

**Exploitation and Inference: Mapping the Damage From Therapist-Patient Sexual
Involvement**

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Abstract

A growing body of evidence documents a clinical pattern of harmful effects of therapist-patient sexual involvement. In addition, surveys suggest that one to 12 percent of all therapists may have engaged in this behavior at least once in their careers. In order to develop a more comprehensive research agenda several of these studies are reviewed in terms of inferences that may or may not be drawn. Case studies and surveys may provide for inference of clinical harm and syndrome but are limited in terms of generalizations about incidence in the overall population. A population approach coupled with case sampling may provide a useful tool by which to approximate a minimum level of incidence and of effects.

Exploitation and Inference: Mapping the Damage From Therapist-Patient Sexual Involvement

An array of studies documents reported harm engendered by therapist-patient sexual involvement as well as rates of incidence and common syndromes of presenting symptoms e.g., Akamatsu, 1988; Bates and Brodsky, 1989; Borys and Pope, 1989; Bouhoutsos, Holroyd, Lerman, Forer, and Greenberg, 1983; Feldman-Summers and Jones, 1984; Gabbard, 1989; Gabbard and Pope, 1988; Gartrell, Herman, Olarte, Feldstein, and Localio, 1986, 1989; Gottlieb, 1990; Herman, Gartrell, Olarte, Feldstein, and Localio, 1987a, 1987b; Pope and Bouhoutsos, 1986; and Pope, Keith-Spiegel and Tabachnick, 1986.² The present paper reviews the case study and survey research in terms of inferences about harm and inferences about incidence in the population based on samples of patients and surveys of providers. An ideal research agenda would include or address issues of clinical relevance such as under what conditions therapist-patient sexual involvement is more likely to occur, or to result in harm, in terms of patient-variables, therapist-variables, and process variables.

In contrast, a separate issue concerns the rate of incidence, for which the available data have only limited usefulness. The distinction drawn here is between describing the clinical nature of the problem, on the one hand, and estimating the scope or extent of the problem (e.g., degree of harm, number of victims) in the population at large, on the other hand. Estimating the scope or extent of the problem is complex based on the case study or survey approach. Holroyd and Bouhoutsos (1985), Koltko (1989), Pope (1990c), Pope and Bouhoutsos (1986), Pope, Tabachnick and Keith-Spiegel (1989), Riskin (1979), Wright (1985), as well as the reports discussed below recognize some of the difficulties in gathering data and making inferences about therapist-patient sexual involvement. However, answering the important and distinct question of the extent or prevalence of therapist-patient sexual involvement in the population may exceed the limits of case study and survey methodology. As a result, research to date may only point to broad upper or lower limits of prevalence or harmfulness.

Several studies looked at harmfulness of therapist-patient sexual involvement. The volunteers in these studies were recruited by advertisements or word-of-mouth publicity and then assessed by interview or asked to complete questionnaires (e.g., Feldman-Summers & Jones, 1984). Additional studies of this variety are cited by Pope and Bouhoutsos (1986) and Pope (1989), including four unpublished doctoral dissertations (Butler, 1975; D'Addario, 1977; Stone, 1980; and Vinson, 1984; 1987). These investigations reported that sexual involvement with therapists harmed patients. In addition, Pope and Bouhoutsos (1986) and Pope (1989) have described the Therapist-Patient Sex Syndrome, a syndrome not unlike Post-Traumatic Stress Disorder, which is believed to be caused by a sexual relationship with a therapist. The Syndrome includes: (1) ambivalence; (2) guilt; (3) feelings of isolation; (4) feelings of emptiness; (5) cognitive dysfunction...; (6) identity and boundary disturbance; (7) inability to trust (often focused on conflicts about dependence, control and power); (8) sexual confusion; (9) lability of mood; (10) suppressed rage; and (11) increased suicidal

²Solutions have been offered, such as better therapist training (e.g., Pope, Keith-Spiegel, & Tabachnick, 1986); stricter regulation and ethical codes (e.g., Nagy, 1990; Shopland & VandeCreek, 1991; Pope, 1990b; Vasquez, 1991); more generous malpractice awards to plaintiffs; and discontinuing possibly ineffectual efforts to rehabilitate offenders and return them to clinical work (Pope, 1990b).

risk. (Pope & Bouhoutsos, 1986, p. 64).
and it “bears similarities to aspects of the borderline (and histrionic) personality disorder” (Pope & Bouhoutsos, 1986, p. 64).

Sampling, Self-Selection and Inference: Who Wants to be Studied, And Who Does Not—And When?

Studies which have attempted to answer questions concerning prevalence (How many patients have been victimized; how many therapists have become sexually involved with patients?) and harmfulness (How seriously have patients been harmed?) have sampling problems. The problems lie with studying research volunteers who may have motives to participate, or to avoid the research sample, that correlate with the topic of investigation. As a result one does not know how representative the sample studied may be of some larger population to which one wishes to generalize.

This sampling issue was raised during the 1990 United States Census efforts to tally illegal aliens and homeless families. For example, the former group has motives to avoid governmental agencies and the latter is difficult to find in a systematic fashion. As a result, estimates of the size (or prevalence) of either group in the broader population are difficult to make.

Because individuals have motives to join or evade inclusion, the resulting sample may be biased, e.g., as an estimate of size, in that only certain strata of the population may participate or evade. In such a situation, consistent sampling methods across studies would result only in consistently biased samples. Results may be empirically reliable, i.e., consistent across studies, but invalid for inferences about the larger population in terms of incidence, prevalence, or living conditions.

As a further example, one might recruit volunteers for a study of reaction time. If the variables investigated are mediated only by the nervous system then they would be independent of variables that might predispose or motivate a volunteer to participate (such as learned behavior, social events, incentives for participation). One could assume that volunteers were similar, in terms of nervous systems (the set of variables under investigation), to others who did not choose to volunteer (or know about the opportunity). As long as the variables related to volunteering were independent of the variables or events under investigation, the generalization to others in the larger population would be straightforward with high external validity (cf. Campbell & Stanley, 1963).

Case studies of therapist-patient sexual involvement may result in clinically important data concerning the specific patients or volunteers who participated. However, just as in the Census example above, some patients may have been motivated to be included while others may have been motivated to be excluded. Thus, it is not clear how far one might generalize results or make inferences beyond the actual patients involved in the study. That harm to these patients occurred or that therapists became sexually involved with patients is not in question—the results from the participants are clear. The clinical and descriptive results, within the limits of case study samples, provide a useful picture of the clinical sequellae for these patients.

It is a distinct issue, however, to say how harmful therapist-patient sexual involvement may be in the population, or relative to other relationships, or to say how prevalent the problem may be in the the population. A separate issue of causality concerns under what conditions therapist-patient sexual involvement may occur and generate harm in the broader population beyond that of the volunteer participants. Was the unwitting selection by the patient of an exploitative therapist the only causal factor, or a proximal event in a long chain of events? Was there something in the patient’s history (let alone the therapist’s history) engendering vulnerability to therapist-patient sexual involvement and to damage from that involvement?

We know that patients volunteering for case study research were harmed. But due to potential motivated self-selection into the research, it is difficult to estimate how representative these patients may have been in terms of a larger population of patient-therapist interactions. Patients who volunteered to participate may have been drawn only from that part of the population who in fact suffered harm. Pope and Bouhoutsos (1986) have observed:

Unfortunately, empirical research either to prove or disprove benefit or harm has been very difficult to undertake because of the inaccessibility of both therapist and patient populations. Small-scale studies on self-selected individuals responding to newspaper advertisements or “grapevine” requests for subjects have provided most of the information. (p. 59).

These have been “one-shot case studies” (Campbell & Stanley, 1963) in which “a single group is studied only once, subsequent to some agent or treatment presumed to cause change...The inferences are based upon general expectations of what the data would have been had the X not occurred” (p. 6-7). The agent or treatment (X) in these case studies is therapist-patient sexual involvement, and the implicit comparison—which is usually unstated—is with an hypothetical “unharmed” patient who had not been sexually involved with a therapist.

Recruitment for “One Shot Case Studies”

If one assumes that two populations exist—the population of individuals who were harmed by their sexual involvement with therapists and the population of individuals who were unharmed (to properly design and interpret studies one must presume the existence of the latter population, even if one is personally certain it contains no members)—one can ask how each of these populations might react to research methods which used a newspaper advertisement or other communications to recruit subjects who had experienced sexual involvement with their therapists. Patients who had experiences in therapist-patient sexual relationships that were neutral or positive may not be strongly motivated to respond to a newspaper advertisement, or word-of-mouth publicity, seeking research participants. Most people, after all, do not generally seek opportunities to participate in psychological research. Assuming this population exists, that these individuals were not suffering, were not looking for a sympathetic listener, or indeed, help with their plight, would they bother to participate in a study? On the other hand, individuals who were suffering, who had something to get off their chests—a special experience of just the sort the researcher is investigating, an experience of which one is ashamed and about which one despaired of ever finding a sympathetic listener—might take the time to volunteer.

The suffering victim appears to be someone who would be motivated to participate in interview or survey research on the consequences of therapist-patient sexual involvement, although shame, fear, or denial probably would limit actual participation (Pope, 1990a) much as these motives probably inhibit complaint-filing (Gottlieb, 1990). The non-suffering survivor of therapist-patient sexual involvement, in contrast, might only wish to participate in the research to a casual degree, if at all. If the involvement with the therapist were perceived only as an unremarkable sexual relationship in one’s life, one might not bother to pick up the phone, or drive somewhere, to tell someone about it. And if it had been a positive experience—perhaps a patient was not unhappily involved with one of the sexually involved psychiatrists who reported to researchers (Gartrell et al., 1986), albeit with unknown accuracy, that they had married or remained friends with their former patient-lovers—such a patient might not want to reveal this to a researcher and risk derailing the psychiatrists’ career. In short, the suffering victims have a motive to reveal data (to a listener who is perceived as sympathetic and discreet) while the non-suffering survivors have a motive either to conceal data, or, at the very least, to be indifferent to researchers’ calls for volunteers.

If this state of affairs were to exist, the result would be a consistent sampling bias which would reliably affect all survey research carried out on survivors of therapist-patient sexual involvement: Individuals belonging to the population of patients who had neutral or positive experiences in such relationships would be systematically lost to researchers by virtue of their indifference, while damaged individuals would make themselves more available for study. If so, the inference drawn from these results could only be one of increased harm. The consequence might have been to produce the result most clinicians expect—findings of consistent and significant harm—because harmed individuals would have been more likely to volunteer for study (in turn leading to an underestimation of the number of incidents).

Typical summations (Pope & Bouhoutsos, 1986; Rodolfa, Kitzrow, Vohra & Wilson, 1990) state, “It is well documented that therapist-patient sexual intimacy has deleterious effects on the client and the therapist” (Rodolfa et al, p.313), and, “Overall, the balance of the empirical findings is heavily weighted in the direction of serious harm resulting to almost all patients sexually involved with their therapists.” (Pope and Bouhoutsos, 1986, p. 63). These are reasonable inferences in a clinical sense, especially if the authors would have stated something like “among those victims/survivors to whom researchers have access.” A separate issue in the empirical sense remains that of estimating prevalence in the population.

A Study Which Included An “Untreated” Comparison Group

One study by Feldman-Summers and Jones (1984) included a group of “untreated” control or comparison subjects. Campbell and Stanley (1963) call this design the “static group comparison.” One of the “treated” groups (quotation marks are used to remind the reader that this was not an experimental design) was composed of self-selected volunteers who had experienced therapist-patient sexual involvement. Another “treated” group was composed of self-selected volunteers who had experienced sexual involvement with non-psychotherapeutic health care providers, primarily physicians. The “untreated” group was composed of patients who had completed psychotherapy. The “untreated” patients were matched on several demographic variables with one of the treated groups. The sexually involved subjects fared worse than those in the “untreated” comparison group on several self-report measures of well-being. Sexual involvement with one’s psychotherapist was found to lead to negative consequences equivalent to sexual involvement with non-psychotherapeutic health care providers.

However, as in all static group comparisons, it is not possible to assume that the groups had been or would have been equivalent in the absence of therapist-patient sexual involvement. As Campbell and Stanley state:

In marked contrast with the “true” experiment... [in which subjects are randomly assigned to groups], there are in these... [static group comparisons] no formal means of certifying that the groups would have been equivalent had it not been for the X. ...matching on background characteristics ...is usually misleading, particularly in those instances in which the persons in the “experimental group” have sought out exposure to the X. (p. 12)

Thus, patients involved sexually either with health care providers or psychotherapists fared worse than those who completed psychotherapy, but one does not know whether the differences were due to causes beyond the sexual involvement, such as pre-existing differences between groups or the positive benefits of having successfully completed psychotherapy.

Was it Significant that the Sexual Partner Was a Therapist?

When one discovers damage in samples of individuals who had been sexually involved with their therapists, one implicitly might assume that those sampled would have escaped this damage had they been sexually involved instead with someone who was not

their therapist. To what extent is this assumption justified? Nagy (1990), in discussing proposed revisions of the APA Ethical Principles, has asked, "How frequently do some individuals become depressed or attempt suicide when any love affair ends, not just one with their former therapist" (p. 41). The divorce courts and psychotherapists' outpatient practices are full of individuals who bear the scars of ordinary sexual relationships. In fact, ordinary marital discord brings more Americans to a psychotherapist than any other stressor (Veroff, Kulka & Douvan, 1981). Indeed, some radical feminists assert that heterosexual relationships are structured to cause harm to women (e.g., Dworkin, 1987), and, as Pope (1990b, 1990c) and others have pointed out, therapist-patient sex may be overwhelmingly heterosexual—a male therapist involved with a female patient.

The therapist-patient sexual involvement literature may implicitly assume that heterosexual relationships are relatively benign as compared with therapist-patient sexual involvement, making the latter appear, by contrast, unusually malignant. Clinically, the victims of therapist-patient sexual involvement who come to our attention are clearly harmed. However, the data gathering situations do not allow one to extrapolate to a larger population concerning under what conditions harm does or does not occur, or, more specifically, what factors in the near-term or in the victims' life span may have caused the observed damage.

A Study Which Located Victims Who Did Not Volunteer

A unique study was carried out by Bouhoutsos et al. (1983). This study attempted to bypass the problem of asking therapists or patients to volunteer to talk about their own sexual experiences. Bouhoutsos et al. mailed a questionnaire to every licensed psychologist in California, asking them to report on experiences of therapist-patient sexual involvement with former therapists that their patients had discussed with them. Although the return-rate of questionnaires was low (16 percent), and the return-rate of therapists reporting positive instances of therapist-patient sexual involvement was even lower (7.3 percent), this study provided a new source of data regarding this topic. The central finding was that 90 percent of the reported instances of therapist-patient sexual involvement were associated with negative consequences for the patient. Bouhoutsos et al. concluded "Even though ample opportunity was provided for reporting positive outcomes, the results of this study clearly demonstrate that sexual intimacy within the therapeutic context is harmful to patients" (p. 194). This is the only study with data concerning the incidence of harm befalling the patient that did not rely on volunteer victims or perpetrators for data.

While this study seemed to bypass problems of self-selection among the sexually involved parties, it still uses, although indirectly, a sample of self-selecting volunteers. As with the studies discussed above, this study might be biased because of its sampling methods towards finding predominantly harmful outcomes of therapist-patient sexual relationships. Bouhoutsos et al. stated that, "It is important to stress the truncated nature of the sample, which included only those patients returning to therapy after having become involved in sexually intimate behavior with a former therapist" (p.195) but asserted that 90 percent of sexually involved patients probably do seek subsequent therapy.

This "truncated sample," however, remains an issue in need of clarification in the literature. For example, Gabbard (1989) stated: "A survey of practitioners (Bouhoutsos et al. 1983) has demonstrated that at least 90 percent of patients are seriously harmed by this form of sexual exploitation" (p. xi). Akamatsu (1988) writes, "Bouhoutsos [et al.] (1983) surveyed psychologists who had treated clients who had been involved in sexual relationships with prior therapists. They found very negative effects of such involvement" (p. 457). In both cases these secondary discussions would have been more accurate had they stated that "although 90 percent of the sexually involved patients in the sample were harmed, one does not know the actual incidence of harm in the larger population of sexually involved patients."

The Bouhoutsos et al. cautionary statement about a truncated sample points to the implications of the sampling problems in this study. Many hurdles needed to be cleared for a patient's sexual experience with a prior therapist to become data. At each hurdle, unknown numbers of patients might have been lost to the researchers—especially unharmed patients. As noted above, this is not a problem when the goal is to study damaged survivors of therapist-patient sexual involvement. However, it remains a problem when one wants to infer the incidence of harm from therapist-patient sexual involvement in the broader population. For estimating the incidence of harm, one needs an unbiased sampling method that is as likely to locate undamaged as damaged survivors.

The following possible groups of patients, who might have been sexually involved with a therapist without experiencing any harm, might have been excluded from this study:

1. Patients who, after the sexual involvement, never returned to therapy (possibly because they had not been harmed by the sexual involvement and did not experience psychic distress). The possible existence of this lost group is explicitly acknowledged by Bouhoutsos et al.
2. Patients who returned to therapy but never mentioned the sexual involvement, either because it was not relevant to the new presenting problem, or because it was not a problem itself.
3. Patients who returned to therapy, and mentioned the sexual relationship, while omitting that the other party was a therapist (possibly to protect the prior therapist who, they believed, had caused them no harm).
4. Patients who returned to therapy, reported the sexual involvement to the new therapist, but whose new therapist failed to complete the questionnaire possibly because no perceived harm had occurred, and the new therapist felt no subjective need to participate in the study. Recall that 84 percent of California's psychologists failed to complete the questionnaire, perhaps because they were aware of nothing, such as a seriously damaged patient, that made the research personally relevant to them.
5. Patients whose new therapist, for some reason—such as a perceived need to protect a colleague—completed the questionnaire but failed to report a known case of sexual involvement. Presumably, one would be even more inclined to protect a colleague if one believed that the colleague, although acting unethically, did no harm. Contrast this with a therapist who is aware of a negative outcome of therapist-patient sexual involvement. Such a therapist might welcome the opportunity to report, via the questionnaire, and sort of “blow the whistle” on the offending therapist, especially since therapists currently, and at the time of this study, have no authority to report offenders.

One does not know whether any of the above five possible groups contained members, or even whether membership in one of the groups would have affected the accuracy of the findings. Furthermore, one could generate additional lost groups and speculate on how their inclusion might have changed the results one way or the other. To complicate matters further, damaged victims of sexual abuse sometimes protect the abuser from discovery, or minimize the damage that was done to them, which makes questionable more general statements about harm in the population. If a significant number of patients escaped from the sample by concealing from the subsequent therapist real damage that had been done to them, that would weaken the present contention that the 90 percent figure could be an artifactual overestimation.

In sum, it is difficult to draw definitive conclusions about the incidence of harm. Although the Bouhoutsos et al. study is useful qualitatively for elaborating the nature of the harm which occurs when therapist-patient sexual involvement does lead to harm, this study does not establish a rate, such as 90 percent, at which harm occurs.

Inferences About The Therapist-Patient Sex Syndrome

In addition to inferential problems concerning the incidence of damage, there are

problems concerning descriptions of damage such as the Therapist-Patient Sex Syndrome. Pope and Bouhoutsos (1986) acknowledge the central inferential problem:

For many patients there may be no data deriving from formal testing and assessments performed prior to the sexual involvement with the therapist. Without such baseline data, the assessment of the damage that was due to the sexual involvement becomes more complex and difficult. (p. 64)

Pope and Bouhoutsos attribute the suffering of these individuals to the exploitation perpetrated by their therapists. However, based on the case study methodology, one could propose that these patients suffered from Borderline or other personality disorders to begin with and that the distress symptoms such as the Therapist-Patient Sex Syndrome represented the sorts of symptoms generally found among these patients regardless of therapist-patient sexual involvement. Pope and Bouhoutsos (1986) suggested that patients in the “high risk category” may “frequently possess characteristics associated with Histrionic Personality Disorder or Borderline Personality Disorder” (pp. 53-54) as part of their premorbid personalities, prior to victimization by a therapist, and that the Syndrome “bears similarities to aspects of the borderline (and histrionic) personality disorder” (Pope & Bouhoutsos, 1986, p. 64). As with case studies in general, the issue remains as to whether the Syndrome symptoms were caused by therapist-patient sexual involvement or were present as part of the patient’s condition.

Consistent with the authors’ suggestion, certain personality disorders may increase the likelihood of therapist-patient sexual involvement incidents. Thus the causal issue remains whether the symptoms were caused by therapist-patient sexual involvement or were present as part of the premorbid condition or both. The causality may not be possible to define empirically, in light of case study methodology and potential sampling bias on the part of patient volunteers. It is clear, however, that patients who have been victims of therapist-patient sexual involvement and who volunteered to participate in this research had negative consequences clinically.

There are separate issues that involve estimates of absolute harmfulness (comparing outcomes of therapist-patient sexual involvement with those of mundane sexual relationships), and determining who is at risk, either for sexual involvement or for subsequent harm. In addition, the courts have recently held perpetrating therapists responsible for all the damage manifested by patient-victims, not merely for exacerbation of any pre-existing conditions (cf. Perr, 1989).

Building upon these extant case studies, a fuller, causal-oriented research agenda would ask about the premorbid personalities of both victims and therapist victimizers: Why do some patients get victimized while some do not? Is a certain kind of patient more at risk for victimization? Are victims of child sexual abuse more highly at risk for “revictimization” (Russell, 1986) by therapists? Is the population that is at risk for therapist-patient sexual involvement also at greater risk for damage? What are the properties of the undamaged survivors of therapist-patient sexual involvement? Did they generally experience forms of psychotherapy that would not have been expected to induce transference-reactions? These questions can only be satisfactorily answered if researchers find some way of drawing unbiased samples of survivors of therapist-patient sexual involvement—samples that had a reasonable chance of including members of the purported population of undamaged survivors—but this may be a practical impossibility.

Finding or constructing such samples, while an ideal approach, may be an impossibility. Drawing random samples of psychotherapy patients and subjecting them to interviews regarding possible sexual involvement with their therapists may solve the problem of sampling bias, but this may be seen as invasive, if not remarkably costly. Indeed, there may be no practical way around this sampling impasse. The reader is referred to Lindzey and Aronson (1985) for a more complete discussion of the methods and

problems of carrying out survey research and to Russell (1986) for a unique example of an extraordinary effort that located and interviewed a random sample of adult survivors of child sexual abuse.³

Survey Research: How Many Therapists Completed The Surveys, Or Abused Patients, Or Did Both?

There have been several questionnaire surveys of therapists' practices and attitudes (e.g., Akamatsu, 1988; Borys & Pope, 1989; Gartrell et al., 1986; Gechtman, 1989; Herman et al., 1987b; Holroyd & Brodsky, 1977, 1980; Pope, Levenson & Schover, 1979; and Pope et al. 1986; Pope, Tabachnick, & Keith-Spiegel, 1987). Pope et al. (1986) mailed a questionnaire to 1000 psychologists, randomly selected from the 4356 members of the Division of Independent Practice of the American Psychological Association and received 585 returns (a 58.5 percent response rate). Gartrell et al. (1986) and Herman et al. (1987b) mailed questionnaires to 5574 psychiatrists (randomly selected by drawing every fifth name from the list of psychiatrists belonging to the American Medical Association) and received 1442 returns (a 26 percent response rate). The studies consistently found that between one and 12 percent of those returning the questionnaire reported sexual involvement with a patient at some point in their career. Akamatsu (1988) found that more than 14 percent of male respondents and almost five percent of female respondents reported sexual involvement with former patients. Gottlieb (1990) succinctly sums up the status of this research: "The true base rate of sexual misconduct among psychologists is not known; however, estimates based on self-report surveys are generally in agreement." (p. 455).

One would like to learn how many therapists in the population do become sexually involved with patients (a question of rate or incidence) and under what circumstances this occurs. Issues of incidence and causality would necessitate access to the population, either directly or through unbiased sampling. Questionnaire surveys, however, involve limited return rates. The problem here is not that the return rates are relatively low. For example, Holroyd and Brodsky (1977, 1980) achieved a 70 percent rate of return using a questionnaire mailed to psychologists. The problem instead is that some practitioners did not return the questionnaire, and one does not know whether this failure to return is systematic and related or unrelated to therapist-patient sexual involvement. Furthermore, if it were related, one would not know how, or even in what direction, such a relationship might skew or bias the apparent results.

Thus, the problem of self-selecting volunteers appears in surveys also. Some therapists volunteered to return the questionnaire, and others "volunteered" to throw it in the trash. Why did some subjects make one choice and not the other? The answer to this question is not known. Moreover, examining demographic data as Gartrell et al. did regarding therapist-subjects who did, or did not, return the questionnaire, or who did, or did not, report sexual involvement, has limited usefulness for inferences in that the demographic properties of the sexually involved population are unknown. Thus, even if the sample of "questionnaire-returners" in the Gartrell et al. study is similar to the population of psychiatrists, one still does not know whether the population of sexually

³Riskin (1979) proposed that experimental designs, although preposterous, might ideally be the way to establish harmfulness. However, even this method would fail to provide valid causal findings. In randomly assigning hypothetical patients to "therapy with sex" and "therapy without sex" experimental conditions, one would gather no data on the interaction between patients intrinsically at risk to become sexually involved with therapists or to experience greater harm from such involvement, therapists at risk to victimize, and ensuing consequences for the victim.

involved psychiatrists was, as a rule, composed of “questionnaire-returners,” “questionnaire-withholders,” or individuals with some other, systematic behavior pattern regarding such questionnaires.

Holroyd and Brodsky (1977) suggested that the questionnaire-withholders in their self-report study may have been more likely to have been sexually involved, since the sexually involved respondents in their sample tended to be among the last to return the questionnaire. In contrast, Bouhoutsos et al. (1983) asserted that questionnaire-withholders, in their study of sexual involvement among patients’ former therapists, probably had no instances of therapist-sexual involvement to report, since those who did have such instances tended to be among the first to return their questionnaires. Finally, Akamatsu (1988), who did not find substantial differences among early and late questionnaire returners, suggested that this pattern “by implication confirms the representativeness of this sample” (p. 457), meaning that the withholders were like the returners.

These three investigators used return latency time to draw inferences about the properties of the questionnaire withholders. However, conclusions about the questionnaire-withholders cannot be drawn based on the behavior of the questionnaire-returners. These researchers assumed that failure to return the questionnaire is an extreme manifestation of whatever makes a person return the questionnaire late. Hence, the logic goes, one can get a picture of the questionnaire withholders by looking at the late-returners. However, questionnaire withholding behavior may have nothing to do with procrastination, and withholders may be entirely unlike or entirely like the late returners since we do not know whether the sample of withholders and the sample of returners belong to the same population.

Gartrell et al. (1986), who obtained a 26 percent return rate of their questionnaires, and a 6.4 percent prevalence rate of therapist-patient sexual contact, asserted that they knew who their questionnaire-withholders were. They state:

We assume that our data can provide only conservative estimates of the prevalence of psychiatrist-patient sexual contact. Some offenders are undoubtedly so concerned about confidentiality, despite assurances of anonymity, that they would never return a questionnaire of this nature (p.1129).

This assumption about the sample, by Gartrell et al., adds the variable of fear to those of procrastination, posited by Holroyd and Brodsky, and eagerness, posited by Bouhoutsos et al., to the list of variables that are presumed to account for questionnaire withholding or returning behavior. Gartrell et al. argued that offending therapists feared discovery if they returned the questionnaires, so they withheld them, creating sampling bias that minimized the percentage of sexually involved therapists. Pope et al. (1987) suggested, in a similar fashion, that fear of discovery might have been a source of sampling bias that limited their reported percentage of therapist-patient sexual involvement to 1.9 percent, although they also acknowledged the possibility that therapist-patient sexual involvement may be actually a less common occurrence.

Speculation in studies like these about the nature of the sampling bias is inevitably fruitless. Reliance on volunteer-subjects, where the act of volunteering may be a correlate of the variables under investigation, inevitably results in generalizability or validity problems. One cannot learn the percentage of sexually involved therapists from these studies except in the most general terms. One could state the limits of these percentages for each study by recalculating the findings twice, assuming in one computation that every questionnaire-withholder was sexually involved and assuming in another computation that every questionnaire-withholder was not sexually involved. This exercise provides a range of prevalence that is, of course, excessively broad. For example, in the Gartrell et al. study, the range of prevalence of sexual involvement among psychiatrists would be between 1.6

percent and 75 percent.

Why Are the Percentages of Sexually Involved Therapists Declining?

Some recent therapist-surveys show that only one to two percent of respondents now acknowledge sexual involvement with patients (Borys & Pope, 1989; Gechtman, 1989; Pope et al., 1987). Borys and Pope state that these lower rates might be caused either by an actual decline in sexual involvement with patients, or by a greater reluctance by practitioners to acknowledge such involvement. When comparing their prevalence findings of 0.2 percent for women and 0.9 percent for men with previous findings of up to 12 percent, they state:

First, it may, of course, represent an actual decline in the rate of sexual intimacies with clients...

Second, the discrepancy may be due to a decline in reporting—even on an anonymous survey—a behavior that is becoming recognized as a felony in an increasing number of states (1989, p. 289).

Thus, as has been the case in all surveys of sexually involved therapists, one does not know whether investigators are measuring actual behavior, or merely the tendency to report such behavior. Without knowing which of these phenomena is being measured, findings have limited utility for estimating incidence or trends in incidence.

Schoener (January, 1991) has recently observed that these studies are inappropriate for drawing inferences regarding a change in frequency of sexual involvement. He states: the most critical issue in self-report studies done to date [is] that none of them has specified a time period in the therapist's life or practice when the sexual contact occurred...That same therapist might have given the same responses to the questionnaire in 1975 and in 1985, even if the only sexual episode occurred in 1965. (pp. 14-15)

The questionnaires have not asked when the reported sexual contact occurred and, hence, limit inferences regarding changes in therapists' sexual behavior. There is no reason to believe that one researcher's recent questionnaire is measuring only sexual involvement which occurred since the last researcher sent out the last questionnaire. On the contrary, Akamatsu's (1988) wording is typical of how the crucial question is asked, "Have you ever [italics added] been involved in an intimate relationship with a client during treatment" (p. 454).

Although retirement and replacement of psychotherapists does occur, it does not occur at a fast rate. Repeated surveys, a few years apart, would probably not reflect changes in behavior between the former and latter populations of practicing psychotherapists. Furthermore, the mean age for sexualizing therapists has been reported as 42 or 43 (Pope, 1990c), making retirement and replacement of mean age mid-career practitioners an unlikely explanation for population differences. Finally, since the American Psychological Association, for example, currently expels or drops only about 11 members per year (Nagy, 1990), detectable changes in the population because the offenders are being removed from the field are unlikely. Any changes in reports of sexual involvement on repeated surveys, then, are likely changes only in the rate of research-compliance among the sexually involved therapists—the second of Borys and Pope's explanations offered above.

Inference of Prevalence Based on Therapists' Response: How Many Patients Are Affected?

Another problem concerns the distinction between the percentage of sexually involved therapists and the percentage of sexually involved patients. Even if the prevalence of sexually involved therapists were, in fact, the 6.4 percent found by Gartrell et al., the problem of therapist-patient sexual involvement as far as numbers of patients are concerned remains less clear. The figure of 6.4 percent is not the percentage of patients who became

sexually involved with their therapists. It is the percentage of therapists who at some time in their careers reported having been sexually involved with one or more patients.

Most of the sexually involved therapists in the Gartrell et al. study (67 percent) reported sexual involvement with only a single patient in their entire professional careers, consistent with Pope et al. (1986) who found that 86 percent of their sample of sexually involved therapists engaged in this practice only once or twice in their careers. Despite the methodological and inferential problems discussed here, one could assume these findings are valid—that most sexually involved therapists make this mistake only once. Since psychotherapists treat, perhaps, several hundred patients in their careers, even sexual involvement by every therapist with one of his or her patients would create a percentage of sexually involved patients that is far smaller than 6.4 percent.

Gartrell et al. suggest that, even including cases of multiple sexual involvement by therapists, the total number of reportedly affected patients in their sample was 144. The psychiatrists in the sample had been in practice an average of 11.2 years. This results in a rate of patient-sexual involvement of 8.9 patients per 1000 psychiatrists per year, or an incidence of 248 patients becoming sexually involved each year with the nearly 28,000 psychiatrists practicing in the United States. One might assume, conservatively, across a very diverse set of practices that each psychiatrist sees an average of 25 new patients each year. If this assumption were correct, then 248 out of 700,000 new patients seen yearly become sexually involved with their psychiatrists. The odds, then, of becoming sexually involved with a psychiatrist, given that a patient kept an initial appointment, would be once for every 2823 new patients seen, or a likelihood of 0.035 percent. Furthermore, to give every assurance that the problem is not being minimized, one could assume that each psychiatrist sees an average of only 10 new patients per year resulting in an estimate of only once per 1129 new patients seen, or 0.09 percent.

The Limits and Achievements of Sampling

There are several issues of potential sampling bias that may limit generalizations. These issues may involve motives that might influence a victim to come forward or to decline an interview, to re-enter therapy, to reveal a past therapist-patient sexual involvement, or that might influence a practitioner to complete or withhold a questionnaire. These sampling issues theoretically limit extrapolations to a wider population of patients and practitioners in terms of incidence in the population or causal sequence in the population.

Despite potential limitations, the research to date has achieved a critical goal: The research has convincingly disproved the non-existence of the problem of therapist-patient sexual involvement, demonstrated that harmed individuals exist, pointed to certain commonalities among the known (self-selected) victims and perpetrators, and proposed a clinical syndrome which may result. However, the patient-sampling and therapist-sampling approaches have both largely convergent and divergent findings concerning the question of unharmed survivors. Gartrell et al. (1986) found that the psychiatrists in their sample provided data suggesting that 42 percent of the survivors may have suffered no undue harm from therapist-patient sexual involvement. This contrasts sharply with the Bouhoutsos et al. (1986) finding of less than ten percent unharmed survivors. Whether this divergence is the result of biased reporting by the psychiatrists sampled by Gartrell et al., or biased sampling of the victims by Bouhoutsos et al., is unknown and probably unknowable.

Despite its problems, though, the existing research supported efforts to revise the Ethical Principles of Psychologists. As Nagy (1990), chair of the Task Force to revise the ethics code, has stated, “Research by Pope and Bouhoutsos, and others, highlights the profound psychological harm which can result for the person who becomes sexually involved with his or her therapist” (p. 41). For this and similar purposes, those inferences which cannot be drawn from the sampling-based designs discussed above may not have

significant clinical application.

A Third Source of Data: Research That Unobtrusively Tracks The Behavior of Populations

The research discussed above has relied on sampling strategies in which subjects were asked to cooperate with a researcher, leading to inferential questions concerning unknown degrees of compliance in the population from which the samples were drawn. An alternative strategy looks at the naturalistic behavior of populations. When one investigates the naturalistic behavior of populations, rather than trying to sample them, one may observe fewer phenomena but be better able to understand what has been observed. For example, complaints filed to all possible sources—ethics committees, licensing boards, and civil and criminal courts—could be tallied and used to track the numbers and characteristics of known victims and perpetrators. These findings might be integrated with other data concerning the number of individuals practicing psychotherapy and the number of patients being treated to provide estimates on the minimum incidence and changing patterns of therapist-patient sexual involvement and harm .

That primarily, if not exclusively, damaged individuals would be accessible from such methods would be clear (not all damaged individuals file complaints), while complaint-filing behavior seems to covary with the degree of media attention devoted to the problem (Schoener, January, 1991). Such a tally would be informative, though, of the lower limit of the numbers of damaged patients and perpetrating therapists. Further, as media attention, professional support (Committee on Women in Psychology, 1989; State of California Department of Consumer Affairs, 1990), and high malpractice awards continue to encourage the filing of complaints, one would expect victims to continue to come forward at an increasing rate. If a decline were to occur in new reports of abuse, one might have reason to believe that the incidence of therapist-patient sexual involvement had actually begun to decline.

Ideally, a clearinghouse would receive reports from insurance companies, state licensing boards, and civil court actions. Such data would lead to conclusions regarding possible offender or victim profiles, or, perhaps, therapeutic process events which may be precursors of sexual victimization. Thus far, inferences along these lines have relied largely on anecdotal reports or sampling of unknown validity. Findings from this proposed population approach should provide support for inferences which have already been drawn.

Several authors have reported on previous, smaller scale attempts to gather population data from insurance companies (e.g., Brownfain, 1971; Cummings & Sobel, 1985), or state licensing boards and state association ethics committees, either internationally (Gottlieb, Sell, & Schoenfeld, 1988; Sell, Gottlieb, & Schoenfeld, 1986) or in a single state (Vinson, 1987). Some of these findings have used descriptive, rather than inferential, statistics because the response rate from ethics committees and state boards, for example, “so nearly approximated the population [of agencies] surveyed” (Sell et al., 1986).

However, there are limitations for these data also. The proposed clearinghouse would, in some cases, receive duplicate reports from the various sources. Sell et al. (1986) reported a study in which researchers did not have access to names of accused perpetrators or reporting victims, making the researchers unable to identify instances in which duplicate reports had been filed to both a state board and a state association ethics committee. Some cases known to victims, attorneys, health care organizations, ethics committees, state boards, and insurance companies may overlap.

In other cases, however, only a single source would report, and in some instances, no reports would be forthcoming because in the real world “deals” may be made. A sexualizing practitioner may evade licensing sanctions in return for agreeing to a negotiated settlement. A staff member of a health care organization may agree to relinquish an appointment, the health care organization may quietly pay a claim, the plaintiff may agree to

generate no adverse publicity and to file no formal complaints to state boards or ethics committees.

The advantage of such a population-based approach, however, is that the “sampling” of the population would be naturalistically made, by reinforcement contingencies that may be fairly well understood. Individuals who wish to take some action about the damage they believe befell them would come forward, while individuals who did not perceive themselves to have been harmed would not. Individuals would be motivated to come forward for reasons other than those which drive volunteers to participate in case study research. Motives would include a wish to punish offending therapists, a wish to protect future victims, a wish for financial remediation, a wish for an objective analysis of what had taken place, or simply a wish for help. The findings would be more circumscribed, and the implications could still be argued—harm could have occurred but be unknown to the victim, severe harm could make the victim afraid to take any action, or the very process of taking action may be perceived as too intimidating or otherwise daunting by the victim. One would know that the overall incidence of therapist-patient sexual involvement had not been studied and that unharmed individuals likely played no important role in the data gathering.

One would not, at the conclusion of this project, know how harmful is therapist-patient sexual involvement, i.e., how many of all those who experience it are harmed by it, under what circumstances harm occurs, who is more or less susceptible to harm, how does this kind of sexual relationship compare in its adverse effects with mundane sexual relationships, and so on, but one would have a working idea of the minimum number or percentage of patients who experience harm and therapists who perpetrate it. Furthermore, such population data would reflect changes in the rate at which new complaints are filed, the rate at which they are found meritorious, and any changes in the rate at which individuals of various genders or sexual preferences file complaints involving same or opposite sex therapists. Recall Schoener’s (1991) observation above that practitioner incidence surveys do not allow for inferences concerning changing rates of new offenses. These surveys are also unable to identify changing patterns in terms of gender or sexual preference-linked offenses, and so on, since they typically inquire about the practitioners’ entire professional history.

Ultimately, non-sampling, population based, naturalistic approaches may prove complementary to case study sampling-based methods. More than any other desirable attribute, population approaches benefit from their unobtrusiveness: The data are gathered independently of any decision by subjects either to cooperate with or to avoid researchers. Altruism towards data gatherers, a significant motive which makes survey research possible, has no relevance in naturalistic studies. The data come to light, instead, because of a chain of events which had been set in motion when the victim discovered that he or she had been harmed and which culminated in the filing of one of several types of complaints. Considering the present and future need to track changing trends in the now established problem of therapist-patient sexual involvement while expending reasonable resources, and protecting patients’ privacy, this proposed direction may be the most effective.

Conclusion

This paper examined three sources of data on therapist-patient sexual involvement: samples of patients, samples of therapists, and naturalistic observations which are not based on sampling. Each contributes to our understanding of the degree of harm, nature of harm, and incidence rate of therapist-patient sexual involvement. However, each source of data has inferential limits, some of which have been identified. Findings of clinical applicability have been more available than those which can generalize to the larger population. Thus, statements of likely degree or nature of harm from therapist-patient sexual involvement in the population, relative to other sexual relationships or other sources of psychological

trauma, or of the incidence rate, may not be attainable by extant methods. Biased sampling caused by motivated self-selection of research participants has been identified as a recurrent threat to validity. This limits our empirically based understanding of the nature of therapist-patient sexual relationships, and the harm and victimization caused by them, but not necessarily that of the nature of harm sufficient to create ethical standards or treat survivors.

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