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METHODOLOGICAL REALITIES: SOCIAL SCIENCE METHODS AND BUSINESS REORGANIZATIONS

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How to study reality is an old problem in the design of social science research. The question is not whether a study is to be empirical, for there are many different ways to encounter social reality empirically. The problem is rather one of viewpoint. Biologists know that each organism is unique, but large numbers of organisms can be described and defined into species and genera. Similarly, social scientists know that social behavior can be studied by intensive investigation of a single case (psychoanalysis, for example) or by examining enormous numbers of observations (such as comparisons of population censuses). Often conceptualized as a conflict between qualitative and quantitative methods, the viewpoint problem is better conceptualized as the trade-off between rich detailing of a few selected cases—the case study—versus the ability to generalize to the universe of events under study—the social survey.

In practice, most social scientists develop a career not only in a substantive area (the kind of behavior they study) but also in a methodological tradition (how they will study it). Thus, the social scientist may feel compelled to defend the viewpoint as well as the findings. Standing at the beginning of a long-term study, we have had the opportunity to ponder the issue of viewpoint and the best procedures to study businesses in bankruptcy. In designing the Business Bankruptcy Study, we have continued a method derived from the social survey tradition and previously used in the two waves of the Consumer Bankruptcy Project. This decision opens us to the charge that we are using this technique merely because it is familiar to us. The objective of this Article is to argue that our methods

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are appropriate for analyzing business bankruptcy as a class of human and organizational behavior.

The viewpoint problem is accentuated in any study in which there is extreme inequality or heterogeneity in the underlying metric of the phenomenon. Business bankruptcy is such a case, for a few very large cases dwarf the many thousands of smaller cases in terms of their size. By contrast, if there were many equally-situated actors, a situation that is more true of consumer bankruptcy, then a survey would be the evident strategy. The viewpoint issue can be further specified, in this instance, as the use of the survey under conditions of high heterogeneity. The issue can be clarified by considering the analogy of predicting voting outcomes.

Sample surveys are the preferred method to study political opinions, e.g., in a presidential campaign, if each voter has one vote. But if a few actors have disproportionate influence, then a modified method is required. For example, to learn the fate of a takeover bid in a corporation would require a more complicated survey because each share has a vote, and polling (sampling) the shareholders without taking account of how many shares they own would yield inaccurate results. Possibly only the large shareholders should be studied, or, alternatively, the survey method would have to be altered substantially. The researcher could sample the shares, as opposed to the shareholders, or she could weight shareholders' opinions by the number of shares they control.

Even this commonplace example is greatly complicated by the observation that powerful individuals and corporations with great financial resources have disproportionate influence with the electorate during a presidential campaign. It is methodologically difficult to take account of the votes of the many while still acknowledging the disproportionate influence of the few, and probably no single study will accomplish the feat.

Researchers studying business bankruptcy encounter a similar problem: the routine cases of many, relatively small businesses are overshadowed by the time and money deployed in a few marquee cases. Embedded in our study design are two assumptions about business bankruptcy. The first assumption is that at any point in time, there is a spectrum of activity in business bankruptcy that varies by petitioner, chapter, and locale, and the reality of business bankruptcy includes this entire spectrum. The second assumption is that business bankruptcy has long-term effects, which are also likely to vary by petitioner, chapter, and perhaps locale. The two major sections below explore how these two assumptions have shaped our study.

I. THE BANKRUPTCY SPECTRUM: SYSTEMATIC SAMPLES VERSUS CASE STUDIES

The classical critique of case studies is that the single case may not be generalizable to other cases.¹ Large, complex business bankruptcies lend themselves to the case study precisely because of their unique properties.² Many of the cases have sufficient drama, human interest, and large-scale economic consequences to grab the public's attention. When these large cases, interesting in themselves and full of analytic opportunities, enter public discourse, they potentially shape public views of business bankruptcy. No matter how interesting such cases are, however, both practitioners and judges would probably regard them as unique rather than representative of the business bankruptcy docket.

The message that the public takes from the well-publicized case, however, might or might not be the message the analyst intends. The images formed from a single case may be so prominent that they come to define the social phenomenon, to determine what is "real" about the phenomenon. This process, which might be called ontological primacy, is similar to the stereotyping that is often done of groups of individuals: a partial image becomes definitive.

A. *Case Studies, Anecdotes, and the Danger of Ontological Primacy*

Case studies, however, enjoy a number of advantages, most of which derive from the depth of information they provide.³ The intensive investigation of a single case can allow the researcher to illuminate the history, side issues, back-door politics, and backstage negotiations that might otherwise never be seen to have influenced the outcome of the case.⁴ A complex case, studied after the fact, reveals the location of decision points and the identity of actors who influenced those decisions. Case studies have long been the staple of education in law, business, and a few other areas because they yield rich insights and allow students to experi-

1. For a general comparison and critique of ethnography, case studies, and social surveys, see RANDY HODSON & TERESA A. SULLIVAN, *THE SOCIAL ORGANIZATION OF WORK* 31-36 (1990).

2. See, e.g., RICHARD B. SOBOL, *BENDING THE LAW: THE STORY OF THE DALKON SHIELD BANKRUPTCY* (1992); KEVIN J. DELANEY, *STRATEGIC BANKRUPTCY: HOW CORPORATIONS AND CREDITORS USE CHAPTER 11 TO THEIR ADVANTAGE* (1992) (presenting several case studies of large corporate bankruptcies).

3. See generally JOE R. FEAGIN ET AL., *A CASE FOR THE CASE STUDY* (1991).

4. SOBOL, *supra* note 2, at ix.

ence vicariously the decisionmaking environment.⁵

Researchers always select their cases for some reason, and most researchers select a case that is interesting rather than ordinary. A complex case with many issues is more intellectually challenging to discuss and analyze than a case that is ordinary or routine. In other words, the case has not been chosen for its representativeness. In terms of the criteria of social science research, the reliability of the case is doubtful—that is, since the same case might never recur, the same analysis might never again be appropriate. Moreover, because the case data are often based on interviews, observations, and other nonreplicable information, a second researcher may not be able to review the data. Finally, because the data are then subjectively interpreted, a second researcher might not reach the same conclusions as the first.

Methodologically, in their unrepresentativeness, case studies have some points in common with anecdotes. Both cases and anecdotes are selected, the case for analysis and the anecdote for remembering, because they are remarkable. An anecdote becomes memorable to the practitioner, in contrast to thousands of unremembered cases, precisely because it is extraordinary. Experienced practitioners in any field mentally archive accounts of particularly interesting or telling cases they have encountered.

But with the anecdote, frequent retelling refines and polishes the details. Extraneous details are discarded, and salient details are emphasized or even embellished. At this point the anecdote becomes the opposite of the case study. Although both have been selected for a reason, the case study preserves details and complexity; the anecdote achieves simplicity.

Case studies, however, may easily become anecdotes. The reader, drawing an important point or an interesting story from the case, is likely to discard many of the details when remembering and retelling the case. This practice is often observed among students, for whom a legal case becomes associated only with a particular principle or issue. Other interesting details or issues of the case may be blurred or discarded to throw the “moral” or teaching point of the case into sharper relief.

The impact of the case study and the anecdote on public policy may be ultimately similar. After the process of softening and simplifying details occurs, the anecdote or case study may assume ontological primacy. The point or moral that is drawn from the particular case, no matter how

5. See, e.g., C. ROLAND CHRISTENSEN, *TEACHING AND THE CASE METHOD* (1987); R. SILVERMAN ET AL., *CASE STUDIES FOR TEACHER PROBLEM SOLVING* (1992).

isolated a circumstance the case represented, comes to define the meaning or reality of the larger class of behavior to which it belongs. Chapter 11 cases may be especially vulnerable to this process, with cases recalled because they are especially large, complex, laden with broader ramifications, or simply time-consuming, and subsequently assuming a privileged position as characteristic or typical of business bankruptcy. We have earlier warned of the dangers of using war stories or anecdotes to make bankruptcy policy.⁶ War stories, especially anecdotes of abuse, are staples in journalism and congressional hearings. Statutes written only in response to case studies could yield highly complex codes that anticipate bizarre circumstances while ignoring the commonplace circumstances citizens are most likely to encounter.

There is a role for the case study, the anecdote, and even the war story, but none of these approaches can describe the full spectrum of business bankruptcy; none of them, therefore, is adequate for describing how the business bankruptcy system operates. Anecdotes and war stories, in any event, are in plentiful supply. What is lacking is a more systematic effort to represent the full spectrum of cases in bankruptcy.

B. Surveys and the Study of the Ordinary Event

The social science survey and its derivative methods, in comparison with the case study, try to represent proportionately the full heterogeneity of the phenomenon under study, within the limited time and funds available to the researcher. While the extremes of variation should be represented in a survey, by definition they will not determine the focus of the study. Instead, survey data emphasize the central tendencies, or the average characteristics, of the population. *Population* in this usage refers to all of the events of interest; in the current study, the population consists of business bankruptcies. Another name for the population, commonly used in statistics, is the *universe*.

The characteristic feature of survey methods is the selection of some cases from the population for study. In principle, researchers could either study every example of the phenomenon (*universe*), or they could select some examples of the phenomenon for study (*sample*). It is usually impractical to study the *universe*, defined as all cases of the phenomenon. An expensive, costly, and technically difficult example of studying a

6. See Teresa A. Sullivan, Elizabeth Warren & Jay L. Westbrook, *The Use of Empirical Research in Formulating Bankruptcy Policy*, LAW & CONTEMP. PROB., Spring 1987, at 195, 205-07.

universe is the decennial census, which enumerates the universe of all persons residing within the United States.⁷ On the other hand, a physician ordering a blood test does not test the universe of blood—that would cost the patient's life.

The physician who orders a blood test is making an assumption that the cellular structure and the chemical properties of the blood are basically the same regardless of the vein that is pricked.⁸ Whether the blood comes from the right forefinger or the left forefinger, it will be assumed to be *representative* of all venous blood. This idea of representativeness is critical to the conduct of a survey. Survey researchers must not pick their subjects because they are interesting, or blue-eyed, or friendly-looking. And unlike the case study, the researcher cannot select the case because it is interesting.

Neutral selection criteria must be used to avoid biasing the result, and mathematics provides the usual neutral basis for the selection. The textbook example of the social survey is conducted using a random sample, in which every person (or event) in the universe has an equal chance of falling into the sample. This approach is the antithesis of the case study. Representativeness permits the survey researcher to generalize, within the limits of sampling error, to the entire universe. And the estimate of sampling error results from the mathematical parameters of the sample selection.

In the case of our research project on business bankruptcy, it would be prohibitively expensive to study the universe of every business bankruptcy filed within the United States in a specific time period. The alternative, which is studying a sample, requires identifying a representative group of cases for more intensive study. Like the physician who makes assumptions about the characteristics of blood, we make the assumption that bankruptcies across the country are more similar to each other than they are different.⁹

7. The inability to complete the enumeration of the entire population has led to a great public controversy over statistical augmentation of the count. Underlying this controversy is the great difficulty of the task. *See generally* U.S. BUREAU OF THE CENSUS, ESTIMATES OF UNDERCOVERAGE IN THE 1990 CENSUS OF POPULATION (1993).

8. The physician is also assuming that the quantitative blood tests are an adequate summary of the properties of the blood. The patient assumes that the blood tests are reliable—that is, that another physician would reach a similar interpretation. Preserving the data in a form in which another physician can read them is important.

9. This assumption might be incorrect, however. In business bankruptcy, it is possible to shop for venues. For example, A. H. Robins sought to file in the Eastern District of Virginia. *See* SOBOL,

The very best way to sample business bankruptcies would be to use a random sample, but the contingencies of time and space mitigate against a true random sample. In terms of time, a random sample would require foreknowledge of every business bankruptcy to be filed within the study's time framework, so that each case would have an equal chance of being selected.¹⁰ In terms of geographic space, a literal random sample of bankruptcies would have researchers scurrying from Sheboygan to Schenectady to Shreveport. This approach is nearly as economically and logistically impractical as studying the universe of business bankruptcies.

A second approximation to random sampling is systematic sampling with particular sites. The sites themselves can be chosen randomly, an approach that is often taken with large national surveys that sample Primary Sampling Units—large geographic areas with equal probability of falling into the sample. Sites may also be chosen for a particular purpose, such as representing some important aspect of geographic diversity or minimizing travel costs. The cases themselves are sampled, again with the intention of giving each member an equal probability of selection. A random sample can be approximated by a systematic sample—for example, taking a random start within the first x cases and then selecting every x th case from a list that contains every case filed. The number x is a function of the desired sample size. This procedure approximates randomness because there is no selection of which particular case will fall into the sample. In our previous study, reported in *As We Forgive Our Debtors*,¹¹ we selected three states because of their varying levels of exemptions for bankrupt debtors. We then were able to sample from the dockets to obtain a systematic sample of the cases filed during our target year (1981).

For the Business Bankruptcy Study, we have tried to retain the advantages of sampling, while limiting the locations at which we will collect data to a relatively small number of sites. We have clustered our sample in

supra note 2, at 25, 60-68. Moreover, we have evidence that there are persistent differences in bankruptcy statistics among districts. See Teresa A. Sullivan, Elizabeth Warren & Jay L. Westbrook, *Local Legal Culture: Twenty Years of Evidence from Federal Bankruptcy Courts*, 17 HARV. J.L. & PUB. POL. 801, 817-33 (1994) [hereinafter *Local Legal Culture*]. We will be able to test for the robustness of this assumption, however, by examining design effects. See *infra* note 14.

10. Alternatively, the sample could be drawn from cases filed in 1993, all of which are now known. We plan to interview debtors, however, and the longer we wait to interview them the more difficult it will be to locate them. Locating debtors and securing their cooperation is likely to be a difficult process in any event. See *infra* note 26.

11. TERESA A. SULLIVAN, ELIZABETH WARREN & JAY L. WESTBROOK, *AS WE FORGIVE OUR DEBTORS: BANKRUPTCY AND CONSUMER CREDIT IN AMERICA* (1989) [hereinafter *AS WE FORGIVE*].

twenty-three districts as the sites for our study; in each site we will sample cases. The sites take account of some of the major sources of heterogeneity that can be foreseen at the beginning of the study. Our strata take account of the following: judicial circuit; the relative number of filings within the district (high-filing versus low-filing); the distribution of filings among the different kinds of business bankruptcies (Chapter 7, Chapter 11, Chapter 13), and case management practices. The two most important methodological decisions we have had to make are: (1) how to select the districts, and (2) how to select the cases within each district.

Our first decision was to represent each circuit. Circuit-level rulings are a conceivable influence on the operation of the courts. Besides, this criterion gave us a suitable geographic distribution of districts, and also tended to represent equally the parts of the country that are experiencing good economic times and those that are in regional recessions. Within each judicial circuit, we have established as a criterion the selection of two districts as sampling sites: the district with the highest number of business filings, and the district with the lowest number of business filings that would still yield at least fifty cases in Chapter 11 during the first year of the study. Information on the numbers of cases filed and the filings by chapter came from published data for 1993 from the Administrative Office of the United States Courts. In nonbusiness cases, at least, there appears to be a high correlation between the number and chapter of filings in one year and the same data in subsequent years.¹² For this reason, we felt justified in assuming that 1993 filings would be an adequate guide to 1994 filings.

Within each district, every business bankruptcy case should have an equal chance of being selected, lest the sample be skewed. We draw cases from divisions in proportion to the total number of cases filed in the division during the preceding year. To draw a sample of bankruptcies only from courthouses located in large, urban centers might ignore bankruptcies occurring in rural regions and filed in outlying division courthouses. The resulting study might be generalizable to the set of all urban business bankruptcies, but miss out on telling variations that arise from extension of agricultural credit, floods, or crop failures.

The choice of the high-filing districts is advantageous because a large proportion of all the business bankruptcies filed in the country during 1994 will be eligible to fall into our sample. During the fiscal year ended

12. See *Local Legal Culture*, *supra* note 9, at 829.

September 30, 1993, about thirty-six percent of all business bankruptcies were filed in our high-filing districts.¹³ The choice of these districts also represents a choice of organizations that face different contingencies. These organizational realities can be hypothesized to affect the functioning of the business bankruptcy system. Clerks in the high-filing districts must receive, organize, and retrieve large amounts of data. In these districts the logistical complications alone may mean that business bankruptcies are handled differently there than in districts with relatively few such cases. By contrast, the low-filing districts may also treat business bankruptcies differently—perhaps there are differences in the speed, formality, and outcomes of these cases. Each of these differences could be predicted from organizational theory.

The choice to divide our sample between high-filing and low-filing districts offers an interesting glimpse into a potential source of difference within the business bankruptcy system, but it also involves some deviation away from a “pure” sampling approach. We did not select the districts because we wanted to travel to Honolulu or had cousins in Seattle—although both of these things happen to be true. We selected the districts because of a neutral criterion—the relative level of filings in 1993—over which we had no control. Nevertheless, it would be prudent for us to test a design effect in the resulting multivariate analyses.¹⁴

We also sample within a time framework, and in this case our framework is calendar year 1994. Cases not filed in 1994 are not in our universe, even if they converted to Chapter 7 in 1994 and thus appear as a new filing. We face the problem that there might be some seasonality in the filing of business cases that we cannot anticipate. Our previous studies have not revealed such seasonality in nonbusiness cases.¹⁵ We cannot rule

13. Calculated from ADMIN. OFFICE OF THE U.S. COURTS, JUDICIAL BUSINESS OF THE UNITED STATES COURTS, ANNUAL REPORT OF THE DIRECTOR OF THE ADMIN. OFFICE OF THE U.S. COURTS app. I at 81-83 tbl. F-2 (1993) [hereinafter ADMINISTRATIVE OFFICE].

14. The design effect is the result on the data that is caused by the design of the study. In our previous study, we tested for state and district effects, which is an example of testing for a design effect. See AS WE FORGIVE, *supra* note 11; *Local Legal Culture*, *supra* note 9, at 834-35. For another description of design effects, see JAMES S. COLEMAN ET AL., *HIGH SCHOOL AND BEYOND* (1982). Coleman notes that design effects are also needed when measures are repeated in a longitudinal study; this also affects our study. See *infra* note 23 and accompanying text.

15. See AS WE FORGIVE, *supra* note 11, at 345, 353 n.1 (reporting minimal seasonality effects in a study of 1981 filings in Texas, Illinois, and Pennsylvania); Teresa A. Sullivan, Elizabeth Warren & Jay L. Westbrook, *Bankruptcy and the Demographic ‘Curse of Job’ 7* (1992) (unpublished manuscript, on file with the *Washington University Law Quarterly*) [hereinafter *Curse of Job*] (showing cumulative linearity in 1991 filings in Texas and California).

seasonality out for business cases, but by extending our study to an entire calendar year we have controlled for one type of seasonality.

Ideally, we would like for 1994 to be an "ordinary" year in terms of business bankruptcy. We cannot, however, control for such temporal fluctuations as business cycles, which are typically longer than one year, nor swings in economic growth that are even longer. Every year is also susceptible to its unique shocks; it is predictable that there will be such shocks, but what they are and their effect on the study are impossible to predict. The record floods along the Mississippi and Missouri Rivers may lead to more business bankruptcies in the Sixth and Eighth Circuits, and the recent earthquake outside Los Angeles may lead to even more business cases than usual in the Central District of California. The geographic dispersion of our test districts, however, increases the likelihood that 1994 will be an "ordinary" year for most of them.

In considering the selection of a representative sample of cases, we face the thorny problem that the cases have not yet been filed, so there is no way to sample systematically from a list. We must select our sample prospectively, rather than retrospectively as we have previously done to study nonbusiness cases.¹⁶ To handle these problems, we intend to sample 150 cases in each district: 50 Chapter 7 cases, 50 Chapter 13 cases, and 50 Chapter 11 cases, divided into four business quarters.¹⁷ A typical method of handling this problem is some form of quota sampling.

A common problem with quota sampling, however, is that researcher intentionality distorts the process. Suppose, for example, that a researcher is asked to interview two people every hour during an eight-hour day. There is a tendency for the interviewer to select subjects who look friendly and relaxed, and to skew the selection in terms of age, gender, race, and perhaps apparent socioeconomic status.¹⁸ To select cases without inappropriately skewing the process, we established a neutral selection

16. See AS WE FORGIVE, *supra* note 11, at 17. A retrospective study of business bankruptcies, although it would allow for a systematic sample of filings, would be problematic because the high number of businesses being liquidated would limit the number of the debtors available to be interviewed.

17. About 15% of the Chapter 11 cases filed in the fiscal year ended September 30, 1993 were listed as "nonbusiness" cases. ADMINISTRATIVE OFFICE, *supra* note 13, app. I, at 81 tbl. F-2. Because we have not previously examined Chapter 11 filings in our study of nonbusiness cases, we are also drawing a sample of nonbusiness Chapter 11s for later study.

18. We have criticized a previous study of consumer bankruptcy that used quota sampling of Chapter 7 debtors. See Teresa A. Sullivan, Elizabeth Warren & Jay L. Westbrook, *Limiting Access to Bankruptcy Discharge: An Analysis of the Creditors' Data*, 1983 WIS. L. REV. 1091, 1093.

criterion of the first twelve or thirteen cases filed during a business quarter within a specific chapter within a district. If, within the first twelve cases, there is a case that is linked to a preceding case (for example, several partners who have filed serially), the second and all subsequent linked cases are excluded from the twelve-case subsample.

Although this criterion is neutral, in the sense that we have no control over which cases are filed or in what order, this criterion may also be biased in an unforeseeable direction. Although we have so far been unable to detect evidence of a bias, it is possible that cases might be filed at the beginning of a business quarter because of time-bound economic circumstances.¹⁹ An example might be taxes that are due on a quarterly basis. One advantage of the division of the study into high-filing and low-filing districts is that we can compare cases filed early in the quarter (in the high-filing districts) with those filed later in the quarter (in the low-filing districts).

We have already encountered the problem in some high-filing districts that our entire subsample for the quarter has filed within the first two hours of the quarter. Moreover, with several divisions accepting filings simultaneously, it is practically impossible to determine which Chapter 11 filings were among “the first thirteen” of the quarter. In these districts, we are listing all the cases filed early on the first day and sampling randomly from this list to generate our sample of cases.

The processes of “listing” and selecting our cases for study are facilitated by access to the computer databases available in most bankruptcy courts—either the PACER or NIBS systems. This “virtual docket” takes the place of the sampling from the physical docket that we used in the Consumer Bankruptcy Project.²⁰ By reviewing these databases systematically, we are able to apply simultaneously our criteria for a business case.²¹ In a high-filing district, we are able to list simultaneously filed cases for random selection and, in a low-filing district, we can scan filings throughout the quarter until we reach the number of cases needed to complete the sample.

19. To draw an analogy to nonbusiness cases, there would be bias in Texas from drawing a sample of bankruptcy cases on the first Monday of each month. Mortgage foreclosures occur in Texas on the first Tuesday of the month, and there is a discernible peak in filings during the first week of the month. See *Curse of Job*, *supra* note 15, at 7. It is predictable, then, that a sample of first-Monday filings would contain relatively more homeowners than a sample drawn evenly from the entire month.

20. See *AS WE FORGIVE*, *supra* note 11, at 18, 350.

21. See Warren & Westbrook, *supra* note *, at 1274.

The objective of these procedures is to minimize human judgment that might also introduce bias into the sample. By setting up a priori procedures, we increase the likelihood that the sample will be representative. This sampling framework approximates the ideal procedure of giving each case an equal chance to be included in the sample. There is, however, no provision for selecting large or important cases. The odds are against any single case, even a case of stunning magnitude, falling into the sample.

II. OBSERVING THE SPECTRUM OF BUSINESS BANKRUPTCY OVER TIME

The Business Bankruptcy Project has been designed to collect data over a five-year period of time. The intent of this design feature is to follow the progress of the bankruptcy and subsequent reorganizations through the courts. This type of study, in which a sample of cases is followed for a period of time, is called a longitudinal study.

The principal technique for following the cases through the courts will be the examination and data coding of court records. The "virtual docket" available to us electronically will allow us to follow the dating of important events within the case. We anticipate that a great deal of variation exists in how quickly and how elaborately the cases are processed. Some cases, especially some of the Chapter 7 liquidations, will close very early. Some cases will be dismissed without further proceedings. Other cases will lead to confirmation of a plan, with the opportunity to observe the success of the plan during the five years of the study.

Besides examining court documents, we also plan to interview the debtors during the course of their bankruptcy. Debtor surveys will give us the opportunity to view the bankruptcy from a different perspective and to gain access to information not normally available in the files, such as the changes in the number of workers employed.

All longitudinal studies fall prey to certain common problems. In the case of individuals, we expect that certain developmental or aging processes will take place over a five-year span of time, but we are fairly certain that, regardless of the weathering effects of time, after five years the individual either will be dead or will be the same individual.²² When the object of study is a social group, however, such as a family or a business, what to study over five years is more difficult to understand. In some longitudinal

22. Even individuals, however, are often difficult to locate for a longitudinal sample. See Brian R. Claridge et al., *Tracing Members of a Panel: A 17-Year Follow-Up*, in *SOCIOLOGICAL METHODOLOGY* 185, 187 (Karl F. Schuessler ed., 1978).

studies of families, for example, the issue arises of whom to follow over a period of time: the adult children who have left home, married, and started their own families? the divorced father who leaves the household but retains visiting rights with the minor children? the stepfather who enters the picture in year three after marrying the mother in the family? What exactly is a longitudinal family?²³

Moreover, following all of the family members after they are restructured into a new family is difficult. Some of them will not want to participate because they do not want to remember the old unhappinesses before the divorce and the remarriage. Some will have moved away. The divorced father may owe child support and not wish to be found again; the divorced mother may not want him to find her.

Placed in survey jargon, these problems constitute biases of various sorts. The death, disappearance, or noncooperation of the sample members constitutes attrition. Moreover, the attrition is not neutral; the sample members who are lost because they die may have been less healthy at the beginning of the study. The people who disappear, never to be heard from again, may have something to hide. Those who refuse to talk to the researcher may have the most stressful jobs or the most acrimonious personalities. Whatever the reason for the losses from the sample, the differential attrition will affect the estimation of statistical parameters of the sample as time passes.

As difficult as such problems are in surveying families, they are even more difficult for businesses. Businesses form, reorganize, and go out of business; small businesses, in particular, tend to have short life spans and high "infant mortality" rates.²⁴ Bankruptcy covers only a fraction of the casualties.²⁵ But following that small fraction of casualties for five years will be a major challenge. Many of the businesses in the sample will

23. Two major longitudinal studies, the Panel Study of Income Dynamics and the Survey of Income and Program Participation, were designed to follow family groups and have encountered exactly these problems. See, e.g., Sean Beckett et al., *The Panel Study of Income Dynamics After Fourteen Years*, 6 J. LABOR ECON. 472 (1988); PANEL TO EVALUATE THE SURVEY OF INCOME AND PROGRAM PARTICIPATION, NAT'L RES. COUNCIL, *THE FUTURE OF THE SURVEY OF INCOME AND PROGRAM PARTICIPATION* 14 (Constance F. Citro & Graham Kalton eds., 1993).

24. In 1992, 8,805,000 businesses were formed and 96,857 failed. U.S. BUREAU OF THE CENSUS, *STATISTICAL ABSTRACT OF THE UNITED STATES* 1993 tbl. 861 (1993).

25. Compare the number of businesses terminated, *id.*, with the 66,438 business bankruptcies filed in the United States in fiscal year 1993. See ADMIN. OFFICE OF THE U.S. COURTS, *BANKRUPTCY STATISTICAL INFORMATION* 5 (rev. ed. Nov. 4, 1993) (on file with the *Washington University Law Quarterly*).

be difficult to contact, some of them having already closed their doors.²⁶ Some business owners will refuse to talk with us. Other businesses will fission into new or multiple entities, each of which shares some common history with the bankrupt entity but is now pursuing an economic life of its own. Numerous decisions lie ahead of us concerning how we follow up on these “progeny” of our original sample.

A great deal of technical work has been done to permit investigators to identify, measure, and model attrition bias in their data.²⁷ Much less work has been completed on the conceptual issues of how to understand multiple consequences that flow from a business bankruptcy. For example, if the two divisions of the Blitz Corporation go their separate ways, and Blitz-Ever becomes wildly profitable while Blitz-Ertz eventually liquidates, was this bankruptcy successful? Did it represent an example of a system working properly, a dismal failure, or a partial success with a partial failure? While we would not at this point define what “success” or “failure” will mean for our sample by 1999, we will try to preserve as much information as we reasonably can, in a machine-readable format, so that we and other researchers will be able to draw such conclusions.²⁸

III. SUMMARY AND CONCLUSION

Any research project requires a series of design decisions that ultimately affect the type and quality of data that will be available. The Business Bankruptcy Project has been designed to provide a systematic view of how the business bankruptcy system operates, including a view from the debtors' perspective in addition to the documents filed with the court. To examine the system, and not merely a large number of cases, we have included districts with differing levels of filings and we have included business filings in Chapters 7, 11, and 13. The study, which begins with cases filed

26. Low response rates have been characteristic of previous bankruptcy studies, either because of high refusal rates or location difficulties. See NORMAN BRADBURN & SEYMOUR SUDMAN, *IMPROVING INTERVIEW METHOD AND QUESTIONNAIRE DESIGN* 7, 81 (1979) (reporting that 27% of bankruptcy sample could not be located; up to one-third denied having been in bankruptcy in later interviews); DAVID STANLEY & MARJORIE GIRTH, *BANKRUPTCY: PROBLEM, PROCESS, REFORM* 41 n.1, 224-25 (1971) (reporting such difficulty in locating original sample for interviews that another interview sample was drawn); CREDIT RESEARCH CENTER, PURDUE UNIVERSITY, *1 CONSUMER BANKRUPTCY STUDY* 25-26 (1982) (reporting that 1050 of the 2249 debtors approached refused to be interviewed).

27. Michael T. Hannan & Alice A. Young, *Estimation in Panel Models: Results on Pooling Cross-Sections and Time Series*, in *SOCIOLOGICAL METHODOLOGY* 52 (David R. Heise ed., 1977).

28. We have archived the data from our earlier study in machine-readable form so that other researchers may use them.

during 1994, will follow the cases for five years to the extent possible. Although we anticipate a number of technical and operational difficulties, we are optimistic that this design will yield data of sufficient depth and quality to permit generalization about how well business bankruptcy works.

