



Delbert Elliott

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# Reflections on Six Decades of Research

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## Abstract

A brief autobiographical history is presented covering my 57-year career as a criminologist. I begin with my early childhood experiences, growing up during World War II, my undergraduate and graduate school experiences, and my early career years at San Diego State University and the University of Colorado, Boulder. I then discuss two of the major themes in my research developed during these early career years: self-report measures of delinquent behavior and the Integrated Theory of delinquency. My later career years are described, and the third major theme of my work, the identification and promotion of effective delinquency prevention programs, is discussed.

## INTRODUCTION

When I started my professional career in 1961, an assessment of the state of criminological knowledge was reflected in the widely accepted conclusion that there were no crime prevention programs or practices that worked (Martinson 1976, Romig 1978). However, over the course of my 57-year career, I have seen remarkable advances in criminological theory and research and the development of effective crime prevention strategies. This improvement in the criminological knowledge base recently led me to offer the opinion that we could now launch a national crime prevention initiative that could significantly reduce the national rates of delinquency and crime in the United States if the political will were there to undertake it (Elliott 2018).

This autobiographical review is primarily an account of three main themes in my work that I think contributed to this improvement in criminological knowledge and practice. But I also offer an autobiographical account of the major events and experiences over the course of my career, some of which are only indirectly related to my scholarly work but provide some insight into my career decisions and provide a context to this work. The major part of the article focuses on the development of my research and thinking relating to (a) self-reported delinquency (SRD) measures and their use, (b) advances in criminological theory, and (c) the identification and dissemination of evidence-based crime and delinquency prevention interventions.

## EARLY CAREER

### Growing Up in Southern California

I was born in 1933 in the middle of the Great Depression and grew up in Southern California in a suburb not far from Los Angeles. My father earned BA and MA degrees in education and worked as a teacher, school principal, and, for most of his career, as a district-level administrator. Not surprisingly, three of his four children chose careers in education: two university professors, a college librarian, and the outlier, a clinical psychologist in private practice.

Both my parents had a deep commitment to the Lord that continued throughout their lives, my father teaching and serving as an elder in the church and my mother directing the church choir. Going to church was a routine part of our lives. Their strong faith had a profound influence on my life. Those who know me well know that I too am a committed Christian, and my faith has influenced my career decisions throughout my life.

Shortly after the raid on Pearl Harbor and the start of World War II, when I was eight years old, my mother was killed by a drunk driver, and my father, younger brother, and I were injured in the accident. For the next four years, we lived with my paternal grandmother, a very devout, godly woman. I became very attached to her.

These were the World War II days and I remember the rationing, air raid sirens and blackouts, collecting scrap metal and tinfoil to contribute to the war effort, and watching the newsreels at the local theater describing the conduct of the war. One of my best friends in the neighborhood was relocated overnight to a Japanese internment camp. I never saw Sammy again.

My father remarried and we moved to a new community in Orange County, which required a change in schools and some major adjustments for this teenager. During my high school years, I lettered in football, basketball, and tennis. I broke my nose twice playing football; there were no face guards on helmets in the late 1940s. But my major interests were in math and musical performance. I sang professionally with a dance band for a short time and in a male quartette that performed professionally, e.g., at Knott's Berry Farm and on *The Betty White Show*. I graduated from Fullerton Union High School as Valedictorian in 1951.

I received a four-year, George F. Baker scholarship to Pomona College, the "Oxford of the West." I continued my interest in music, singing in the college choirs and directing the chapel

choir. But I dropped my plan to major in music when it required that I be proficient in playing the piano. I also struggled in calculus and abandoned my plan to major in math. By my junior year, I found my interest was in the social sciences and subsequently graduated with a BA and a major in Sociology. In the summer between my junior and senior years I got married, and my new wife and I moved into married student housing for that last year at Pomona.

After my freshman year, I worked as a substitute mail carrier on weekends and during most holidays. It helped that my uncle was the postmaster there, although in the beginning, some mail carriers thought I might be a spy. This was a great job, it paid well and I learned a lot about the post office mail carrier culture, their union perspective and political views, their thinking about sex, and their attitudes toward “rate-busters” and “Christmas Help.” After getting married, I worked evenings at a four-star restaurant, The Magic Lamp, as a waiter. The hourly wage was pitiful, but the tips were great. I had the privilege of waiting on Gregory Peck one evening. He was with a pretty young woman I did not recognize, and I remember he ordered a bottle of Lancers wine with dinner.

### **Graduate School: The University of Washington**

I entered graduate school in the Department of Sociology at the University of Washington, Seattle in 1955 with a graduate research assistantship with Professor George Lundberg and an ROTC commitment for two-years’ active duty and an additional two years in the US Army Reserve. I was allowed to defer my active duty for two years, then took a leave of absence and returned to complete my MA in 1960 and PhD in 1961.

The last two years I had a graduate teaching assistantship with my own lower-division classes. I was turned on to criminology by Professor Clarence Schrag, who had recently returned to the faculty after serving as the deputy warden at the Washington State Prison in Walla Walla. My training in research methods and statistics was primarily by Santo Camilleri, Herb Costner, and Sandy Dornbusch, the latter having recently published a basic statistics book that I memorized [*A Primer of Social Statistics* (Dornbusch & Schmid 1955)]. I received a Ford Foundation grant that funded my dissertation: *Delinquent Opportunity and Patterns of Orientation* (1960–1961).

### **San Diego State University: 1961–1967**

My first academic appointment was as an Assistant Professor of Sociology at San Diego State University (SDSU). During this time, I had two great office partners/colleagues. The first was Harwin Voss. Harv was an excellent editor and worked over my writing with no mercy. Together, we worked on a study of delinquency and dropout in San Diego schools, and he was a coauthor on several articles and the book reporting on our findings from that study. Harv accepted a position at the University of Kentucky in 1965 and was replaced by David Dodge, who kept me entertained and helped me through some difficult personal times. His laughter was contagious, and he graciously read and critiqued my work. But he too left before I did, accepting a position at the University of Notre Dame. My other close colleague was Aubrey Wendling, a University of Washington PhD a few years ahead of me, who was largely responsible for my going to SDSU. He graciously handled all the closing down issues of the delinquency and dropout study after I left. He was a kind, loyal, and supportive friend. I was promoted to Associate Professor in 1964.

### **University of Colorado: 1967–1973**

My position at the University of Colorado throughout my tenure was a unique dream job, half-time teaching in the Sociology Department and half-time research in the Institute of Behavioral

Science (IBS). I had more time to work on grants and funded research than do most professors. And I always had grant funding to cover 100% of my summer salary. My research thus had a heavier influence on my career than may be typical.

The IBS had a strong commitment to interdisciplinary research, and, in this context, I started to interact with members who had expertise in different academic fields and shared an interest in criminology or problem behavior. Dick Jessor had just published his book on the tri-ethnic study (Jessor et al. 1968), a longitudinal study with his first test of Problem Behavior Theory. Our interaction over the years to the present day has had a profound influence on my thinking. The IBS provided an interdisciplinary environment for research that was truly exceptional: a critical number of researchers with different academic training focusing on a particular societal issue; administrative personnel assisting on grant preparation and submission; a librarian who systematically searched for federal and state requests for proposals in areas being investigated by IBS faculty and assisted in locating and copying publications requested by researchers; and a dedicated computer science team to assist in data analysis.

These early years at the University of Colorado were very productive. I completed the book manuscript for the delinquency and dropout study and received new funding for several studies: a small grant from the National Science Foundation to analyze the sociometric data collected in the delinquency and dropout study, a National Institute of Mental Health (NIMH)-funded study of the effects of legal processing in the juvenile justice system on self-definitions, and a statewide population survey on alcohol and drug use (Colorado Division of Alcohol and Drug Abuse). In 1969, I was promoted to full professor. My first PhD students, James C. (Buddy) Howell and John Quicker, completed their degrees, both eventually achieving recognition as gang researchers. My teaching typically involved large lecture classes with 250–300 students and several graduate teaching assistants.

### **The Behavioral Research Institute: 1973–1985**

In 1973, I left the University of Colorado and established a private research organization, the Behavioral Research Institute (BRI). This move from a tenured position to a soft money organization was scary but was precipitated by both personal financial concerns and an increasing difficulty in doing survey research within the university. Paying respondents in cash at the completion of an interview was a huge problem for the university at that time. David Huizinga, Frank Dunford, Suzy Ageton, Barbara Morse, Wayne Osgood, Hart Weichselbaum, Brian Knowles, Shelly Canter, and Tim Brennan all served on the research team at BRI. Most were successful in getting their own grants and several served as co-principal investigators (Co-PIs) or investigators on the National Youth Survey (NYS). I was successful in securing several major grants during this 12-year period at BRI. Three were evaluation studies: a national evaluation of Youth Service Systems (Department of Health, Education, and Welfare), a national evaluation of the LEAA (Law Enforcement Assistance Administration) Diversion Initiative [Department of Justice (DOJ)], and an evaluation of the Oakland Youth Work Experience Program (Department of Labor). The National Youth Survey (NIMH) was also funded in 1975 and was continuously funded throughout my time at BRI.

While at BRI, I had the good fortune of serving on several national committees: the NIMH Crime and Delinquency Review Committee (1974–1975); the NIMH Crime and Violent Behavior Review Committee (1981–1986), serving as chair for the last three years; and the National Research Council Panel on Research on Criminal Careers (1983–1986). The grants being reviewed for NIMH at that time were huge; 100+ pages was not unusual. Preparing for these meetings involved a major time commitment, but the rewards were great. Colleagues on the committee

were skilled researchers, and I had a good view of the kinds of studies and research methods being funded. The work on the Criminal Career Panel, chaired by Al Blumstein, involved a major expansion of the conceptualization of delinquency and crime and significantly influenced my own subsequent research. These were great learning experiences with some of the brightest and most experienced criminologists.

Travis Hirschi appointed me as an Executive Counselor on the American Society of Criminology (ASC) Board in 1982. When I showed up for the first meeting, Travis said “You know, I appointed you to rescue you from obscurity.” This was the beginning of a good relationship for me. He was a critic of my work, but I profited from his insights and recommendations and we were always on friendly terms.

## University of Colorado: 1985–1996

I returned to the University of Colorado in 1985 as a Professor in the Department of Sociology and the Associate Director of the Program on Problem Behavior in the IBS. Most of the BRI staff came with me. The NYS was still in progress and Scott Menard joined the NYS team. I directed three other major grants during this period: an evaluation of the Ignition Interlock device as a deterrent to DUI (National Institute on Alcohol Abuse and Alcoholism); a Research Center grant from the Carnegie Corporation of New York; and a study of successful adolescent development in Denver neighborhoods (MacArthur Foundation). This latter study was one of three research projects completed by the MacArthur Network on Successful Adolescent Development in High-Risk Settings. My involvement in this network, chaired by Dick Jessor, with an amazing group of researchers and scholars, including Albert Bandura, James Comer, Thomas Cook, Jacquelynne Eccles, Glen Elder, Frank Furstenberg, Norman Garmezy, Robert Haggerty, Betty Hamburg, Arnold Sameroff, Marta Tienda, and William Julius Wilson, had a significant impact on my thinking and precipitated a significant change in the direction of my work. The network had a strong interdisciplinary approach, a focus on successful youth development, and a life course developmental conceptual framework. Bill Wilson and I were commissioned by the network to focus on neighborhood factors contributing to positive child and youth development and other members focused on the family, school, and rural contexts. The results of our study were reported in *Good Kids from Bad Neighborhoods: Successful Development in Social Context* (Elliott et al. 2006).

The Carnegie grant funded the Center for the Study and Prevention of Violence (CSPV) with a mission to bridge the gap between research and practice to foster research-informed prevention programs that are effective and promote them at the national and local levels. This required working with justice system agencies; legislative groups and foundations that funded crime prevention programs, practices, and policies; and the media that report on what works. I was not trained to do this kind of work, and testifying at congressional hearings and working with legislative staff and reporters was new ground. I learned fast that academic jargon and qualifying every generalization with five possible contingencies was frustrating for all these groups and largely ineffective. Although I learned to communicate more effectively, it is still frustrating that research evidence of effectiveness, even when effectively communicated, is often ignored or considered a low priority compared to political considerations and local “old boy” networks.

I served on the ASC Board from 1991 to 1994 and as ASC President in 1992–1993. The next year was particularly difficult, as I lost my second oldest son to cancer in October. There was much grieving but my IBS work continued. The program was staffed by many gifted graduate research assistants, professional research assistants, and research associates, including Jack Brigham, Finn Esbensen, Jane Grady, Jen Grotmeter, Larry Severy, and Bill Woodward. These were good productive years. I turn now to two of the major themes of my work.



## SELF-REPORTED MEASURES OF DELINQUENCY

### The Delinquency and Dropout Study

In my second year at SDSU (1962), I received a grant from NIMH to study delinquency and dropout in the San Diego Unified School District. This was a six-year prospective longitudinal study following all entering ninth-grade students in selected schools to their graduation four years later. The SRD measure we developed was based on the Nye & Short (1957) SRD checklist involving 21 items. We made two modifications to this measure: (a) We limited the items to officially recognized delinquent acts, and (b) we used a coding scheme to approximate frequencies from the Nye & Short categorical response set. Our resulting SRD measure involved 10 items and provided both categorical and frequency estimates of offending rates.

I thought we had a good working relationship with the school district based on a successful pilot study. However, a press release describing our study appeared in the San Diego Union newspaper, and, almost immediately, we were accused, primarily by members of the John Birch Society but also others, of collecting intrusive, sensitive information on these students. Members of the John Birch Society claimed that we were collecting information that could be used in the future by the DOJ and NIMH to identify students who opposed the government on any issue as criminals or mentally ill so they could be jailed or committed to a mental institution. A series of letters to the editor and investigative articles followed in local newspapers, and an article on the controversy appeared on the front page of the San Diego Union.

We were instantly infamous! The San Diego School District Board scheduled a formal hearing to decide whether the study should be allowed to continue. We were not allowed to attend the hearing, but there were multiple persons there who argued on our behalf. Carl Rogers, the eminent psychologist who was at the Western Behavioral Sciences Institute in La Jolla, was among our defenders. In the end, we were denied access to any schools in San Diego. In addition, we were formally investigated by the San Diego County Grand Jury and had to report to them annually for each of the next two years. I was off to a great start on my first major research project and first SRD measure.

Fortunately, the Sweetwater Unified School District, a district south of San Diego, reviewed our protocols and procedures and approved the study with two changes in our SRD measure. We agreed to drop the sexual activity item and limit the collection of SRD data to the first and last of four data collection waves (Elliott & Voss 1974).

### The National Youth Survey Self-Reported Delinquency Measure

The NYS was initially funded by NIMH as a five-wave representative national youth panel of 1,725 American youth born between 1959 and 1965 (ages 11–17). The first wave of this panel study provided self-reported delinquency and drug use data for 1976. Subsequent grants (NIMH, DOJ, National Institute on Alcohol Abuse and Alcoholism, Behavioral and Social Science Research Division) funded waves 7–9 on a three-year cycle and then waves 10–12, the final wave occurring in 2005.

We made a substantial number of modifications to the SRD measure developed for the delinquency and dropout study. Our first concern was with face validity, and we decided to include the full range of delinquent acts for which youth might be arrested. We included items for all Uniform Crime Report (UCR) offenses that accounted for more than one percent of juvenile arrests between 1972 and 1974, including all Part I offenses except homicide, 75% of Part II offenses, and a wide range of Other offenses. The measure included 47 delinquent offenses and 15 common drug substances, including alcohol and tobacco. We attempted to minimize the risk of item overlap and

double counting with precise descriptions of each offense as defined in the UCR, pretesting this set of definitions with students and teenagers known to the research team.

We were particularly concerned to address the seriously truncated frequency estimates derived from the typical normative categorical response sets used in many studies. The delinquency and dropout study had suggested that social class differences varied by the type of SRD response set used. We included two response sets, an open-ended raw frequency count and a set of follow-up categories (once a month, every 2–3 weeks, once a week, 2–3 times a week, once a day, and 2–3 times a day) to assess the regularity of these offenses if the raw frequency response was 10 or greater. These response sets were designed to provide better discrimination at the high end of the frequency distribution, alternative estimates of the actual number of offenses committed, and an estimate of the overtime patterning of an offense. In our epidemiological analyses, we used the open-ended frequency responses and, in theory, tested the categorical responses, as they had better distributional characteristics.

For the first five waves, the recall period at each wave was the past calendar year. For waves six through nine, the recall period was the past three years, but with the use of some innovative bounding techniques, we obtained annual estimates during these three-year periods. Waves 10–12 used one-year recall periods. There were four primary SRD scales used in most analyses, a General Delinquency Scale with 31 items, an Index Offense Scale with 9 items, a Problem Alcohol Use Scale with 8 items, and a Problem Drug Use Scale with 16 items. All four had acceptable psychometric properties (Huizinga & Elliott 1986).

Our experience with these scales over the first three waves led us to question the relatively high frequencies reported for some types of offenses, primarily misdemeanors but also for reports of numerous felonies. Our concern was that the face-to-face interviews might be creating a response demand that led to overreporting or that overlapping items might be leading to double counting. Starting with wave four, we added detailed follow-up questions to items when the respondent reported more than one event. The interviewer went through the entire set of items and then back to ask follow-up questions about the first three events reported for an item. For example, for aggravated assault, the follow-up questions included what type of force did you use, was a gun involved, was some other weapon used, was the person hurt and how badly, what was your relationship to the victim, were others with you involved in the assault, had you been drinking or taking drugs before the incident, and did you report this same event for any other question. Although we were not the first to use follow-up questions, this was a much more detailed set of questions on a broader set of offenses than previously used.

This follow-up information proved very useful. For offenses in the Index Scale, nearly all reported offenses were captured in the follow-up questions. This was less likely with the misdemeanor offenses in the General Delinquency Scale. We adjusted frequency estimates for each respondent for inappropriate events, trivial events, and double counting. We were able to estimate the frequency and type of weapons used in various types of crime, drug and/or alcohol use prior to events, and whether and how often others were involved in a reported event.

This SRD measure was unique at that time. It addressed many of the critical concerns about self-reported measures: better representation of the conceptual domain of offending, estimates of both raw frequencies and categorical responses that captured the range of frequency and temporal patterns of offending, an assessment and adjustment for item overlap, adjustments for inappropriate or trivial reported behaviors, and information about numerous details of offense events like weapons use, injury, number of persons involved, and type of vehicle stolen (Elliott & Huizinga 2014).



## The Reliability and Validity of Self-Reported Crime Measures

In 1981, Hindelang, Hirschi, and Weis published *Measuring Delinquency*, a study that is widely credited for establishing the reliability and validity of SRD measures (Hindelang et al. 1981). Although I agreed with most of their conclusions and recommendations for improving self-report measures, I took exception to two specific conclusions (Elliott 1982). One was their assertion that the reverse record check indicated that the validity of Black male self-reports was marginal and that Black males underreport their involvement in delinquent behavior. This conclusion assumed that official records were irrefutable and any discrepancies between self-reports and official records indicated an error in the accuracy of self-report data. I thought this assumption questionable based on my own experience with official records and existing research on this issue.

The other concern was with their recommendation that an ever-variety measure was the preferred SRD measure. They did acknowledge that this measure had little value in longitudinal research, which is not a minor limitation as most etiological and experimental research use this design. But this is also a limited self-report measure for epidemiological studies when a comparison is being made to a UCR arrest rate measure, which is an incident measure. In fact, I think an ever-variety measure is one of the least useful measures for criminological research.

A few years later, David Huizinga and I reported on a more comprehensive analysis of the reliability and validity of SRD measures (Huizinga & Elliott 1986), reviewing all available studies of reliability and validity and covering the same issues addressed earlier by Hindelang et al. (1981) as well as additional ones. We reported that internal consistency measures of reliability are problematic for most types of SRD measures and recommended that test-retest measures are more appropriate. Although test-retest reliabilities in the eighties and low nineties (Pearson correlation coefficient) are generally accepted as demonstrating reliability, we noted the level of precision required varies by the type of analyses being done and this level may not be adequate for some analyses.

We reviewed two types of content validity (face validity and sampling validity) and empirical or criterion validity (known group and correlational validity). We also reviewed reverse official record checks as a measure of validity. The NYS evidence for differential subgroup validity in reverse record checks indicated overall levels of underreporting did not differ by sex or social class. The evidence for differential validity by race was mixed, and we concluded that the validity of SRD scales as determined by this criterion remains in question and the lack of any good criterion was a major obstacle in assessing the validity of self-report measures. Finally, we suggested that the quality of SRD measures should not be taken for granted, as the reliabilities and validities are not so high that these measures should be used without question. This report on the reliability and validity of self-report measures was the most comprehensive, detailed, and up-to-date assessment at the time.

In my ASC Sutherland Award Presentation (Elliott 1995), I claimed that juvenile contact records and arrests are appropriate and reasonably reliable and valid measures for studying the activity of criminal justice agencies, but they are problematic when used to describe patterns of criminal behavior, characteristics of offenders, and their patterns of offending. It was distressing that a large segment of the contemporary criminal research community was doing just that. I compared both participation and individual offending rates for index offenses by race and sex using UCR and NYS data; considered the “worst offender” hypothesis, that is, that self-reports and arrests identify the same people as the worst offenders; and compared offense patterns, evidence of specialization, and criminal career length by type of measure. I found race and gender differences were less pronounced in self-reported estimates of participation and offending; there was little evidence of specialization; the worst offender hypothesis was soundly rejected; and self-reported

careers started earlier and lasted longer than found in official arrest records. I think these analyses provided some important clarification of the differences in official and self-report findings.

I also examined the probability of arrest given reported involvement in an index offense between 1976 and 1990. Black males were approximately six times as likely as white males to be arrested for a self-reported robbery or aggravated assault, controlling for weapons used, injury, and other characteristics of the offense. Moreover, less than 10% of the most serious violent self-reported offenders in the NYS sample were ever arrested for a serious offense.

I was arguing not that we discard or abandon the use of officially generated crime measures but rather for a more appropriate use and interpretation of such data. Nor did I claim that self-report measures are error-free and have greater validity than official measures (Elliott & Huizinga 1989, Huizinga & Elliott 1986). But it seemed clear in the NYS sample that self-report and official measures of delinquency provide substantially different pictures of the onset, developmental course, offending patterns, specialization, length, and termination of criminal careers and that conceptually and operationally, self-reports are more appropriate measures for studying the causes of criminal behavior and describing the distribution and dynamics of criminal behavior in the general population (Elliott 1994).

When concluding my address, I found Al Blumstein waiting for me. I was not surprised. We agreed to disagree on the appropriate uses of official records and limitations of self-report measures. We had a similar discussion earlier while serving together on the National Research Council Panel on Research on Criminal Careers. I think we still disagree on this issue but respect each other's viewpoint.

## **Other Features of Self-Reported Measures of Delinquency**

We reported on a detailed analysis of several other self-report measure issues (Elliott & Huizinga 1989, Huizinga & Elliott 1986). These included item content and scale construction issues, sampling validity and item specificity, the logical overlap of reported offenses, reference/recall periods, response sets and scoring procedures, and methods of administration. We concluded that over-reporting was likely to be more of a problem with SRD scales than underreporting; reporting periods greater than 12 months seriously underestimate prevalence and incidence rates; and there was no evidence of differences in estimates from telephone and face-to-face interviews. We recommended strategies for improving content and sampling validity and item specificity. I suspect these issues are not yet settled and further, more refined work on these characteristics of self-report measures is needed.

## **Summary: Self-Reported Measures of Delinquency**

The SRD measure developed for the NYS was unique in many ways: It was constructed as a parallel measure to the UCR with good face and sampling validity; included a more detailed set of follow-up questions than previously used, allowing for more accurate estimates of rates; provided new analyses of scale validity, item triviality, and overlap; and provided estimates of the accuracy and reliability of variable lengths of recall and the validation of open-end high-frequency responses. We published national estimates of the annual prevalence and incidence of delinquent behavior in the *Sourcebook of Criminal Justice Statistics* for the years 1976–1987 (Elliott et al. 1986, Elliott & Huizinga 1988) and the full set of offender and offense measures proposed by the NRC Panel on Research on Criminal Careers in journal articles (Dunford & Elliott 1984, Elliott 1994, Elliott et al. 1989, Esbensen & Elliott 1994). I summarized the research on self-reported delinquency surveys for the Oxford Bibliographies in Criminology (Elliott 2017) and continue to get requests from the United States and around the world for information about and permission to use these

SRD scales (they are in the public domain). To my knowledge, they have been translated into at least six languages.

There is still a need to improve self-report measures. Our ability to establish temporal sequencing is still limited when using annual recall periods. The use of mobile media, especially text messages, to obtain monthly reports should be explored. I know both these approaches have been tried but am not aware they have been evaluated or widely adopted by criminologists. I also think the use of detailed follow-up questions should become a standard component of self-report measures, but to this point that has not happened. And we should be collecting national self-report rates for the full range of offenses, like we do for national victimization reports, to provide a better understanding of the changing nature and dynamics of delinquent behavior in the United States.

## **DELINQUENCY THEORY**

### **Delinquency and Dropout Study**

From the beginning, I was convinced the explanation of delinquent behavior was more complicated than proposed in current theories at that time. Cloward & Ohlin (1960) had just published *Delinquency and Opportunity*, and I thought this model integrating elements from strain and social learning theories was an important innovation. This theory was our starting point for the theoretical model developed for the delinquency and dropout study. There were three innovative ideas in our proposed model. First, the focus on limited opportunities was extended to a wider range of conventional goals. Second, the goal-means disjunction was modified to be logically independent of social class. Third, the role of social learning in the development of delinquent behavior was further emphasized.

This study informed my theoretical thinking in several ways. First, we found no substantive support for the classic strain formulation involving anticipated failure to achieve long-term societal success goals such as educational or occupational goals. There was support for more immediate types of failure, e.g., academic failure and parental rejection, but not for failure to achieve peer culture goals. Second, we found a strong commitment to peers was predictive of delinquency whether the group was involved in delinquency or not. Finally, there was support for at least some predictors from all three theories.

### **The National Youth Survey**

The development of the theoretical model for the NYS was driven by three other concerns. First, I was convinced that there were multiple causes for involvement in delinquent behavior. It may be each causal path is predicting a different type of offense or, more likely, there are multiple paths to delinquent behavior in general. Second, I questioned whether the competitive hypothesis approach (Hirschi 1979) to theory building and verification was paying off. Theories rarely provided competing hypotheses that were testable, and when examined crucial tests were seldom definitive. Moreover, specific predictor variables were not unique to a given theory, undermining theoretical independence and any interpretation of direct comparisons. And there are logical problems viewing two different hypotheses as simple alternatives to one another and presuming that the acceptance of one implies the rejection of the other. Finally, I thought the levels of explained variance reported in tests of these independent theories were embarrassing. Moreover, nearly all tests involved cross-sectional studies without any control for prior delinquency and the wrong temporal order in the analysis. In rare studies involving longitudinal designs with controls for prior delinquency, the explained variance was not statistically significant (Elliott 1985). The

value of these separate theories for establishing criminal justice policy or developing prevention interventions was minimal.

In 1979, we published a description of the theoretical model guiding the NYS study, along with comments on the model by Jim Short and Travis Hirschi (Elliott et al. 1979, Hirschi 1979, Short 1979). I called this theoretical formulation an Integrated Theory, a theory that combined elements of strain, control, and social learning theories, building on the model examined in the delinquency and dropout study. These three theories were primarily attempts to explain the onset of delinquency but with the increasing availability of longitudinal data, it was now possible to consider developmental models proposing time-lagged sequences of variables and multiple paths to delinquency. Furthermore, the improvement in self-reported delinquency measures provided for tests of the theory with the full range of delinquency outcomes such as prevalence, individual offending, onset, specialization, continuity, and termination. These were the goals of the integrated theoretical model proposed.

I am basically a control theorist. It reflects my theological understanding of human nature and logically can be expanded to cover many of the variables included in strain and learning theories. The model described multiple sequences of variables covering the early socialization experiences in childhood, subsequent experiences associated with the onset of adolescence and involvement in school and peer group contexts, and the later transition into early adulthood. We claimed that there are several advantages to Integrated Theory. First, the model assumed the two primary alternative paths were independent and additive and that their combined effect would account for more variance in delinquency than either alone. Prior tests of strain and control treated these two paths as competing hypotheses and compared their relative strengths, missing the possibility that both are correct and account for different portions of the explained variance in delinquency. Second, we hypothesized that the traditional strain path was a path that was less likely to involve delinquent peers, less likely to be sustained into adulthood, and more likely to involve working- and middle-class youth. This offered a potential explanation for the weaker relationship between social class and delinquent behavior during the teenage years, as observed in self-reports compared to official arrest measures of delinquency.

In our first full test of Integrated Theory (Elliott et al. 1985), we described it as a modified social control theory. Both the concepts and terminology are now those of control theory, although the same predictors we earlier described as strain or social learning predictors are now conceptualized as social control predictors. Later, the model was cast in a life course developmental framework but with no substantive changes in its basic structure (Elliott & Fagan 2017).

Short's (1979) primary comment on Integrated Theory was that the integration was limited to the individual level of explanation. He was correct, but it was a start. Hirschi's concerns were more difficult to address. He described and critiqued three general approaches to theory integration: organizing theories end-to-end, side-by-side, or up-and-down. From my perspective, the most serious limitation noted by Hirschi was that in an end-to-end integration, the final theoretical predictor captures all the explained variance of the earlier theoretical predictors. Thus, there can be no increased explanatory power from the integration beyond the simple bivariate association of the last predictor with delinquency and "...all theories but the last one in the sequence are wrong" (Hirschi 1979, p. 35).

Our integration was not a simple end-to-end integration, but this argument was relevant to the dominant causal path we proposed that ended with involvement in a delinquent peer group. I found surprising his argument that a variable (or theory) in a causal chain has no causal influence and is "wrong" unless it is the most proximate cause. I offered two responses to this assertion (Elliott 1985). First, I agreed that in a pure sequential model the distal predictors add nothing to the predictive power of the most proximate predictor. But the fact that they do not contribute any

additional predictive power to that of the proximate predictor does not negate their contribution to our understanding of early causes or their role in the process leading to the outcome. Moreover, these distal predictors have an important role in the development of prevention and treatment programs. They identify the conditions and experiences to target for early interventions to prevent the onset of delinquency. Second, in our primary causal path, the most proximate predictor was a conditional one, dependent on the preexisting level of bonding to conventional values and groups. An interaction effect between this predictor and the delinquent peer group predictor was expected. In this type of end-to-end sequence, the explained variance of the combined direct effect and interaction effect can be significantly greater than the direct effect of the most proximate predictor alone. This proved to be the case in our tests of the model.

The general thrust of Hirschi's comment was to question the integrating strategy for theory development and testing and our belief that integrated models would significantly increase the levels of explained variance over that found with tests of independent theories. He clearly preferred the competitive strategy of theory development and testing. In a review of other tests of integrated or mixed theory models, I found these models typically demonstrated an increase in explained variance over that of the separate theories included in the integration, sometimes a relatively small increase and in some cases a more substantial increase (Elliott 1985). Our tests of the integrated model demonstrated significant increases in explained variance over the strongest single theoretical component effect or the sum of the unique effects of different theoretical predictors (Elliott et al. 1985, 1989). Although I have not done a recent review of the findings from other tests of Integrated Theory with prospective longitudinal data, I think they have generally reported similar findings (Elliott & Menard 1996, Esbensen & Elliott 1994, Menard & Johnson 2015, Menard & Morris 2012, Rodriguez & Weisburd 1991).

### Summary: Delinquency Theory

The critical question is whether the promise of better levels of explained variance was confirmed with this integrated theoretical model. In our tests of general offending rates, the levels were 1.5–2 times higher than most cross-sectional tests of individual theories and much greater than tests with longitudinal data controlling for prior delinquency that were published prior to 1985 (Elliott 1985). But it is still not very high for serious offending and that was disappointing.

The evidence for multiple causal paths was weak for the direct delinquent peer group bonding path, and the interaction between delinquent peer group bonding and conventional values and group bonding accounted for almost all the explained variance. The causal effect of the other predictors in the model was indirect through these two direct predictors. But these improved levels of explanation and multiple causal paths played an important role in the development of some effective delinquency prevention programs. There was no evidence that different causal paths were related to different types of delinquency.

Integrated Theory is routinely described and critiqued in criminology texts and books on criminological theory. Ours was not the first attempt to combine theories, but most of the other attempts were mixed theoretical models in which there was no attempt to reconcile the different assumptions of the combined theories. I believe the publication of Integrated Theory with the comments by Short and Hirschi led to an important discussion and debate about strategies for developing theory and stimulated the development of other integrated models, e.g., Network Theory (Krohn 1986), Interaction Theory (Thornberry 1987), and the Social Development Model (Hawkins & Weis 1985). I believe the integration approach to theory development may now be as or more frequently used than the competitive hypothesis approach. I reviewed the full variety

of models attempting to integrate or combine different theories in the Oxford Bibliography on Integrated Theory (Elliott 2012).

## **LATER CAREER YEARS: UNIVERSITY OF COLORADO 1996–2018**

As noted above, I changed the direction of my career in the mid-nineties. It was precipitated by a growing personal desire to be more directly involved in applying the growing body of research on the causes of delinquency to the practical problem of preventing delinquent behavior in our society. This new work started with funding for the Center for the Study and Prevention of Violence described above. A new opportunity came in 1996 with funding to establish a registry of effective violence prevention programs for juveniles: the Blueprints for Violence Prevention Registry. The story of the development and history of the Blueprint Registry is described below.

Shortly after the Columbine High School tragedy, I traveled with the Colorado Attorney General, Ken Salazar, to every county in Colorado, meeting with junior and senior high school students in general assemblies and smaller groups, discussing the many issues raised by the mass shooting. I was granted the opportunity to review the interviews conducted by the Jefferson County Sheriff's Department in their investigation of the tragedy and met with the parents of the victims. I also met several times with the parents of both Eric Harris and Dylan Klebold, discussing the possibility of a series of in-depth interviews with them. Unfortunately, both families decided not to participate, for quite different reasons. Then Judge Lewis Babcock ordered the Basement Tapes, videos by Harris and Klebold talking about their upcoming attack, and other critical documents involved in this case sealed for 20 years. My involvement in the Columbine tragedy ended.

Between 1990 and 2002, I served on the CDC Advisory Committee for Injury Prevention and Control and the Science and Program Review Subcommittee. I assumed the responsibility of the Senior Science Editor for the US Surgeon General's report on youth violence in 2000. The immediate impetus for this report was the Columbine High School tragedy. This report was mandated by the Clinton Administration and Congress, sponsored jointly by the CDC, NIMH, and the Center for Mental Health Services (CMHS), and had a very short timeline, with a release date of January 21, 2001. It was an exciting project and we had a talented team of researchers and writers. The managing editor, Captain Norma Hatot of CMHS, was a skilled project manager and a supportive ally. The decision to identify specific evidence-based violence prevention programs in the report was controversial but, in the end, supported by the Surgeon General. I came to have great respect for Dr. David Satcher. I was awarded a Public Health Service Medallion for Distinguished Service in 2001; a Research to Practice Award from the Society for Prevention Research in 2000; and the August Vollmer Award from the ASC in 2003.

I resigned as IBS Director of the Program on Problem Behavior in 2004, and Terry Thornberry assumed this position. The program grew under his leadership; he made a major contribution to the ongoing criminological work in the IBS and our success in securing grants. For me personally, he became a trusted colleague. Unfortunately, he was lured away to the University of Maryland in 2009, and I reassumed the directorship of the program until 2017 when Karl Hill became the director. I was also named a Distinguished Professor in 2004.

I resigned from my position as Director of CSPV in 2012, and Beverly Kingston became the new director. This was one of the best decisions I have made. The CSPV has flourished under her leadership. We received one of the Academic Center of Excellence for Violence Prevention grants (CDC) in 2011, and I turned this major project over to Beverly as the principal investigator in 2013. In 2016, I teamed up with Abby Fagan for a textbook, *The Prevention of Crime* (Elliott & Fagan 2017). Finally, I resigned from the Blueprints Board in 2020. I turn now to a discussion of a third major theme of my work.

## REGISTRY OF EVIDENCE-BASED DELINQUENCY PREVENTION INTERVENTIONS

### Background

In the mid-1990s, I had a grant to provide evaluation technical assistance to state-funded delinquency, drug use, and dropout programs. It was an impossible task given the way the grant awards were structured. The members of the state board that reviewed and approved these grants were appointed by the governor and the president of the state senate. Programs that were successful in obtaining funding were a mixed bag of interventions. A few had some research-informed intervention strategies but most did not. My interaction with the board about what kinds of programs should be funded, what criteria should be used to make decisions about potential effectiveness, and how to obtain a meaningful evaluation of program effectiveness was instructive. The research-to-practice gap was huge. I got the idea of making a list of delinquency prevention programs that had some evidence of effectiveness for the board. The idea of providing a registry of programs with proven effectiveness in preventing violent behavior to assist funder decision-making was born.

### Blueprints for Violence Prevention Registry<sup>1</sup>

The initial task in developing a registry of effective programs was to establish a credible standard for certifying programs as effective prevention interventions. The minimum standard we initially considered was (a) a strong research design, a quasi-experimental (QED) or randomized controlled trial (RCT) design with good internal validity; (b) evidence of a significant deterrent effect; (c) an independent replication of that effect in a second quality QED or RCT study; (d) evidence that the effect was sustained for at least one year post intervention; and (e) evidence that the deterrent effect was mediated by change in the targeted risk or protective factor(s).

I was dreaming. In 1996, we reviewed over 500 violence, delinquency, and drug programs and none could have qualified by this standard. We dropped the mediating effect criteria, dropped the requirement that one replication had to be by an independent evaluation team, and kept the other four as the minimum standard for a model program ready for scale-up. We found 10 programs that we judged to qualify as model programs. We then contacted these programs to see whether they would work with us in developing a comprehensive description of the program with details on its theoretical rationale, logic model, implementation protocols, training and technical assistance provided, and a description of evaluations and their findings. This information was then published as a Blueprint booklet for each program. This work with developers evolved into an additional standard element, i.e., that the program must have the organizational capacity to actually deliver the program with fidelity simultaneously in multiple sites. We called this standard “dissemination readiness.” We published the first of 10 Blueprint Model Program books in 1997. The Blueprints for Violence Prevention Registry (hereafter Blueprints Registry) with all 10 model programs was online in 1998.

By 2004, two model programs were dropped as additional evaluations failed to confirm positive effects, and two new programs were added to our list of model programs. There now were other registries, all sponsored by federal agencies: the Center for Substance Abuse Prevention (CSAP), the Department of Education (DOE), the National Institute of Drug Abuse

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<sup>1</sup>Initial funders for Blueprints included The Colorado Division of Criminal Justice, the Centers for Disease Control and Prevention, and the Pennsylvania Crime Commission. Later funders included the Office of Juvenile Justice and Delinquency Prevention (OJJDP), the Annie E. Casey Foundation, and the Laura and John Arnold Foundation (now Arnold Ventures).



(NIDA), and the CMHS. These federal agency registries collectively listed 92 model, exemplary, or effective programs. In most cases, a single QED evaluation was sufficient to make one of these lists. We were criticized for having a standard that was too high and certifying too few programs.

In August of 2004, I was contacted by Terry Donahue of the National Institute of Justice (NIJ) to conduct a feasibility study of the Office of Justice Programs' (OJP) What Works Repository. This repository was designed by the Federal Collaboration on What Works, a committee convened by the US Assistant Attorney General in response to recommendations from the White House Task Force for Disadvantaged Youth and the Coalition for Evidence-Based Policy, which called for a common federal standard for assessing the effectiveness of youth interventions. This committee was composed of top officials from the OJP, Substance Abuse and Mental Health Services Administration, Department of Health and Human Services (DHHS), NIJ, and DOE. I consulted with the Working Group of the Federal Collaboration on What Works (hereafter, Working Group) as they developed this standard. I was skeptical of what would come of this federal bureaucratic effort to set a scientifically credible standard for judging program effectiveness.

The Working Group's proposed federal standard involved a three-level rating: Effective, Effective with Reservations, and Promising (Work. Group Fed. Collab. What Works 2005). The criteria for an Effective rating involved a quality RCT with statistically significant behavioral effects, evidence that this effect was sustained for at least 12 months post intervention, and at least one independent quality RCT replication. An Effective with Reservation rating did not require the replicated RCT to involve an independent evaluation team but was otherwise the same as for an Effective rating.

I was stunned. I loved this standard and have been trying to sell it at conferences and in my books and journal articles and to the Blueprints Board. To date, I have been unsuccessful. More critically, none of the participating federal agencies on the collaboration formally adopted it. Subsequently, we added a program rating on the Blueprint Registry called Promising. The minimum standard was one quality RCT or two quality QEDs and dissemination readiness. No replication or sustained effect is required. But we clearly warned registry users that we were not recommending promising programs for widespread adoption but as local programs that would need to be evaluated with replication.

## **Blueprints for Healthy Youth Development Registry**

Over the years, many important changes have taken place in the Blueprint Registry. In 2010, the name of the registry was changed to the Blueprints for Healthy Youth Development, reflecting the broader range of prevention outcomes now included in the registry. These include mental and physical health, educational achievement, and other positive youth development outcomes. The certification standard for a model rating was also upgraded. Model programs now require at least one quality RCT, a quality replication RCT or QED, sustainability for at least 12 months, no iatrogenic effects, and dissemination readiness. This change required we go back and reassess the evidence for model programs certified earlier and resulted in some downgrading of specific programs from model to promising and upgrading from promising to model.

In 2019–2020, we made additional changes to the registry, in part as a response to a critical study of the “what works marketplace” (Neuhoff et al. 2015) that found available registries were not being used in any systematic fashion by decision-makers when selecting prevention programs. Registry users wanted information about all available programs, including those lacking strong evidence of effectiveness; more contact information about programs, especially about peer experience with a program; and information about best practices as well as individual programs.

The new 2020 Blueprints Registry included several upgrades. First, the registry is now called the Blueprints for Experimentally Proven Programs. The term “evidence-based” is ambiguous with respect to the type of evidence and is no longer used. Second, fewer than five percent of programs reviewed met our standards for certification as a model or promising program, so the registry now uses an expanded classification of prevention programs that includes noncertified (i.e., not meeting Blueprint standards) as well as certified programs. Noncertified programs are rated as (a) insufficient, i.e., having no credible evaluations; (b) inconclusive, i.e., having limited experimental evidence; or (c) having strong experimental evidence but ineffective, harmful, or not dissemination ready. This classification is similar to that proposed by the Working Group cited above. In addition, the reason each intervention received this rating is provided. Third, the registry now includes ratings for prevention practices and policies as well as programs. Fourth, programs following adolescents with services into their adult years are now included.

I was blessed with an outstanding Blueprints Advisory Board that reviewed all the research evidence for an intervention and decided whether it could be certified as model or promising. The original members were Denise Gottfredson, Peter Greenwood, Hope Hill, Mark Lipsey, Patrick Tolan, and myself. Tom Cook joined the Board in 2000 and over the years Abby Fagan, Frances Gardner, David Hawkins, Larry Hedges, Karl Hill, Velma McBride Murray, and Robert Slavin all served on the board. Our board meetings were in person, similar to the DHHS grant review process, with primary and secondary reviewers, then discussion and decision by the whole board. These meetings were some of the most exciting, challenging, and intense learning times of my entire career. The Blueprint project staff was exceptional, with Sharon Mihalic directing the day-to-day operation of the initiative and Fred Pampel overseeing the initial reviews and screening for a second review by the Blueprints board.

The Blueprints Registry was the earliest regularly maintained online registry that systematically searched for crime and delinquency programs. The National Registry of Effective Prevention Programs (NREPP) started at the same time as Blueprints but did not systematically search for delinquency and crime prevention programs. It was suspended in 2017. Currently, there are as many as 20 other public or private online registries in the United States, but many do not systematically include delinquency or drug use prevention programs. The two DOJ registries, CrimeSolutions (<http://www.crimesolutions.gov>) and the Model Programs Guide (<http://www.ojjdp.gov/mpg>), were launched in 2011 and 2000.

## **Blueprint Conferences**

Sharon Mihalic, Director of the Blueprints Project, conceived of having biannual conferences to bring together developers of prevention programs, evaluators of programs, delinquency program service providers, and potential funders to showcase Blueprint Model Programs and exchange ideas and information about preventing delinquency and crime. This was a risky proposition for us, as we had no help from the university and had to have at least 600 paid reservations to break even. The first Blueprints Conference was held in 2006 in Denver with 1,000 registrations. These conferences have been a huge success and continued every other year until 2020 when the conference was canceled because of the coronavirus pandemic. They drew a wide range of participants: researchers, service providers, private and public funding agencies, program developers, and a significant number of international participants.

## **Summary: The Blueprints Registry**

I think the Blueprints registry is unique among registries that systematically search for delinquency prevention programs. It has the highest scientific standard for certification as an effective

intervention. This standard is the closest to that recommended by the Working Group among the registries listing crime and delinquency prevention programs. I know that is not a positive characteristic for some. I have debated this issue with many, including Howard Spivak and others who manage the OJP and Office of Juvenile Justice and Delinquency Prevention (OJJDP) registries, who believe the DOJ has a responsibility to give a wider set of recommended program options to local justice agencies and communities to meet the full range of their program needs. Although not a high-frequency occurrence, the DOJ registries certify programs with a single quality QED as Effective programs, and I think the risk that such programs will fail to reduce crime when taken to scale is high. Apparently, the DOJ is willing to take a higher risk of program failure than I am (Elliott et al. 2020). And I think the public and governmental support for crime prevention research and recommended programming will be undermined by continued failures to have a significant impact when programs with this level of evidence are certified as evidence-based and taken to scale.

I also believe the high scientific standards established by Blueprints have had a positive impact on the general quality of evaluations over the past two decades, highlighting the design, methods, and analysis issues that are needed to support a causal claim of intervention effectiveness. We have provided a summary of the specific design and analysis issues that undermine internal validity based on our board's extensive experience in certifying model and promising programs (Steeger et al. 2021). There is a general awareness that a Blueprints certification as model or promising requires serious attention to internal validity issues. A review of the evidence supporting the scale-up effectiveness of Blueprint Model Programs is provided in Elliott et al. (2020).

Blueprints is unique in several other respects. I think the review process used by Blueprints is more rigorous than that used by other registries listing crime and delinquency programs; the amount of information provided for each listed program is more extensive than found on other registries; and the ability to search for almost any prevention program and find a rating and explanation for that rating is unique. Finally, it is a non-government-funded registry, making it less likely to be influenced by political vicissitudes.

## CONCLUDING THOUGHTS

It has been an exciting six decades. I take great satisfaction in having taught thousands of undergraduate students and mentored several dozen graduate students, both as PhD students and as research assistants on my many grants. I was greatly blessed by an unending series of research grants that allowed me to carry out an extensive research agenda that I believe contributed to the current body of criminological knowledge and improved prevention programming that benefits children and families. But it should be obvious I did not do this alone. I have had a truly exceptional group of colleagues: Dick Jessor, David Huizinga, Frank Dunford, Scott Menard, Sharon Mihalic, Abby Fagan, Beverly Kingston, Kirk Williams, Jen Grotzinger, Sabrina Arredondo-Mattson, and Bill Woodward at the University of Colorado and members of the Blueprints Board, especially David Hawkins, Pat Tolan, Tom Cook, and Denise Gottfredson. Our collaboration taught me much. We did not always agree, but our collaboration was always cordial and almost always led to a better understanding of the issues and how to address them.

I left the Program on Problem Behavior and Positive Youth Development and the Blueprints initiative in good hands. Sharon Mihalic retired in 2016 and was replaced by Pam Buckley as Blueprints Director, managing the daily operation of the Blueprints initiative. Karl Hill became the director of the program in 2017 and, along with Pam Buckley, Fred Pampel, and Amanda Ladika, provides the ongoing leadership for Blueprints. Both the Program on Problem Behavior and Positive Youth Development and Blueprints are thriving under their capable leadership.

Most importantly, I have been greatly blessed by the Lord throughout my career. By His grace, “my cup runneth over.”

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The author is not aware of any undisclosed affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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