or 15 per cent) become delinquent. It is unreasonable, therefore, to emphasize the role of neighborhood influences on the small percentage of boys who become delinquent and utterly ignore the fact that the vast majority of the boys in the same neighborhoods somehow manage *not* to follow a persistent antisocial career.

(3) As to the wider *general culture*, the New York City Youth Board has established that in America's leading urban center no fewer than 75 per cent of the delinquents are contributed by only 1 per cent of the families.<sup>55a</sup>

If the neighborhood subculture and the general cultural patterns are as permeatively and inevitably criminalistic as they are said to be, and if the values they represent are as powerful in their criminogenic influences as claimed, how account for these statistics?

B. It will not be amiss to say a few words about the relationship of culture to the individual differences of those subjected to it, a crucial matter which certain sociologists either ignore or explain away very unconvincingly:

Suppose ten youths go out in a boat on a lake. The boat springs a leak and fills with water. Two boys drown; the other eight successfully reach shore. It happens that one of the drowned boys did not know how to swim, and the second could swim but had a weak heart which could not stand the excessive exertion; the other eight were good swimmers in fine physical condition.

Under the circumstances, what is the more rational procedure: to focus primarily and (according to some even exclusively) upon the composition of the water in the lake or, while not ignoring the lake as the setting of the deaths to be explained, to concentrate on the relevant *varying* characteristics of the individuals subjected to the very same hazard but with such widely differing results?

established can hardly be regarded as relevant to the original etiology of maladaptation, except as they may reflect deep-rooted forces which do not make themselves felt in a tendency to dissocial behavior until puberty or adolescence is reached." UNRAVELING at 272. See, also, DELINQUENTS IN THE MAKING (based on UNRAVELING) at 166.

<sup>55a</sup> The basic importance of family life has been confirmed in Japan: "(e) family circumstances are more important prediction factors than neighborhood circumstances." JUVENILE DELINQUENCY AS SEEN IN THE FAMILY COURT OF JAPAN 15 (Ministry of Justice, Japan, 1957).

What was the chief "cause" of the drowning of the first two boys? Was the water the cause? This cannot be so because, despite the fact that two of the youths drowned, eight others, subjected to the same hazard, managed to survive.

The eight youths were saved, not because the water, in their case, was less deep or less wet, but because they could swim and were in good physical condition. The first boy was drowned, not because the water, in his case, was different from that of the others, but because he could not swim. The second lad was drowned, not because the general "cause" inevitably would result in people drowning, but because, although he could swim, he did not have the necessary strength to swim the required distance to shore.

Now the water is equivalent to the "delinquency subculture"; of ten persons subjected to a like external influence, only two succumbed. The condition that affected them is *general*. It is equivalent to the existence of the institution of property and of laws against theft: *all* men are subject to such a uniform situation, but the vast majority of them do *not* commit larceny.

Now suppose this same little drama were enacted on the high seas, where the background forces are much stronger than in a lake. *All* youths are by this *force majeure* reduced to an almost similar state. True, the first two boys probably drown earlier than the others; but the general environmental condition with which all the boys have to struggle is now so overpowering as to make virtually irrelevant any difference in *individual* capacity or equipment.

Of course, the analogy is not perfect; the characteristics of a culture-medium are much more subtle and complex than are the properties of a lake, as is also the dynamic interplay between the culture and the human organism. But the basic principle illustrated by the foregoing example is similar, as an explanatory device, to the principle involved in assessing the role in criminogenesis of the special subculture of gang membership, or the interstitial area, or the working or middle class subculture, or the process of differential association—favorite explanations of delinquency advanced by certain criminologists.

It cannot be denied that despite the many unwholesome and antisocial features of our culture the great majority of people are, in normal times, relatively law-abiding. In the very research under review, we were able to find 500 boys, living in high delinquency areas of Greater Boston, whom

extremely careful investigation showed to be nondelinquent. In times of exceptional crisis, such as widespread economic depression, some persons who have been treading a precarious zone between law-abidingness and criminalism go over, or are pushed over, into antisocial territory; but even in such times the great majority remain law-abiding. Yet they have been living and making decisions and acting in the same malign culture as those who become delinquent or criminal. To insist, therefore, that in such a situation cultural influences are the most satisfactory explanation of the incidence of delinquency and crime is seriously to distort the actual picture.

Antisocial aspects of culture are only *potential* or *possible* causes of delinquency; persons of varied innate nature and early parent-child relationships respond to those elements of the culture which they wish, or are impelled, to *introject* and thereby transform into antisocial motives. In brief, certain sociologists, in their eagerness to promote their profession and its assumedly deserved leadership in explanation of delinquency and crime, forget that environment—"culture"—plays no role in conduct unless and until it is, as it were, emotionally swallowed, becoming part of the motivating force against the taboos and demands of the prevailing culture. This oversight, among others (including a defective conception of the learning process and of motivation), accounts for the superficiality and puerility of the differential association theory.<sup>56</sup> To those who cling to the theory, there seemingly are no such realities as the gene and the germ plasm or individual differences in bodily morphology, temperament, intelligence, etc.; nor is there much significance to them in that most crucial of all cultural influences, parent-child relationships during the first few years of life. To these theorists it is all the peer-group, the gang, the regional subculture, the general culture, the *Massenmensch*. How they can reconcile their extreme overemphasis of environmental conditioning with actual differences among human beings from birth is a question they do not deign to answer.

C. The critic whose misinterpretation of Schachtel's Rorschach statement I have mentioned also claims, it will be recalled, that the Gluecks ignore culture completely. With certain loyal sociologists it is all or nothing. Although all experience, in

botany, zoology, biology, medicine, proves daily that the resultant product is the outcome of the interplay of the seed and the soil, they write as if nothing counts but the soil.<sup>57</sup> But in point of fact, the Gluecks do *not* ignore culture. In stating their views in the opening chapter of *UNRAVELING*,<sup>58</sup> and in the close of their summary of the dynamic etiologic pattern arrived at inductively, they say, *in italics*:

"In the exciting, stimulating, but little-controlled and culturally inconsistent environment of the underprivileged area, such boys readily give expression to their untamed impulses and their self-centered desires by means of various forms of delinquent behavior."<sup>59</sup>

But they add, on the basis of carefully assembled, verified, and as nearly as possible measured data, that the

"tendencies toward uninhibited energy-expression are deeply anchored in soma and psyche and in the malformations of character [compare Schachtel's misinterpreted statement] during the first few years of life."<sup>60</sup>

## V. THE PREDICTION METHOD

A. Ever since the publication of *UNRAVELING*, a few critics have attacked that work on the ground that the prediction tables could not possibly forecast efficiently because they are based on equal proportions of delinquents and nondelinquents (500:500), whereas the proportion of nondelinquents to delinquents, even in the most extreme "delinquency area," is only about 9:1.

We have avoided taking issue with these critics, although from the beginning we believed them wrong and certainly premature in their theorizing, because we felt that the most effective answer would have to be the pragmatic one of the "proof of the pudding"<sup>61</sup> through actual experience.

<sup>57</sup> *Ibid.* There are of course many exceptions; the names of Professors Paul Tappan, Thorsten Sellin, Albert Morris, Peter Lejins, Marvin Wolfgang and, in Europe, Hermann Mannheim and Wolf Middelndorf, come to mind.

<sup>58</sup> *UNRAVELING* at 5-6.

<sup>59</sup> *Id.* at 282.

<sup>60</sup> *Ibid.* See also *UNRAVELING* at 15.

<sup>61</sup> "In their present work, after a long and careful analysis of a large number of physical, psychological and social characters attributable to 500 delinquents and to an equal number of controls (non-delinquents matched pairwise with the delinquents in respect to age, ethnic origin, intelligence and type of area of residence), the Gluecks set up a series of proposed prediction tables based on attributes common among delinquents and uncommon among the controls, or

<sup>56</sup> Glueck, S., *Theory and Fact in Criminology*, 7 BRIT. J. DELINQ. 92 (1956).

Sufficient evidence has now been accumulated in various validation experiments of one of the tables (known in the literature as the Glueck Social Prediction Table) to enable us to give careful consideration to these criticisms.

The basic objection is entered, for example, by Reiss, who states dogmatically that "unless this [50%] is the actual rate in a similar population for which the predictions are made, the tables will yield very poor prediction."<sup>62</sup> Reiss recomputed the Glueck Social Prediction Table, taking account of an assumed 9:1 ratio of nondelinquents to delinquents, and purports to prove that, by his figures, the original table can show very little predictive capacity:

"It can be seen in Table 2 that, so far as prediction of delinquency or nondelinquency is concerned, the table has a low predictive efficiency when the rate of delinquency is estimated at 10 per cent. For example, in the score 250-99 the chances of delinquency were 63.5 in the Gluecks' table, while the chances are only 16.2 per hundred in the table which assumes a rate of 10 per cent habitual delinquency."<sup>63</sup>

Reiss and similar critics have not clearly explained just why the adjustment to a supposed actual proportion of 9:1 is necessary; or, why differences in the incidence of delinquents and nondelinquents in any population should and would have a serious distorting influence on the distribution of scores of the predictive factors as presented in our 50-50 table.

(1) If their point is, simply, that where the proportion of nondelinquents to delinquents in a population is far higher than one-half, one can just as readily "predict" by assuming all boys to be nondelinquent as by going to the trouble of examining the families to see which boys possess the deleterious categories of the predictive factors in their background and which do not, then the point is insignificant. *Of course* one can assume

vice versa. I do not see how they could have done better, but the proof of the pudding can come only with its eating." Wilson, *Prediction*, 64 HARV. L. REV. 1040 (1951).

<sup>62</sup> Reiss, *op. cit. supra* note 5 at 118. Similar criticisms have been made by Shaplin & Tiedman, *Comment on the Juvenile Delinquency Prediction Tables in the Gluecks' "Unraveling Juvenile Delinquency,"* 16 AM. SOC. REV. 544 (1951); Stott, *The Prediction of Delinquency from Non-Delinquent Behaviour*, 10 BRIT. J. DELINQ. 195 (1960); Duncan, *Review of "Predicting Delinquency and Crime,"* by S. and E. T. Glueck, 65 AM. J. SOC. 537 (1960).

<sup>63</sup> REISS, *op. cit. supra* note 5 at 119.

that in a population in which the proportion of nondelinquents to delinquents is 9:1 all boys are nondelinquent and thereby triumphantly point out that one has guessed wrongly in only 10 per cent of all cases, which is alleged to be as good as the Glueck table is able to do.<sup>64</sup>

But that is not the issue.

The issue is whether one can *identify*, individually, the future delinquents and the future nondelinquents; otherwise one is not really predicting at all but asserting what was known, *ex hypothesi*, beforehand.

(2) It may be, however, that what the critics have in mind is the possibility that there could be factors involved in distinguishing delinquents from nondelinquents *other* than the ones used in the Glueck table, which express themselves in the result that in the community to which a 50-50 table is to be applied the proportions are really 90:10.

This raises certain important issues. There are seemingly three problems involved in the criticism under discussion:

(a) Will the Glueck table's distribution of scores for samples of *delinquents*, as found in the original Boston study, reproduce itself in other communities?

(b) Will the table's distribution of scores for *nondelinquents* reproduce itself in other populations?

(c) Even assuming that the distributions of new populations of delinquents and nondelinquents are identical with the Glueck table, will the table prove to be an effective predictive instrument in a population in which the proportion of nondelinquents is much higher than one-half?

It seems to us that in respect to all three of these questions the critics are confusing the counting of heads with the *weighing* of heads; a blind census with a device for *pinpointing* delinquents and nondelinquents; the percentage-incidence of nondelinquents and delinquents in a particular region with the incidence of certain criminogenic factors in a representative sample of delinquents compared with a representative sample of nondelinquents. To put it differently, they are confusing the effect of differences in the size of two statistical

<sup>64</sup> Indeed, one critic rushed into print in the New York City press (claiming, incidentally, that he represents a society of some 2,000 psychologists and thereby implicating them) with the assertion that by "pulling names out of a hat" he could do just as well as can be done by the Glueck table!

"universes" with differences in the size of substantial, representative *samples* drawn from those two universes. By confusing the concept of universe with that of sample, the critics are begging the very question at issue, which is: Will the factors embodied in the Glueck table actually identify prospective delinquents and potential nondelinquents with a high degree of accuracy, irrespective of other influences, *including the differences in the size of the universes from which the original samples were drawn and to which they are applied?*

Our prediction table is not designed to forecast the probable chances of a boy's delinquency if he lives in Boston or a city like Boston; but rather the chances of his becoming delinquent if he happens to have in his makeup and background the crucial factors which have been shown, in comparing numerous delinquents with numerous nondelinquents, to be highly associated with persistency in misbehavior.

(3) *The use of equal numbers in the samples* originally compared is not only legitimate but important for the accurate determination of the incidence of the factors under comparison. It is, for example, a frequent technique in medical research. The difference in the proportion of the *pools* from which the samples of nondelinquents and delinquents were drawn can affect the outcome only if influential factors other than those embraced in the samples are omitted or included; and this is to be determined, not *a priori*, by manipulating the original table to reflect a 90:10 proportion, but only empirically, by the application of the table to other populations. Assuming that the sample of delinquents and the sample of nondelinquents are fairly representative of the populations from which the cases were derived, the fact that the total group of nondelinquents in the general population is nine times as numerous as the total group of delinquents can have little to do with the outcome when comparing the two samples; and it should, equally, have little to do with the outcome when applying the table to new populations.

If one were making a study comparing the incidence of blood pressure, pulse, certain chemicals in the blood and urine, etc., of persons with a malignant disease, with their incidence among healthy persons, would it make any difference whether the *general* incidence of such diseased persons in the particular community amounted to 10 per cent or 50 per cent? And, assuming that in the city in which the original experiment was done

the population proportions of the well and the ill were 50-50, would this fact interfere with the predictive capacity of a table of indications and symptoms when applied to a city in which the proportions were 90:10?

Our own view has been and is that the variation in the proportion of nondelinquents to delinquents as between the original population and the new one to which the table is applied should have very little to do with the capacity of the table to "spot" the delinquents and the nondelinquents. The reason is that the characteristics inductively arrived at as most markedly distinguishing the two groups were selected from among more than four hundred factors as to the incidence of which the 500 pairs of matched delinquents and nondelinquents had been compared at levels of study ranging, widely, from anthropometric and psychiatric to social. It should be borne in mind, too, that by chi-square calculation we regarded as statistically significant only those factors in which the probability of the difference found between the delinquents and the controls being due to chance or random sampling was less than one in a hundred.<sup>65</sup>

We are aware of the fact, however, that, despite all this, empirical evidence might disprove our hypothesis as to the high identifying capacity of the prediction table. But one should not dogmatize at the outset that the influence of differences in proportions will seriously affect the outcome; one must await the proof of the pudding.

B. This brings me to the series of *validation studies* that have thus far been made. They have been carried out by different investigators, some in Cambridge and Boston, Massachusetts, some in New York City, one in New Jersey, one in Ohio, one in Strasbourg, France, several in Japan. The table has so far been applied to many hundreds of children and adolescents (in three instances to females). In each study the table has given highly encouraging results.

Dr. Eleanor T. Glueck has analyzed the results in two articles,<sup>65a</sup> with the following general conclusion:

"The accumulated evidence thus far gathered from 'retrospective' and 'prospective' studies

<sup>65</sup> See "Explanation of Statistical Method," by Carl R. Doering, in *UNRAVELING* at 75-76.

<sup>65a</sup> Glueck, E. T., *Spotting Potential Delinquents: Can it Be Done?*, 20 *FED. PROB.* 7 (No. 3 1956); Glueck, E. T., *Efforts to Identify Delinquents*, 24 *FED. PROB.* 49 (No. 2 1960).

both in the United States and in foreign countries all seems to be tending in the same direction. A total of 18 inquiries in which the Social Prediction Table has been applied are all suggestive of its usefulness. The studies include four samples of nondelinquents (the latter incorporated into the first of the two investigations by Thompson in 1954, in the New York City Youth Board Study, in the Maximum Benefits Project [Washington, D. C.], and in the first of the Japanese inquiries); three studies include girls (the second Thompson study, the study by Glick and Donnell of 150 unmarried mothers, and the Maximum Benefits Project).<sup>66</sup>

Two types of validation studies have been made by various investigators:

(1) The *retrospective* checkups include a study by Black and Glick of the Jewish Board of Guardians on 100 Jewish delinquent boys at the Hawthorne-Cedar Knolls School (the 500 matched pairs in the Glueck study comprised, largely, English-American, Italian, and Irish boys of Protestant and Catholic religions; only 10 Jewish boys were included<sup>67</sup>); one by Thompson on 100 boys of the well-known Cambridge-Somerville Youth Study<sup>68</sup> (involving differentiation of true delinquents from pseudodelinquents among children showing behavioral difficulties); a study in 1955 from the New Jersey State Department of Institutions and Agencies on 51 delinquents on parole; a study in 1956 by clinicians of the Thom Clinic for children (Boston) on 57 "antisocial young children"; a study by Thompson, 1957, comprising 50 boys who had been before the Boston Juvenile Court in 1950 and 50 girls committed by that tribunal to the care of the Massachusetts Youth Service Board; a study by Glick and Donnell of 150 young unmarried mothers; a study by Glick of 150 boys of a larger sample from upper-income families (\$7,500 and thereabouts), to which it had been originally planned to apply the table.

The typical outcome of these checkups is that in some nine-tenths of the cases (very occasionally a somewhat lower proportion) the Glueck table would have correctly identified the boys at a very early age as potential persistent delinquents or (where the study involved them) as potential nondelinquents.

<sup>66</sup> Glueck, E. T., *Efforts to Identify Delinquents*, *op. cit. supra* note 65a at 54.

<sup>67</sup> UNRAVELING, Table IV-6 at 38.

<sup>68</sup> These 100 were chosen, essentially, by a method intended to select a random sample from a much larger quantity of cases.

As suggested, there have been other retrospective checkups of the table both here and abroad.<sup>69</sup>

In 1956 it was applied by a psychiatrist to 100 boys, "chosen at random for a wide variety of behavior and emotional difficulties," who had been referred to the Thom Clinic, Boston. Of 31 boys previously diagnosed by the clinicians as of "anti-social character," all "without exception" had been placed by the Glueck table in the group with a high likelihood of persistent delinquency.

In Japan the first study applied the table to 30 delinquent and 30 nondelinquent boys and found that 87 per cent of the delinquents and 92 per cent of the nondelinquents were correctly identified; other inquiries there made, and some under way, are producing similarly favorable results, though specific figures are as yet unavailable.

In Strasbourg, France, application of the table to a group of 46 delinquents by trained psychologists (a pilot study to be followed by a more comprehensive one) showed that 91.4 per cent of the boys would have been correctly identified at the age of six. A second investigation in 1959, by the same group, under the oversight of Professor Jacques Léauté of the *Institut de Science Criminelle* of the Faculty of Law, involved 140 court delinquents and 63 boys brought in by parents for incorrigibility; 89.9 per cent of the first group and almost all of the second would have been correctly identified by the table as potential delinquents.

So much for the retrospective checkups.

(2) As to *prospective* studies, the most important one is that of the New York City Youth Board, originally set up in 1953 in two public schools in regions of very high delinquency rates. The table was applied to boys entering the first grade (usually at age six).<sup>70</sup> The boys have been followed for some seven years into a great many schools in New York City. On the 223 boys involved, the findings to date show that of 186 predicted at school entrance to be *nondelinquents*, 176, or 94.6 per cent, are still (1960) in fact *nondelinquent*—a remarkable confirmation when it is considered that these boys came from the same high delinquency areas as did the boys identified to be delinquent. Of 37

<sup>69</sup> References will be found in Glueck, E. T., *Efforts to Identify Delinquents*, *op. cit. supra* note 65a. Public Prosecutor Haruo Abe is preparing a comprehensive study of Japanese prediction experiments, including validations of the Glueck Social Prediction Table.

<sup>70</sup> The claim that tables based on boys around 14½ years old could not possibly be predictive of the behavior of boys of 6-7 was made, among others, by Rubin, *op. cit. supra* note 5 at 112; Reiss, *op. cit. supra* note 5 at 117; Burgess, *op. cit. supra* note 7 at 53-54; Monachesi, *op. cit. supra* note 36 at 57.

boys predicted as *delinquents*, 13 are already adjudicated delinquents and 4 more are "unofficial" offenders, making a total of 46 per cent—again a remarkable confirmation of a forecast when it is considered that the peak age of arrest for delinquency is in the middle and late 'teens and these boys are of an average age of only 12–13 years.<sup>71</sup> It is necessary to continue to follow up the careers of these youths at least until they reach 18, the juvenile court limit; but it would be astonishing indeed if the results of the seven-year follow-up were to be nullified by the five or six years the study still has to run.

A second checkup on future behavior is a preliminary report on the Maximum Benefits Project, begun in Washington, D. C., in 1954. Children selected for study and treatment were those who presented various classroom problems and whose teachers thought them to be in need of professional aid. The study involved 179 youngsters received over three academic years for behavioral difficulties. Application of the Glueck table to these children showed that only 21 of the 179 had scores less than 250, which we deem to be a satisfactory cut-off point for distinguishing between children unlikely, and those likely, to become delinquent; the remaining 158 (88.3%) had at least a 63.5 per cent chance (half of them an 89.2 per cent chance) of becoming delinquent.

In a follow-up study early in 1958, 58 of these children had already been to court or police for delinquent acts; and the Glueck score had been over 250 in all but one of these. It must be borne in mind that the checking is far from complete; and, as indicated, the maximum age of arrests is in the middle and late 'teens.

(3) It is true that the follow-up studies thus far made<sup>72</sup> deal with relatively small samples of cases; but they are all random samples and all of

them, without exception, support the original table. It can hardly be claimed that this consistent outcome, in such a variety of cases and places, is attributable to "the long arm of coincidence." Should later evidence nevertheless run counter to the record thus far established, we will be the first to make it known.

(4) It should be pointed out that the tables reflecting the experience in these checkups resemble *not* the adjusted tables of Reiss and others, but the original Glueck table. Thus, for example, the report by the New Jersey Department of Institutions and Agencies on delinquent boys on parole says: "It will be observed that the closeness of the findings on the basis of the New Jersey data with the original findings in the study of *Unraveling Juvenile Delinquency* is rather noteworthy, since the New Jersey boys were selected at random, and no attempt was made to match the individual characteristics of the New Jersey delinquent boys with the delinquent boys included in the Harvard Law School study."<sup>73</sup> The resemblance of the distribution of scores for samples of nondelinquents and delinquents in such a study as, for example, the New York City Youth Board's, suggests that, had the adjusted table of Reiss or other critics been used instead of the original, the utility of our table would have been seriously reduced, the predictions would have greatly miscarried, and there would have been many more "false positives" (a concept discussed below) than turned up through the use of the original table.

(5) On the basis of the checkup studies thus far made, we may now consider the first two questions posed at the outset of this section:

Does the Glueck table's distribution of scores for samples of *delinquents* reproduce itself elsewhere?

Nobody can read the evidence marshalled in the papers by Mrs. Glueck and too briefly summarized herein without acknowledging that the answer can already safely be *Yes*.

Does the Glueck table's distribution of scores for new samples of *nondelinquents* reproduce itself elsewhere?

Here, while it is true that only two of the validation studies so far carried out are prospective,<sup>74</sup> the

<sup>71</sup> In *UNRAVELING*, the average age of the delinquents at first court appearance was 12.4 years, and 25.8% were first summonsed at 14, 15 or 16.

<sup>72</sup> Several more follow-up studies have been called to our attention since the above was written. For a more recent summary of prediction validation studies in Japan which confirm the earlier findings in regard to the validity of the Glueck Social Prediction Table, see Juhei Takeuchi, "Juvenile Delinquency in Japan—Characteristics and Preventive Programs," a speech prepared for the Second United Nations Congress on the Prevention of Crime and the Treatment of Offenders, London, 1960, especially notes 24–32 (mimeographed). Those interested in the growth of Japanese prediction studies are invited to write to the Criminological Research Division of the Research Training Institute of the Ministry of Justice, 1–1 Kasumigaseki, Chiyodaku, Tokyo, Japan.

<sup>73</sup> STATE DEPARTMENT OF INSTITUTIONS AND AGENCIES, *PREDICTING JUVENILE DELINQUENCY*, Research Bulletin No. 124, Trenton, N. J. (April, 1955).

<sup>74</sup> The first Thompson study may also be deemed prospective, because it involved a follow-up of cases previously prognosticated by a Selection Committee as either potentially delinquent or potentially nondelinquent on the basis of study of case-history data.



evidence is already reasonably persuasive that the answer will again be *Yes*. After some seven years, of the 186 boys predicted as future nondelinquents though residing in the same high delinquency areas as the delinquents, 176, or 94.6 per cent, are still nondelinquent, a fact determined by intensive follow-up.

Before turning to the final question posed above, I should like to point out that not only do the various checkup experiments embrace a variety of potential and actual delinquency, ethnic derivation, economic status, religion, cultural background, and even sex, but—something especially pertinent to the claim that, to have predictive power, the table must be applied to a population similar in the *proportion* of delinquents to nondelinquents to that of the original table—the checkups also covered populations presenting a variety of proportions of nondelinquents to delinquents.

Thus it can reasonably be vouchsafed, on the basis of the empirical evidence so far adduced, that the Glueck table will tend to reproduce its distribution of scores for both delinquents and nondelinquents when it is applied to other populations.

The essential support of the table's pattern of factor-scores as related to behavior-outcomes by its application to a variety of samples elsewhere is very important to criminology; for as emphasized by the distinguished biostatistician, Professor Edwin Bidwell Wilson, "Science advances by broadening the base upon which empirical uniformities are established; indeed, it is only by this broadening that we come to know that the descriptions of our samples are uniformities."<sup>75</sup>

(6) This brings me to the third question involved in the issue raised by the critics: *To what extent is the Glueck table an "efficient" predictive instrument*; that is, to what degree does it improve on a "prediction" made by simply calling every boy in the community nondelinquent?

Since no prediction device can reasonably be expected to forecast accurately in *all* cases, there will inevitably be some "false positives," that is, some persons wrongly forecast as future delinquents and some wrongly spotted as future nondelinquents. To be sure, true prediction, especially in this field, cannot be 100 per cent accurate; but the Glueck table is indubitably much more correct

in identifying potential delinquents and potential nondelinquents than the method suggested by the critics which does not *identify* any child but supplies only the proportion of nondelinquents to delinquents that was estimated at the outset.

The "false positives" aspect of the problem is not a scientific one but an issue in social ethics and social policy. In this connection, it is very important to point out that mistakes in *not* spotting future *delinquents* can be very costly to society, while mistakes in assuming a few persons to be potential delinquents (10 out of the 186 in the New York study, for example) who nevertheless ultimately turn out *not* to be delinquents can do little harm and might even do considerable good. Professor Samuel Stouffer, a social scientist for whom we have high respect, has suggested that if, for example, the Glueck table accurately "spots" 17 out of 20 potential delinquents but does so at the cost of predicting, say, 18 nondelinquents to be delinquents,

"the answer might be that the 18 'false positives' may not be hurt by the extra close watching we give them, for many of them may really be borderline or incipient delinquents. Furthermore, in view of the specific social factors used in prediction, these kids may be really in need of some help *in loco parentis*. Hence, the answer from the point of view of social ethics might be that the social gain in spotting the 17 correctly more than justified any damage which might be done to the 18 spotted incorrectly. If everybody scoring above 250 were to be sent to an institution, society would scream. But if the kind of watchful and helpful treatment which the 'false positives' might get, along with the true delinquents would not hurt them or would actually be good for them, you would have a most convincing case."<sup>76</sup>

It should be added, first, that the New York City Youth Board's validation study still has several years to run, and if it proceeds along its present lines the chances are that the percentage of "false positives" will be very small indeed. Secondly, Professor Lloyd Ohlin, who has had both theoretical and practical experience with prediction methods, has rated the *efficiency* of the Glueck Social Prediction Table very high in comparison with others:

"An analysis of published results shows that the predictive efficiency of the experience tables

<sup>75</sup> Wilson, *Prediction*, 64 HARV. L. REV. 1040, 1041 (1951).

<sup>76</sup> Letter to authors by Professor Stouffer.

in follow-up samples varies from an efficiency of 25 per cent to a loss of 41 per cent in efficiency over what could be achieved with a simple prediction of success for all cases, a notable exception to these modest prediction results being the predictive efficiency of the Glueck Social Prediction Scale from *Unraveling Juvenile Delinquency* which has been computed by Ohlin as 55 per cent."<sup>77</sup>

(7) There is a related aspect of social policy to be considered—the question whether, assuming a high predictive potency of the Table, it is desirable to apply it in public schools as the basis of a prophylactic program, when this might entail “stigmatizing” a few children. It has been claimed that such a procedure would “label” such children as predelinquent and by that very process turn them into delinquents to live up to the role thus assigned them.<sup>78</sup> Apart from the superficial conception of child psychology revealed in such a dogmatic claim, it cannot be supposed that trained social workers would typically force themselves into a home and dramatically announce, “Your child has been predicted as a delinquent!”; nor would the child be told this. In the New York City experiment, neither teachers, nor parents, nor children have been told which boys are probably potential delinquents and which are not. If that experiment should be followed by a program of delinquency prophylaxis, it is presumed that trained social case workers will be entrusted with the job, and they can be expected to be more perceptive and tactful than to “stigmatize” a child as a predelinquent. It must be remembered, also, that, typically, the families involved are already the clients of social agencies for all sorts of problems other than delinquency.

If the argument of those who oppose the use of identification techniques to disclose which children are vulnerable were sound, we should logically close all of our child guidance clinics, dismiss our

<sup>77</sup> Report by L. E. Ohlin to Third International Congress of Criminology, London, 1955; GLUECK, S. & E. T., *PREDICTING DELINQUENCY AND CRIME* at 150, n. 5.

<sup>78</sup> “No magic formula exists for identifying boys and girls who will later commit delinquent acts except by stigmatizing as ‘pre-delinquent’ many who will never come before a court.” Report to the Congress on Juvenile Delinquency, U. S. Department of Health, Education, and Welfare, Children’s Bureau. It is submitted that in the light of the discussion in the text, the quotation expresses a superficial interpretation and an unwise social policy. We have never claimed any “magic formula.”

school counselors and visiting teachers, and sit back complacently (as, unfortunately, we too often do today) until the child has developed into a true delinquent or gang member and then haul him into court with the usual far from satisfactory result.

The choice presented to a community is whether its citizens prefer to let potentially delinquent children ripen into persistent offenders or to intervene, prophylactically, at a stage which gives the greatest promise of changing their dangerous attitudes and behavior, by aiding parents to modify their damaging disciplinary and nonaffectional attitudes and practices which have been found, in thousands of cases, to be potent influences in inclining children to delinquency. Our follow-up researches have consistently shown the tragic role of deep-rootedness in rendering antisocial behavior impervious to the usual methods of treatment thus far invented by society.<sup>79</sup> Finally, it should be pointed out that the application of the table might be limited to cases reported by teachers as presenting difficulties in class, in order to distinguish true delinquents from pseudodelinquents, or the services offered the parents of a child found vulnerable can be voluntary, not compulsory; but it is hoped that with the passage of time parents will learn to welcome aid in the behavioral field as they have in the medical.

#### CONCLUSION

I have attempted to give due consideration to all the major criticisms leveled at UNRAVELING JUVENILE DELINQUENCY.<sup>80</sup> Naturally, the authors

<sup>79</sup> In *PREDICTING DELINQUENCY AND CRIME op. cit. supra* note 48a at 82-83, it is shown that in 18 out of 30 prediction tables dealing with adult offenders and 3 of 10 concerned with juvenile delinquents, during all forms of sentence-treatment and for 15 years thereafter, the factor of *age at onset of antisocial behavior* had to be included as a differentiative indicator because of its strong predictive influence as between successes and failures. In all but four of the tables dealing with adult offenders, the *earlier the onset* of the delinquency, the higher the failure-score under one or another form of peno-correctional treatment and thereafter; or, to state it differently, the deeper the roots of childhood maladjustment, the smaller the chance of adult adjustment.

<sup>80</sup> There is a criticism by psychiatrists typified by the following statement, which I have not attempted to go into thoroughly because it would require a separate and lengthy paper: “A truly dynamic approach . . . recognizes that any individual’s behavior is the result of the interaction between internal and external forces, and can be understood dynamically not in terms of his membership in any class, but only in terms of his unique life situation—what he is reacting to and ex-

of that work are convinced, and I trust I have convinced the reader, that the theories advanced by the critics are essentially unsound. With ill-advised Olympian *hauteur*, two of our severest critics call attention to the literature on actuarial work which they claim we should have read.<sup>81</sup> In

pressing in his behavior." Satten, in *Symposium on the Gluecks' Latest Research*, 15 FED. PROB. 53 (No. 1 1951). There is some truth in this, of course; but there is also the danger of the dynamic psychiatrist "individualizing" quite inaccurately. To take account of all the influences of a single person's "unique life situation," is something that only God can do. The criticism under discussion has been largely directed against the use of predictive devices in aid of clinician and judge. Here I can only say that it springs from a misconception of both the aims of UNRAVELING and the prediction tables developed therein. It is of course not intended that either clinicians or judges should make their decisions mechanically, exclusively on the "odds" presented by the prediction device. The aim is not to substitute mechanical gadgets for either clinical or judicial expertise based on much experience but rather to furnish psychiatrist and judge an instrument reflecting *organized experience* based on the follow-up of numerous cases that have gone before. This is clearly superior to exclusive reliance on "clinical experience" or judicial "hunch." Nor does it counteract such individualization as is possible. As the Illinois experience with a prediction device used in parole has shown, "the net result of the use of the tables has been to challenge the application of mechanical formulas at every point and to force more detailed examination of the unique merits of the individual case." Ohlin, quoted in Glueck, S., *Prognosis of Recidivism*, Sec. IV GENERAL REPORT, THIRD INTERNATIONAL CONGRESS OF CRIMINOLOGY 19 n.14 (London, Sept. 1955).

<sup>81</sup> "But then there is a technical literature on the technique of prediction." Reiss, *op. cit. supra* note 5 at 119. "The relevant literature is so voluminous and lucid that ignorance can be no excuse for failure to meet criteria of acceptable research design. . . . It is supererogatory to publish an introduction to the book [PREDICTING DELINQUENCY AND CRIME] by the Chief

the light of the foregoing analysis, I trust they will forgive me if I too call attention to several bits of "literature."

First, I would call attention to the prescient judgment of Professor Edwin Bidwell Wilson: "A priori argument will not get far, howsoever it be extended. What one needs is trial and observation."<sup>82</sup> And in this connection, some advice on the relation of theory to fact, offered by two distinguished scientists of the past, are not without relevancy:

In the wise bequest left by the eminent Russian physiologist-psychologist, Ivan Pavlov, just before his death in 1936, he said:

"Perfect as is the wing of a bird, it never would raise the bird up without resting on air. Facts are the air of a scientist. Without them your 'theories' are vain efforts."

And the distinguished physiologist, Claude Bernard, laid down this safe rule to follow:

"When you meet with a fact opposed to a prevailing theory, you should adhere to the fact and abandon the theory, even when the latter is supported by great authorities and generally adopted."

Justice of the United States Supreme Court [sic] calling for 'unbiased consideration' of the authors' prediction tables on the part of 'people with open minds, minds that are open to accept or reject the thesis solely on reason.' " Duncan, *op. cit. supra* note 62 at 539. (Ah, we pure scientists!)

In the light of the analysis in the text, I believe I can confidently leave it to the reader to judge for himself with whom the charge of supererogation might more fairly be lodged.

<sup>82</sup> Wilson, Book Review, 64 HARV. L. REV. 1039, 1041 (1951).