

**Research Methods
in
Trafficking in Women and Children
A Critique of AFESIP's Ivory Tower Approach**

Part – IV of a Series

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ABSTRACT

Using statistical estimations based on actual counts, Steinfatt, Baker, and Beesey (2002) as Part-I of this series estimated the number of sex workers in Cambodia in 2002 as 20,829 with 5,250 in Phnom Penh. They also estimated that 2,488 women and children had been sexually trafficked in Cambodia.

Since the late 1990s, a much higher figure of 80,000 to 100,000 trafficked women and children in Cambodia circulated in official reports in Phnom Penh. Part-II is an unfinished paper dealing with the specifics of how Steinfatt traced the 80,000 to 100,000 number back to its origins, and is included as the first section of Part-III.

Part-III (Steinfatt, 2003) reports the results of a detailed count of trafficked persons in Cambodia conducted throughout the country, both in terms of underaged workers and those working by force, fraud, or coercion. Funded by a grant from USAID, it summarizes the more detailed work of the unfinished Part-II investigating the origin of the 80,000 to 100,000 figures, and follows them to their sources. The USAID study estimated 18,256 sex workers throughout Cambodia with some 2,000 of them trafficked. It offers recommendations on U.S. policy toward trafficking in women and children in Cambodia and Southeast Asia, based on the observations of trafficking venues, the motivations of the individuals involved, and the methods used in trafficking.

This paper is Part-IV of the series on trafficking in persons in Southeast Asia. As such it considers issues related to definitions of trafficking, the effects of those definitions on research and counts, and research methods used in conducting trafficking research. It also presents more details of the methods used in previous reports and discusses why those methods were selected for use. The methods are discussed in the form of a critique of a detailed response to Parts-I and III of this series.

This paper was written following an international roundtable held at the Juliana Hotel in Phnom Penh on 11 May 2004. Sponsored by the Asia Foundation, it was called to consider the methods and quality of Parts-I and III, and of all known previous research on human trafficking numbers in Cambodia.

This Part-IV paper discusses field research methods in trafficking in persons in the context of questions about those methods raised by AFESIP, a regional NGO. As such, this critique presents its discussion of research methods within the milieu of forces operating that have a financial stake in the size of the number of victims found, forces which can and do have a negative impact on the overall goal of unbiased research in this area.

BACKGROUND AND EXECUTIVE SUMMARY

Steinfatt's Previous Research on Sex Work and Human Trafficking: The Number of Trafficked Women and Children

I have been collecting data on sex work and HIV/AIDS in Southeast Asia since 1988 beginning in Thailand. In 1990 I was invited by the Chulabhorn Research Institute to be one of only two fully sponsored U.S. Social scientists to present their research on health communication and sex work to the *International Congress on AIDS in Developing Nations* in Bangkok. The Chulabhorn Research Institute and the Thai government sponsored the *Congress*. I presented the results of the three studies on sex work and AIDS that I had completed at that point.

Over the past 16 years since 1988, my work has evolved from health communication and HIV/AIDS to sex work as a major factor in the spread of HIV, and then to trafficking in women and children. Between 1990 and 2004 I presented the results of additional individual studies of sex work, sex workers, trafficking, trafficked women and children, managers, owners, customers, and ancillary employees, and methods of gathering data about each, at academic conferences and in meetings with Thai, Cambodian and US government officials. In 1999 I published a chapter on the politics of sex work and AIDS in Southeast Asia with a colleague from UNESCO in a major book on the politics of AIDS (Steinfatt, T. M. and J. Mielke, 1999). Also published is my major ten year academic study of foreign oriented sex work in Thailand, *Working at the Bar* (Steinfatt, 2002), which synthesizes the findings of many of my studies of Thai sex workers up to 1999.

I began work on human trafficking in Vietnam in 1990, and have collected data on human trafficking in Cambodia since 1996. The Cambodian research has been funded by USAID, by the University of Miami, and by the U.S. State Department. The Asia Foundation sponsored and conducted an International Roundtable in Phnom Penh in 2004 where one of its two sessions focused on my research. I am an independent scholar with no particular interest in whether the trafficking numbers are high, low or in between. I want them to be accurate and strive for independent accuracy in my research. The Asia Foundation International Roundtable is discussed below.

US Estimates

Since Richard's (2000, p. 3) publication of a CIA estimate of 700,000 to 2,000,000 trafficked women and children per year around the globe, those figures have been accepted by some as accurate and criticized by others as unsupported. They are often given as 'at least 700,000.' This estimate is represented as cross border trafficking, exclusive of internal trafficking within individual countries. These estimates may refer to 1997, since in his prepared remarks on the role of intelligence services in a globalized world for the Stiftung Conference in Berlin on 21 May 2001, John C. Gannon, Chairman of the National Intelligence Council of the United States stated:

The CIA estimated that in 1997 alone some 700,000 women and children were moved across international borders by trafficking rings. Some NGOs estimate the number to be significantly higher. The US Government also estimates that each year the worldwide brothel industry earns at least \$4 billion from trafficking victims.

The US Intelligence Community assesses that trafficking in women and children is likely to continue at high levels in the years ahead given the large profits, relatively low risk, and rare convictions for traffickers. Increased international attention, countermeasures, and law enforcement will be required to stem this heinous activity (Gannon, 2001).

No report currently exists of how these numerical estimates were obtained, but it was not the result of a study by Richard (personal communication with Richard, 2004). The numbers were apparently given orally at a CIA briefing entitled *Global Trafficking in Women and Children: Assessing the Magnitude*, held in April of

1999. It is not known how the CIA compiled a reliable estimate of the extent of global human trafficking in 1999 or before, or how it was in a position to do so, given the CIA's de-emphasis on ground-based sources of information. U.S. Embassies around the world report country numbers on trafficking in persons to the State Department for the country in which they are based, but these reported numbers come from sources that may or may not be in a position to know, and more often than not, do not result from empirical studies. People and organizations usually provide these estimates to the U.S. Embassy based on what they think the number is likely to be. Whether or not these individual country numbers formed the basis for the initial 700,000 to 2,000,000 estimate does not appear to be known outside of the CIA. It seems more likely that the CIA numbers are based on figures reported by immigration officials around the world since the numbers are represented as cross-border numbers and not internal country numbers. How immigration officials, or any other sources, would know the number of persons trafficked into or out of their countries is not known, given that these estimates do not represent numbers of arrests for trafficking in persons.

The goal of my research is to provide valid, empirically based estimates of the number of women and children trafficked in Cambodia within the limits of statistical estimation. Trafficking in persons is a very serious problem. It is only through understanding the extent of the problem, and specifically when, where, and how it exists, that proper measures can be implemented to solve the problem. If we can measure the problem, that measurement in turn provides some of the 'when, where, and how' information. In addition, if the number of trafficked individuals is not known, then it is not possible to know whether the problem is receding or growing, nor to evaluate the effects of programs designed to reduce trafficking.

Time, effort, and money spent on a problem with no way to measure the problem's extent, and no way to evaluate the effects of anti-trafficking efforts, is at best money questionably spent. There is a perception among some NGOs that smaller numbers of trafficking victims means smaller budgets and less importance attached to the problem. Smaller budgets mean fewer jobs for NGO employees, often Western foreigners. This produces pressures from some NGOs for the numbers to be 'large,' regardless of their actual size. One could also interpret smaller numbers as evidence of progress, or of the application of more accurate measurement methods.

At the 2001 *Second World Congress Against Commercial Sexual Exploitation Of Children*, held in Yokohama, Japan, the trafficking numbers for Cambodia as presented by Cambodian NGOs, claimed the existence of 80,000 to 100,000 trafficked women and children *per year* in Cambodia alone. These numbers were also printed in the annual reports of some Cambodian NGOs in the early 2000s. Details of these numbers, their sources and their veracities, are discussed in Steinfatt, 2003, pp. 1-5. With a population of only some 12,000,000 persons, amounting to about 00.194% of the total world population, it seemed unlikely that Cambodia could have a number representing some 11% to 14% of the claimed world cross-border international trafficking total as an internal destination total within its small land area.

The 80,000 to 100,000 figures printed by some NGOs refer to an official Cambodian government report (the Cambodia Human Development Report, or, CHDR, 2000) prepared by the Ministry of Planning with the assistance of UNDP. But in that report the figures are given as the *number of sex workers*, not the *number of trafficked women*. The process taken in converting the *number of sex workers* in the original source to the *number of trafficked women* in the NGO reports is detailed in Steinfatt, 2003, pp. 1-5, following the process through which these numbers came into being. Briefly, numbers in CHDR (2000) that referred to *sex workers* were changed to refer to *sex slaves* in an NGO report. This NGO falsification was then interpreted to mean *trafficked women and children*. Beyond this equivocation from *sex worker* to *sex slave* to *trafficked women*, no evidence has been presented that these CHDR numbers for *sex workers* were actually accurate or even remotely close to the number of sex workers in Cambodia. The original UNDP/government numbers for Cambodia were based either on unsupported guesses or on typographical errors. No actual study of the problem ever found such numbers for Cambodia. In 1997, the National Assembly of Cambodia, similar in Cambodia to the US Congress in the US, commissioned the best empirical study prior to 2002. It found some 14,725 sex workers in brothels

across Cambodia, but did not attempt to measure the number of indirect sex workers or to provide an estimate of the number of sex trafficking victims. It stated that *trafficked* women in Cambodia are only found in direct sex work venues, not in indirect, a finding also generally verified in my research. *Direct* and *indirect* are common divisions of sex work activity (see Steinfatt, 2002).

Direct commercial sex

Refers to workers who sell sex directly, where no other service is offered as a pretense, and generally with no maintenance communicative interaction with the customer prior to sex, and no pretense that the encounter is for any purpose other than sex. It is the type of commercial sex found in a brothel (Steinfatt, 2002, p. 383).

Direct commercial sex does not involve communicative interaction between the sex worker and the customer prior to sex. Commercial sex with a massage prior to sex but no communicative interaction is classified as direct.

Indirect commercial sex

Refers to workers who sell sex indirectly, in that they offer other services such as companionship in a bar or cocktail lounge, with sex elsewhere for a fee as an option. Indirect commercial sex involves some form of maintenance communicative interaction with the customer prior to sex (Steinfatt, 2002, p. 386).

Indirect commercial sex involves a period of communicative interaction between the sex worker and the customer prior to sex.

The Steinfatt Reports

In 2002 and again in 2003, I conducted empirical studies designed to count the number of sex workers and sex trafficking victims in Cambodia (Steinfatt, Baker, & Beese, 2002; Steinfatt, 2003). In 2003 we surveyed every city in Cambodia, most major towns, and a large sample of villages and rural areas, including travel along the Vietnamese border checking each of the major and several minor crossing points from central Mondul Kiri to Kep, and along the Thai border checking each of the major and several minor crossing points from Koh Kong to Oddar Meanchey. The results indicated that the best estimate of the number of trafficked persons at a given point in time in Cambodia is in the range of 2000 (Steinfatt, 2003) to 2500 (Steinfatt, et al., 2002), with an upper 95% confidence bound around 2,750. These two studies were reviewed at the Asia Foundation Roundtable, discussed below.

The Swingle and Kapoor Report Appears

About ten days prior to the 11 May 2004 roundtable, two papers were released under one cover by the Phnom Penh office of AFESIP International – an anti-trafficking NGO formed in 1996 by Somaly Mam and Pierre Legros, with offices in Cambodia. These two papers were written at the request of Mam and Legros who were fully apprised of their contents and approved of those contents. Without such approval the papers would never have been released by AFESIP as AFESIP papers, which is the normal policy in any such organization. Its French acronym stands for *Acting for Women in Distressing Circumstances*. The AFESIP response offered methodological comments only on the Steinfatt papers (Swingle & Kapoor, 2004a; containing: Swingle & Kapoor, 2004b, and, Kapoor, 2004). References to “AFESIP” in this paper, refer specifically to Somaly Mam and Pierre Legros, since they make all organizational level decisions for AFESIP.

The Asia Foundation International Roundtable

At US Embassy urging, the Asia Foundation, a highly respected international granting and research organization and the formal conduit for a large proportion of US Foreign Aid to Asian countries, sponsored a roundtable principally as a discussion of my research. I was one of four principal speakers on the first of two panels entitled *Existing Empirical Research and Knowledge on Human Trafficking*. The others were Dr. Hean Sokhom, Executive Director of Cambodia’s Center for Advanced Study; Dr. Supang Chanthavanich, Director of the Asian Research Center for Migration of Chulalongkorn University, Bangkok; and Dr. Janice Madden,

Professor, former Vice Provost for Graduate Education and Director of the Alice Paul Center for Research on Gender and Women of the University of Pennsylvania, and an expert on statistical methodology and research methods.

Swingle and Kapoor's AFESIP response was given to all panelists prior to the session and each considered that report in the discussion on my two studies. Dr. Madden was assigned to review and critique the methodology and results of all studies of trafficking numbers in Cambodia, and specifically the Steinfatt methods. She produced a paper for the session (Madden, 2004) critiquing all previous research and numbers estimates on the extent of human trafficking in Cambodia, including the 1997 National Assembly study, and the Steinfatt studies. She also included the AFESIP response, which is not empirically based, as it comments on empirical methods. She concluded that "The two Steinfatt studies are clearly the most scientifically rigorous of the analyses I reviewed" (Madden, 2004, p. 9). Her paper makes several good suggestions, and I intend to incorporate these into a publication on human trafficking in the near future.

Dr. Frank Wiebe, Chief Economist for the Asia Foundation and an expert statistician, moderated the roundtable discussion and offered additional comments on methodology. In Madden's oral remarks at the roundtable she referred to the Steinfatt studies and numerical estimates as the "best by several degrees of magnitude" of all studies done on trafficking numbers in Cambodia, labeling them the "Gold Standard" against which future studies of human trafficking numbers in Cambodia should be measured. The other panelists and the moderator did not disagree with Madden's assessment, and each provided good suggestions. In addition, the U.S. State Department accepted the findings of the Steinfatt Reports as representative of the general level of trafficking of women and children in Cambodia. These reports were used by the US State Department in its annual Trafficking in Persons Report (US State Department, 2004) in creating the ranking for Cambodia in the 2004 TIP Report.

While Swingle and Kapoor (2004a) suggest that my studies have underestimated the trafficking numbers in Cambodia and are deficient, the judgment of the first International Roundtable participants and the responses by Madden refer to these methods used in my studies as "a particularly creative approach," and "ingenious" (Madden 2004a, pp. 5 & 9). The Panel had also received and reviewed Swingle and Kapoor's AFESIP sponsored Response to my methods.

The Asia Foundation International Roundtable judgments dismissed the attacks listed in Swingle and Kapoor's AFESIP response to my work. The fuller consideration of Swingle and Kapoor's points, below, may serve to illustrate those authors' lack of understanding of human trafficking in Cambodia apparent in their response, and by implication, within AFESIP. Swingle and Kapoor's response questions many methodological points in the Steinfatt studies. Our *Brief Critique* of that response, immediately below, is intended only as a summary of major points. A full critique of the AFESIP response, including consideration of more minor points, is then laid out in the *Detailed Critique* below that. In order to maintain continuity in the discussion it was not possible to keep entirely the same order in the extended discussion in the *Detailed Critique*, as that appearing below in the *Brief Critique*.

Brief Critique of AFESIP's Response to the Steinfatt Reports

1. **Introduction and Research Intent.** Our main concern in Steinfatt, et al. (2002) and Steinfatt (2003) is to assess the number of trafficked women and children in Cambodia. In finding accurate numbers we are also able to identify the locations and many of the methods of trafficking in persons. The identification through empirical observation of locations is an important factor in rescuing victims and finding traffickers. Knowing the numbers allows the government to assess and set trafficking policy in the most efficient manner and to evaluate the effects of trafficking reduction programs. While number crunching methods of estimating human trafficking are important and valuable, they seldom if ever provide information on the location of specific trafficking sites not already in evidence.

2. **Moto Drivers.** The use of moto drivers as informants provided one of the best possible ways of obtaining data on trafficked women and children during the time period studied, 2002 and 2003. We selected this method after careful consideration. Our studies showed it to produce data that are more reliable and valid than those produced by any other current method. Recognized experts in research methods have examined our methods and believe the methods are innovative and accurate.

3. **Sudden conversions.** AFESIP published and disseminated a very detailed response to our work written by two of their associates, Dr. Joseph Swingle of Wellesley College, and Aarti Kapoor, AFESIP's legal director in Cambodia. AFESIP represents this response as concerned with the methods and reasoning of the Steinfatt Reports. While many NGO and government reports from 1998 onward provided major overestimates of the number of trafficked women and children in Cambodia, and each stated no method whatever, none of the major flaws in these earlier reports, flaws detailed in Steinfatt, Baker, and Beesey (2002), created sufficient misgivings for AFESIP to respond. But the sound research methods used in the Steinfatt Reports attracted their attention, to the point that they criticized them heavily as invalid. *Why would AFESIP publish their concerns with sound methods now, when they published no concerns about completely unsound methods before? Sudden conversions require explanations.*

4. **The Phantom Cities.** Swingle and Kapoor argue there are large cities in Prey Veng, Pousat, and Takeo that were not represented in our reports. Yet such places do not exist as large cities. This was fully addressed as the first five pages of our 2003 report (Steinfatt, 2003) which Swingle and Kapoor had in front of them and were critiquing. Yet the Swingle and Kapoor critique entirely misses the fact that we spent five pages on this point, the first five pages, in the second of only two of our papers that Swingle and Kapoor chose, or were directed, to critique. The 'large cities' alleged to exist by Swingle and Kapoor in fact villages or small towns, and each was studied. Thus, in addition to failing to read our section on *Cities, Towns, and Villages Outside of Phnom Penh* in our 2003 report, which Swingle and Kapoor were supposedly critiquing, the AFESIP leadership also apparently did not have sufficient knowledge of the cities, towns, and villages of rural Cambodia to know that the basis of their argument could not have been true. How could they have missed this point if Somaly Mam and AFESIP had in fact been rescuing trafficked persons throughout Cambodia, how could they fail to have missed the difference between 'large cities' and towns or villages if these rescues actually occurred?

5. **Ten Erroneous Charges in Swingle and Kapoor.** Swingle and Kapoor did not carefully read our studies prior to responding to them. It is clear from their response that they did not understand our methods. In many cases the arguments they provide are not arguments against our actual methods, but against methods they created that they then claim to be our methods.

As examples, they erroneously state:

- a) That we sent moto drivers into indirect venues to collect data: *We did not.*
- b) That we had moto drivers approach the first brothel that they see: *We did not.*
- c) That we had moto drivers go out the door of one brothel and then into the one next door: *We did not.*
- d) That moto drivers were given a notebook into which they recorded entries while in the brothel: *We did not give them notebooks and have never collected data in that way.*
- e) That we do not present reliability figures for moto drivers' reports: *We do present these reliabilities.*
- f) That we did not consider late arrivals and early leaves among indirect workers: *We did consider such arrivals and leaves, and we did so carefully.*
- g) That we ignore the issue of discreteness in sex work: *We considered discreteness carefully, while Swingle and Kapoor confuse "discreteness" with being hidden.*
- h) That we use random estimates as data: *We gather our data carefully and it accurately represents the numbers of sex workers and trafficked women and children.*
- i) That we use unqualified individuals to gather data: *We use highly qualified persons to gather data.*
- j) That we record rumors and hearsay and present them as reliable data: *We carefully record trace measures of our observations, which are valid representations of the concepts measured.*

6. **Methodological References Ignored.** In addition to the methods statements in the Steinfatt Reports, our 2003 report also lists references to the methods I have used since 1988 in my Thailand studies (Steinfatt, 2002). The AFESIP principals and respondents *could not have read these methodological references* given their comments. Instead, they created their own accounts of those methods, and then presented these accounts as real, and attributed them to my work. This likely would not have happened had Joe Swingle and Aarti Kapoor checked the references I provided in the papers to which they are responding.

7. **Steinfatt responses to AFESIP concerning Methodology are Ignored.** After AFESIP contacted me initially by e-mail in late November 2003 about the two reports, I sent several e-mails to Swingle, Kapoor, and to Somaly Mam via Pierre Legros in December of 2003, and offered to provide them with any details or clarifications they wanted or needed concerning anything about the methods used in my studies. Neither they nor anyone else from AFESIP ever contacted me in response to this offer with a request for methodological details. Swingle and Kapoor's creation of and attribution to me of methods I did not use, must be understood in light of my offer of clarification of any methods to them. It also must be understood in light their failure to request clarification on any details on which they were unclear, in addition to their initial failures to read and understand my studies, and their failure to read the methodological references provided in those studies.

8. **Charge of "ethically objectionable" based on a lack of professionalism.** Based on Swingle and Kapoor's misunderstandings of my methods and work, on their failure to read my reports carefully, on their failure to read the listed references to my previous work, on their failure to accept my offer of any clarification they needed with respect to my methods, and on their subsequent creation of methods that they attribute to me and then attack, they then imply that *my* work is ethically objectionable. Their claimed objection here is that my work does not state *limitations they claim to exist based on their own creations that they represent as mine.* Their statement regarding "ethically objectionable" behavior, as well as other similar statements in Swingle and Kapoor, constitutes a serious violation of professionalism on the part of Swingle and Kapoor, as well as by Somaly Mam and Pierre Legros who were informed of this through our email exchanges yet approved the release of the Swingle and Kapoor paper. These erroneous claims and Swingle and Kapoor's fallacious method of argument bring neither credit nor credibility to them nor to Somaly Mam and Pierre Legros.

9. **Our misstatement.** There is a misstatement on p. 5 of Steinfatt, et al., 2002, in which our wording implies that we asked moto drivers to *present a number to managers*, rather than *describing their interactions with managers*. We thank Swingle and Kapoor for commenting in a way that brought this to our attention.

10. **Conflating sex work with trafficking.** Kapoor's argument that all sex workers, voluntary and otherwise, must be regarded and counted as trafficked women, based on her reading of the *Victims of Trafficking and Violence Protection Act 2000*, is fatally flawed. It fails to consider the effect of the serious usage problems in the definition of SEC. 103 (9) together with SEC. 103 (3) of Public Law 106-386, the *Victims of Trafficking and Violence Protection Act of 2000*, as applied.

11. **Relying on evidence that is only alleged to exist.** Kapoor and AFESIP consistently refuse to publish any study based on their collected data. Yet Kapoor makes reference to knowledge gained from this unpublished research and cites it as evidence for her position. *Evidence that is not in evidence is not evidence.* AFESIP has never provided any method statement for this claimed evidence nor any summary of it, so only AFESIP people have access to what they cite as evidence, and only AFESIP people know how the alleged findings they cite as evidence were produced. This is not a form of argument but rather a form of propaganda.

12. **AFESIP's Ivory Tower approach.** Many arguments Swingle and Kapoor use against my methods stem from a distinct misunderstanding of how trafficking for sexual purposes operates in Cambodia. They do not understand the nature of brothels, especially from the viewpoint of the brothel owner or the customer. AFESIP's limited perspective appears to be from the viewpoint of the specific trafficked women brought to them by others, not from evidence otherwise discovered by them. While the perspective of such victims is very important to our knowledge of sex trafficking, it is still only one perspective from an exceptionally limited point of view directly controlled by traffickers. Taken alone, it does not provide sufficient understanding to create a viable plan to conduct research on brothels or to reduce the spread of trafficking in persons and to restore the human rights taken from these trafficked women. Such victims seldom know where they were held, or much else beyond the specifics of the circumstances they were able to observe from an intentionally limited perspective. These victims are, however, true experts on how they were treated personally and how it made them feel.

A good understanding of the enemy is usually an important requirement for defeating that enemy. Conducting research, or responding to the research of others, from the exceptionally limited perspective and understanding of brothel-based sex work that Swingle, Kapoor, and AFESIP demonstrate in their response, is purely an Ivory Tower approach, with little relationship to the realities of the brothel and trafficking. It is difficult to understand how someone such as Somaly Mam, who claims to have conducted vast numbers of rescues of trafficked women, can misunderstand how trafficking operates to this degree. It would be exceptionally difficult to rescue trafficked individuals if one were operating based on such knowledge.

13. **AFESIP's ability to conduct competent research on human trafficking.** In addition to their lack of understanding of how trafficking for sexual purposes operates in Cambodia, and their lack of familiarity with population centers in rural Cambodia, Swingle and Kapoor as well as Mam and Legros also demonstrate a general lack of knowledge of, and experience with, actual field research and with data collection methods in less developed nations. Attempts to track illegal activities in a less developed country often require innovative methods based on culture-specific knowledge. Research methods that may work in obtaining specific types of knowledge in developed nations – perhaps middle class female white interviewers with a clip board, camera, and printed forms with a defined skip interval asking US households about TV viewing habits – cannot necessarily be exported, in whole or in part, to less developed societies with different cultural norms, beliefs, and practices in situations involving possible illegalities.

Detailed Critique OF AFESIP's Response to the Steinfatt Reports

Organization of our Detailed Critique and Initial Comments

AFESIP's official response to the Steinfatt Reports is divided into two parts. Part I is a jointly authored response on methods by Swingle and Kapoor, while Part II is nominally on definitions and is authored by Kapoor, who is currently Somaly Mam's internal go to legal person. The entire report would likely have been checked for accuracy by Somaly Mam and probably Pierre Legros as well, as it is an official position paper representing AFESIP.

This more Detailed Critique of their response discusses additional minor points raised by AFESIP, as well as filling out the discussion of the thirteen major points listed above. *In the outline used in this Detailed Critique we attempted to follow that of the 13 brief points above. But the remainder of Swingle and Kapoor's critique does not use the same order as in the beginning of their paper, so we were unable to do that. Our comments below employ their order.*

While this Critique could be shorter and more organized if it followed a more ordered format, I chose to follow the organization presented in AFESIP's response here, so readers would be able to follow the arguments more easily, if desired, from that response to this critique. While this results in a degree of circularity, since the AFESIP response raises issues, leaves them, and then returns to them again, the order here makes it clear that each of the coherent points raised by AFESIP are answered. In this form it also provides a historical record of allegations made and answered in a politically charged methodological arena, and this give-and-take aspect may make it of interest to future readers and to graduate students learning about the realities of conducting research and presenting research results in such an atmosphere.

Swingle and Kapoor employ a shotgun negative approach in their response, a style of arguing in which one attacks almost everything possible in the other's position, in every way possible, even if the arguer has no reasoning or evidence with which to do so on many of the points. Its theory-of-use is that responding to the shotgun attack will be so time consuming that the person attacked will not be able to defend his or her work, even if most of what is said in the shotgun attack is fallacious. Swingle and Kapoor employ this shotgun negative style, attacking almost everything, perhaps hoping that at least some of their points will hold up to scrutiny. None of them do.

Dr. Swingle is in the Sociology Department at Wellesley College and is associated with AFESIP-USA. Ms. Kapoor is AFESIP International's Legal Advisor.

INTRODUCTION AND RESEARCH INTENT

AFESIP works in part with "rescued" sex workers, and that work likely provides them with an understanding of brothels largely through the eyes of these women, many of whom have suffered terrible abuses. This is a very important source of knowledge. But it is only one source. Use of this principal knowledge source leaves AFESIP reliant on a very limited set of information about sex work, largely that information provided through the eyes of previous workers who have been mistreated. Data from previous workers considered in isolation is biased in that it is obtained from a very limited in point of view – an intentionally limited point of view, limited by the traffickers themselves. Trafficked workers see and hear what traffickers allow, and sometimes want, them to see and hear. Previous workers, trafficked or otherwise, are limited in their *perceptual domain* (Steinfatt, 2002, pp. 256 - 257) with respect to their knowledge of the system. It is exceptionally important knowledge, but limited by their personal experience nonetheless, and alone is incapable of providing a full, or sometimes even a useful picture of that instance of human trafficking.

Swingle and Kapoor base their response on a concept they contrast with sampling error, and refer to it as *nonsampling error*. A more common term for this concept as they use it is *measurement validity* and it is independent of sampling error and calculation errors. Swingle and Kapoor then state:

One of the more common ways to tell lies with statistics is to overlook, intentionally or unintentionally, nonsampling error. The major problem of Steinfatt's methodology is a near total neglect of nonsampling error. As we will argue, this nonsampling error points to undercounting of unknown magnitude. For this reason, Steinfatt's estimates have to be approached cautiously (Swingle & Kapoor, 2004b, p. 2, underlining as in original).

In this quotation, Swingle and Kapoor do not call me a liar to my face, they simply state that “nonsampling error” is related to lies and then claim to associate me and my work with this nonsampling error. And they use this guilt-by-association form of argument not just once, but in multiple places in their text, such as their claims, discussed below, that our reliabilities are ‘too good to be true.’ Actual data based research by Swingle based on AFESIP’s alleged database would have been more appropriate as a response.

I welcome all argument, response to, and discussion of my work, and appreciate their written comments. But the intemperate form of *ad hominem* statements implying that my paper amounts to lies suggests that theirs is not a neutral and unbiased response. My response to this form of attack is contained in my review of their paper below, and in the deficiencies in their paper and in AFESIP’s work.

They characterize the research I have done with Baker and Beesey as:

“Tenuous estimates . . . questionable data collection procedures . . . questionable assumptions . . . and questionable statistics [that] cast serious doubt on all of these estimates . . . [with] lack of reliability and validity . . . and . . . shortcomings in . . . methodology and reasoning. (Swingle & Kapoor, 2004b, p. 2)

Measurement validity, which they refer to as “nonsampling error,” is fully considered in my research. My studies do not neglect measurement validity as will be obvious from the discussion below. I employ the measurement methods that I do specifically because they provide the most valid measurements. Any study that had “a near total neglect” of measurement validity would be invalid on its face and should be discarded immediately. It would not be something to be “approached cautiously” as Swingle and Kapoor suggest. The lack of validity they suggest here does exist, but in Swingle and Kapoor’s erroneous representations of the Steinfatt methods, rather than in the way our research is actually conducted.

II. The Steinfatt Reports

No comments.

III. The Steinfatt Estimates

No comments.

IV. Sources of Error in Steinfatt's Estimates

A. Undercounting the Number of Prostitution Venues

MOTO DRIVERS

1. *Motodop drivers versus "trained researchers"*

Motodop Drivers – Training

The issue with data collectors is whether they provide good data: Will it be the same data any other data collector would obtain (measurement reliability), and, does it correspond to the actual state of affairs (measurement validity)? One way to collect data is to engage in extensive training of data collectors. This is often done in Western contexts when the data collector is using an expansive questionnaire either in phone surveys or in face-to-face interviewing. The complexity in Western settings of choosing a random start and random skip interval in selecting the homes, and choosing the individual within the home to be interviewed, demands training and is used in some survey attempts to approach random sampling. Training of interviewers in such sampling situations is less problematic than in a foreign sex industry since they clearly are interviewers in the eyes of the respondents. The use of complex questionnaires often requires such training. Interviewers need to be trained how to use the questionnaire and its various jump points, when not to lead and when to lead the respondent, to listen carefully, record accurately, etc. When complex questionnaires, skip intervals, respondent selection within household, etc., are not involved, use of such training becomes more debatable. When the issue is illegal activity as in the present case, use of such training borders on the absurd. We are attempting to study the entire population of Cambodian brothels, not sampling American households as in the work Swingle refers to.

Training can become a major negative in gathering accurate data when the respondent is involved in quasi-legal activities. Cambodian brothels are not equivalent to Western households. The use of a visible questionnaire would be absurd in such situations, as would the visible use of a researcher's notebook, tape recorder, video camera, etc. The use of any of these devices might fundamentally change the research situation and essentially guarantee lack of validity of the data obtained.

One is faced with a choice as a researcher. If researchers are qualified and have extensive knowledge of the situations to be observed, they could do very extensive training of a limited number of interviewers, and hope that the specific situations the interviewers face when entering the brothel are covered in the training. If the researchers do not anticipate and train for all of the possible situations the interviewers may face, and the highly trained interviewers then do in fact face situations for which they are unprepared, the data is likely to substantially misrepresent the actual situation. Any actor, whether a trained interviewer or a Broadway star, method actor or otherwise, will normally be less convincing when faced with a real situation than when taking part in the structured performance for which they have been trained.

Such training methods may also place the data gatherer in substantial danger. The training of the data gatherer potentially reduces validity since it demands that the interviewer be aware of his/her role as interviewer. Training of interviewers is an excellent way of guaranteeing that one will get invalid data in a brothel setting, and can be tricky in indirect sex work settings. In our decade of research with Thai bar workers (Steinfatt, 2002, pp. 363 – 364), we found that questionnaires simply did not provide good data in Thai bars with indirect sex work, as we reported.

Suspicion is aroused in a brothel by many forms of unusual nonverbal behavior and nonverbal communication. Any data gatherer who is aware of his or her status as such must be a very good actor in order to avoid suspicious nonverbals. So the other choice, and the better method, is to use someone as a data

gatherer who is familiar with the brothel setting, familiar to the manager, and without a perceived need to control his nonverbals. In place of depending on the acting skills of a trained interviewer – and academy awards in this area are hard to come by – one can use people who do not need to act and can simply be their natural and familiar selves in the brothel. We chose the latter route while Swingle and Kapoor argue for the former. Additional insight on our type of thinking about measurement may be found in Sechrest, Schwartz, Webb, and Campbell's (1999) *Unobtrusive Measures*.

The training of interviewers also raises the issues of the qualifications of the trainer. How does the trainer come to have the extensive knowledge of brothels and brothel culture that would be needed in order to anticipate the situations that will occur and the training that is required in those situations? Knowledge gained from the perspective of trafficked women would be both biased and insufficient. Quite often they are not parties to the negotiation of their services. Why doesn't AFESIP understand this? How competent is the trainer in passing on extensive knowledge of brothels to the trainees? What studies has the trainer run to show that any training provided produces data that are valid? What validity criterion was used in these studies? My fifteen years of experience in gathering data on both direct and indirect sex work venues led us to use highly knowledgeable observers, not trained observers: knowledge gained from personal past experience rather than through training by persons who likely know less about the data gathering situation than the interviewers. We recognized that training had the pitfalls not only of potential failure to anticipate situations and dependence on unreliable acting skills, but contained the assumption that we as trainers knew more about the data gathering situation than did persons with a direct everyday acquaintance with it. Persons with even less experience than ours would have highly questionable qualifications as trainers. Who would conduct such training who had more knowledge of asking questions in brothels than do moto drivers?

The arguments raised in Swingle & Kapoor strongly suggest that no one at AFESIP involved in their critique of our work understood much about normal procedures at brothels. If they had possessed such knowledge they would have discovered immediately that trained interviewers, in the sense to which they refer to such training therein, would not only fail to gain accurate information, but would potentially endanger the interviewers as well as destroy the research operation.

Motodop Drivers – Qualifications

We did not stumble upon the use of moto drivers as data gatherers by accident. Beginning in 1996, small-scale studies were conducted throughout Phnom Penh in conjunction with my work with Jim Mielke of UNICEF (1999) on HIV and sex work in Cambodia. We noted in our chapter (Steinfatt & Mielke, 1999) the petty jealousies and lack of cooperation with other NGOs that often characterized the work of NGOs in Phnom Penh. We spent time over a period of five years refining the technique of gathering data through moto drivers in brothel settings prior to employing it in our studies. Moto drivers are the most qualified observers of brothels to which it is possible to gain easy access. Many moto drivers use brothels as their principal hangout. The CDC, the Centers for Disease Control, lists moto drivers, as a group, at very high risk for HIV due to the extensive use of brothels by moto drivers, even though that use has declined since 1998.

If Swingle and Kapoor have comparative data that shows that training interviewers to anticipate all of the situations and avoid all of the nonverbals that might arouse suspicion in a brothel is both possible and produces good information, we would like to see it. More power to them if they can in fact do it that way. We can't. That's why we use moto drivers: it is the best way we have found to get valid data from brothels.

Motodop Drivers – Suspicions about Customers

A researcher who openly asks a taxi driver or local man to show him all the places of prostitution in an area, including those for the very poor and those for the very rich, likely raises some doubts about his true motives (Swingle & Kapoor, 2004b, pp. 5).

That is an interesting conjecture. I drove for Courtesy Cab in Lansing Michigan while finishing my B.S. in Mathematics and Statistics at Michigan State many years ago, and long-time drivers told me they often received those questions. I subsequently received such questions, and learned how to provide the locations desired from other drivers. They were not everyday questions but they commonly occurred. I suspect that neither Swingle nor Kapoor has ever driven a cab. Why would a moto driver be suspicious of someone asking them to go to a sex work venue? It is one of the most common questions they receive, especially from foreigners. Moto drivers are normally quite happy with such customers since there is the prospect of a big tip, plus a kickback from the brothel if the customer visits a sex worker. If Swingle and Kapoor have collected data on the suspicions of Cambodian moto drivers they should present them. I don't think they are very familiar with such drivers. Drivers are interested in fares and could care less about concepts such as true motives, whatever those are, and the consequences of this imaginary concept for the behavior for others. How Swingle and Kapoor propose to measure such notions – concepts that may exist largely in the minds of some college professors and barristers rather than within moto drivers – is not presented by them.

Motodop Drivers – Locations and Passengers

Why should it be assumed that taxi drivers know all such locations (Swingle & Kapoor, 2004b, pp. 5).

In general, any brothel not known by taxi drivers will do its best to become known to them. Touts from brothels are constantly buttonholing moto drivers to induce them to bring customers to them. AFESIP does not seem to be aware of this, which raises questions about their claims of rescuing women from them. Did we find every brothel in the city using moto drivers? Of course not, and that is why we used the MTD venues and City Block Sampling in addition to the moto drivers mapping.

And what of the very rich who do not rely on taxis (Swingle & Kapoor, 2004b, pp. 5).

This is a legitimate point. Our moto driver method could, though not necessarily would, miss locations that taxis are seldom to never used to approach. Our drivers were each asked to show us locations used by the rich, and taxi drivers normally know such locations through their everyday observations, even if passengers do not request to go there. The existence of venues frequented by the rich and unknown to moto drivers is not in evidence, and our second two measurement methods, MTD venues and City Block Sampling, would have given us a shot at finding them if missed by the first method. The existence of such locations is indeed speculative, and if they did exist would add little to nothing to the total of trafficked women and children. Customers of sex work seek variety. A room full of the same women and children would not provide much in the way of motivation for repeat customers. If such a room existed it would most likely be stocked from a local brothel with persons we have already counted. This would allow for a constant change in the sex workers available, which is the usual situation. If it were a room with persons isolated from the rest of the trafficking scene, the small number of persons involved is very important for these people as human beings, but would not add appreciably to the total numbers. Their possible presence is accounted for in the high-end figures we present. As Dewey said, “a difference that *makes* no difference *is* no difference.”

More on Motodop drivers' Suspicions about Customers

A person asking suspicious questions might not be taken to places where the worst type of prostitution may exist, i.e., child prostitution or forced prostitution. The taxi driver or local man would have legitimate concerns about his own safety and the possible reprisals that might befall him should he lead a journalist, NGO investigator, undercover policeman, or researcher to any venue, especially a hidden one offering child or indentured prostitutes* (Swingle & Kapoor, 2004b, pp. 5; underlining and asterisk added).

***Note:** The term *prostitute* is not appropriate in respectable academic research in reference to sex workers and trafficked women and children (Steinfatt, 2002, pp. 12 - 14), but I quote Swingle and Kapoor accurately when they use it throughout their work. Use of the term *prostitute* further stigmatizes trafficked women and children. They did not choose to be used in this way, and labeling them *prostitutes* creates even greater hardships for them in Cambodia and Southeast Asia. The branding of potentially trafficked women and children as *prostitutes* by Swingle, Kapoor, and others, supports the local belief system that they are soiled women and makes reintegration into their community of origin even more difficult. That in turn increases the likelihood of their return to sex work to make a living. AFESIP should know better.

With reference to the quotation above the note, we have to ask, what is suspicious about asking to go to a brothel? It is one of the most common requests moto drivers hear from a man. Moto drivers are not afraid of brothels. Outside of their disease aspects, brothels are not dangerous places for customers, and moto drivers are either customers or bring customers. Brothels can be very bad places for workers, depending on who is running them. Customers are the lifeblood of brothels. Creating an atmosphere that is perceived as dangerous by customers, moto drivers or otherwise, will guarantee a lack of customers. Brothels don't do that. Brothels, especially those with children, are set up to be as safe as possible for the customer. All those locks and special precautions on worst-case brothels are not there just to give the owner time to hide the evidence. They protect the customer long enough to appear innocent in the event of a raid, and repeat customers know that. This point makes us wonder if AFESIP personnel are actually familiar with the brothels from which they claim to rescue women.

Fears of Motodop Drivers

“legitimate concerns about his own safety?” Drivers don't fear brothels. If a driver does have worries about a specific place – because he has not been in that specific establishment or because he fears a lack of kickbacks from that place or because someone who does not like him hangs out there or, if he is with a longtime customer, because he cannot insure its safety for his customer – he will normally show the passenger the place from a distance and discourage entry. If one driver does not show the place, others likely will. Our mapping method is designed to find places that any specific driver considers ‘off limits’ for himself, through the use of multiple drivers. The MTD venues and City Block samples also help account for those possibly missed.

Worries about a journalist, NGO investigator, undercover policeman, or researcher? Do Swingle and Kapoor have data showing that moto drivers think in terms of journalists or NGO investigators or researchers? Most moto drivers would not know what most of those occupations were. And undercover policemen? The policemen who enter brothels generally are either customers, own the brothel, or get payoffs from it. The police constantly harass independent sex workers, and occasionally brothel workers. Brothel owners are seldom harassed. Moto drivers are aware of that. This whole line of argument shows a lack of understanding of how sex work operates in Cambodia by representatives of AFESIP. In order to support Swingle and Kapoor's argument here, one must assume into existence a complex chain of reasoning as presented in the five-line quotation from their response printed above, none of the elements of which are in evidence or even make sense in brothels.

Time Taken to Obtain Measurements

Obviously quick counting such as this is subject to large errors (Swingle & Kapoor, 2004b, pp. 6).

That is interesting to learn. Since we spent considerable time on cities and as much time as needed in towns, Swingle and Kapoor must be referring to our counts in villages here. We sometimes spent as little as 15 minutes in a village, and Swingle and Kapoor feel this led to “large errors” although, as usual, they do not specify a mechanism as to why this should lead to any errors, let alone large errors. Very few villages in Cambodia have sex work. AFESIP should be sufficiently familiar with brothels to know this. The exceptions are in border areas near Vietnam, from Bavet down to the Pacific Coast. Cities and towns, as defined in Steinfatt (2003), border or otherwise, always have sex work. A Cambodian village is a small place. We defined it as having no substantial buildings even in the central business area and with fewer than 250 people in the immediate built up area. Nosey neighbors, lack of a sizeable male population, lack of a transient male population, and a lack of disposable income on the part of available males, characterize such places. Villages are not good bets as a place to find sex work, and destination trafficking in such places is essentially nonexistent. No entrepreneur in his or her right mind would pay good money for something expensive by Cambodian standards such as startup and continuing costs for a brothel, let alone one with trafficked individuals in it, and then bring it to a place to sell where no one can get there and no one who is there has any money.

If I had to select an area in which our counts of sex workers are most subject to possible error it would be in **over estimating**, not under estimating, the number of sex workers in Cambodian villages. We purposely selected the largest villages for study since they had the best chance of having sex workers as is found in any study of such villages. The exception would be a small village at a well-traveled crossroad, and those were included as part of the sample. We then applied estimates obtained from these often-larger villages to villages of all sizes across Cambodia. Given the sheer number of such villages in Cambodia, most smaller than those we studied, when the number we found in the usually larger villages we studied is multiplied by that large number of villages, it produces a large estimated number of sex workers in villages. While we strove for accuracy, whenever we had to make a research choice we normally attempted to slightly overcount rather than slightly undercount, so that it would be difficult to argue that there were more sex workers or trafficked individuals than we found. Choosing to *underestimate* in such choice situations would not produce useable information since all we would know is that the actual number was larger up to infinity. But slight overestimates produce a reasonable upper bound plus a lower bound of zero and that is what we were seeking.

If one is looking for a place that sells *Pho Ga* in a Cambodian village, one can learn of its existence in well under 15 minutes. You can spend as many hours as you like in the village, but it will not create new *Pho* restaurants. The same is true for brothels. If they are there, you only have to ask, if the right person, a male, asks in the right way: privately with another male. When a *village* had a “brothel” it usually turned out to be a hut where music is played in the evening and where a woman or two considered “loose” might be found. Quick counts do not lead to errors, large or small. And if our numbers for villages – the only place rapid counts were used – have errors, they are overestimates, not under estimates.

Comparison with IJM Numbers

IJM took several weeks to identify 45 underage prostitutes in the brothel area known as Svay Pak outside of Phnom Penh (Swingle & Kapoor, 2004b, pp. 6).

Swingle & Kapoor’s statement above is incorrect. According to IJM personnel, it took about five days to identify the 45 victims, not several weeks. The remaining time was used to identify levels of owners and operators and obtain evidence with video and audio recording equipment prior to the raid. IJM is

the International Justice Mission, an American investigative group funded in part by the U.S. Department of Labor. Swingle and Kapoor did not bother to learn of this distribution of time from IJM.

Svay Pak is North of Phnom Penh and contains a large complex brothel area. We used moto drivers and other male interviewers to get data from Svay Pak on several days in 2002 and again in 2003 due to its complexity. With the exceptions of Phnom Penh proper, Koh Kong, Siem Reap, and Sihanoukville, no city, town, or village in Cambodia contains a sex work area even vaguely approaching the complexity of Svay Pak. Our time frame for measuring the number of workers in Svay Pak is similar to that of IJM. Since Swingle and Kapoor rely on IJM's numbers for their argument, this presumes that they trust IJM's numbers. With respect to the number of underaged workers in Svay Pak in March 2003, we said:

In late February that number had increased to 24, and then to 46 in two separate March measurements. A major police raid led by the International Justice Mission occurred on Saturday, March 29 (Steinfatt, 2003, p. 24).

Now how is it that our number for Svay Pak of 46 in March 2003 is exceptionally close to the number IJM found for Svay Pak of 45 within the same time frame, and yet Swingle and Kapoor believe the IJM numbers, but do not believe ours? IJM's numbers validate our counts just as our counts validate IJM's numbers. How can Swingle and Kapoor argue that IJM's numbers are good but ours are bad? In large towns, or large complex brothel areas such as Svay Pak, a few days are usually all that is needed. The amount of time needed to obtain a count in any geographic area decreases exponentially with the complexity of that area. No towns or villages have complex areas. Our methods obviously yielded valid results in this case since we found the number of underaged workers that IJM found to be there during their raid. Our method is clearly valid at Svay Pak. Why would it not be valid throughout Cambodia? Yet it is exactly this proven measurement validity that Swingle and Kapoor are questioning in their "nonsampling error" point, which they characterize as the major problem with our methods. Since our numbers were gathered independently and are the same as IJM's numbers, application of our moto driver method clearly produced valid numbers in that instance. Each of Swingle and Kapoor's arguments against our moto driver method is invalidated by that fact by itself.

MTD Lists

2. *MTD lists.* Swingle and Kapoor question our method of estimating the number of open MTD venues, wherein the proportion of MTD venues observed open in major venue areas is used as the best estimate of the proportion open across the city. As Swingle and Kapoor state "it's impossible to know the direction let alone the magnitude of this sample bias" (2004b, p. 6). We agree, but it is the best estimate available, an actual sample of the venues gather by the police.

Swingle and Kapoor then reverse themselves and argue against their own observation as stated above, suggesting that the direction is likely on the low side. We prefer their former position to their latter position. The number of MTD venues in 2002 was given by the MTD as 1,052. Swingle and Kapoor argue that since this is a police generated number that it must be biased on the low side "Given what is known about police complicity and corruption" (2004b, p. 6). Yet Swingle and Kapoor do not appear to understand normal police procedure, which is to compile a list of such establishments as they open, but to fail to remove establishments from the list when they close. AFESIP administration also was apparently unfamiliar with this procedure. The police do not walk around and check to see who is open, absent an order by the Prime Minister. So it is likely that many of the 1,052 ceased operation prior to the Prime Minister's closing order, but were only noted as closed when the police had to make an actual count due to the order. Recall that we were attempting to get a population count with this measure, not to sample. We believe that the 45.8% of MTD venues we found open in major sex work areas is the best estimate of open MTD venues, and there is certainly no good reason to assume a higher proportion open elsewhere in the city. While Swingle and

Kapoor argue for slanting the estimate toward the high side, we believe the observed unslanted estimate is the best, and used it. When the obtained n approaches the population N , as it does here, the issue of the representativeness of the obtained data decreases rapidly in importance.

How Brothels Operate: Discreet vs. “Hidden”

3. *Field observation methods.* Swingle and Kapoor question a portion of the theory of sex work I propose and use in my work. My theory (Steinfatt, 2002) holds among other elements that sex work is a commercial enterprise: no customers, no sex work. In response to this, Swingle and Kapoor write:

Steinfatt offers one side of the story but conveniently ignores the other; he assumes all these businesses would want to operate out in the open and men seeking sex would want to frequent them. For various reasons some men prefer and are willing to pay for discrete prostitution. Furthermore, some small operations, especially those dealing in child prostitution or sexual slavery, benefit by staying relatively hidden and not exposing themselves to unpredictable law enforcement officials who may shut down their business or extort money. Obscurity has advantages. That is why it is the *modus operandi* of illicit businesses around the world. So we find it hard to share Steinfatt’s confidence that his research team got a reasonable count of all the locally oriented sex venues operating in Phnom Penh (2004b, p. 7).

Swingle and Kapoor attempt to place assumptions in my work that do not exist, claiming I “conveniently ignore” the fact that some businesses do not want to operate in the open and that some men want discrete sex. I do not make the assumptions they claim, and in fact the opposite is true. The *offer* of discreteness is one essential ingredient of sex work. While some customers may not require it, venues always provide it to some degree, though different forms of the business provide it in different ways. In other words, it is not something provided by only a few businesses as Swingle and Kapoor suggest. It is almost always provided in one form or another in all such venues, such as with tightly monitored admission, and hidden entrances for known customers, as perhaps through a hidden hallway from a grocery store several doors down. The more illegal the business, such as with children, the more the location is likely to be in a solid building with heavy lockable gates and doors. While entry may be observable, what occurs inside is discrete and well protected. That is the concern of customers.

Then Swingle and Kapoor suggest that since not all sex work is open to public view, that that which is not, is unlikely to be found by my methods. That is a *non sequitur* – it simply does not follow that because sex work is discrete that it cannot be found or observed, or will not be found by my methods. Swingle and Kapoor confuse discreteness here with attempts to make a business impossible to locate. The two are quite different concepts. Sex work is always open enough to be commercial, else of economic necessity, it dies. Discreteness does not equate to non-observability, or even approach it, even with brothels engaged in the worst forms of trafficking.

Consider how brothels do operate rather than Swingle and Kapoor’s guesses about it. Brothels and other venues do not change their locations often. To move is difficult and risks losing customers, and the business owner may also own the land on which it operates. When harassed, some brothels will move, but like any other business they prefer to stay put so their customers can find them easily. Businesses that move always lose a number of repeat customers. Suggesting as AFESIP does, that the local police do not know the locations, which are relatively fixed, is simply not in accord with reality. The police may not tell you that they know the locations, but the local police know. Owners may try to keep a low profile so that the police will not discover them, as Swingle and Kapoor suggest, but they will be unsuccessful. With a very small sex work operation, perhaps one to three women working out of a small room with no manager aside from themselves and very little income, the police may ignore it.

In operations that are far more profitable, such as with children, the police will usually be involved in some way – ownership, payoffs – and will almost always know of its existence if it has been operating for more than a few days. Thus, no one stays relatively hidden so law enforcement will not shut them down. What exactly is “relatively hidden” in Swingle and Kapoor’s conception? Just hidden enough so my methods will not find them, but open enough so anyone who wants to patronize them can do so? Can we please have an operational definition of “relatively hidden”? Swingle and Kapoor’s belief that some sex work operations are so hidden that only those who want secrecy can find them makes no sense. If someone who wants discreteness and secrecy can find them then anyone seeking sex can find them with a little effort. Discreteness does not mean hidden. Entry to the brothel may provide ambiguity of destination, and the sexual activity will be hidden, but not the brothel itself.

The *Modus Operandi* of Illegal Activities

Swingle & Kapoor’s *modus operandi* point is misapplied to point-of-sale locations. It is often applicable to *in transit* and higher-level drug trafficking, sex trafficking, etc., where the location of the women or drugs constantly changes. It does not apply to fixed location sex businesses in Cambodia. In Miami, for example, specific locations where illegal drugs can be purchased are common knowledge among those who are interested. It is not difficult for the police to obtain such knowledge. But drug sales are diffused among multiple low-level dealers who are often of far less interest to the Miami police and the DEA than the higher-level drug traffickers. In contrast, my studies are directly interested in the relatively fixed-location local “dealers” of sex, and such persons and locations are always fairly easy to find. Swingle and Kapoor misapply the obscurity concept to point-of-sale situations, to which it often does not apply.

“Hidden” Brothels

Consider what our research team has learned about “hidden” brothels through our observations. Where brothels appear to be “hidden,” and not immediately visible, the brothel will always be engaged in exterior screening of some form. Brothels do not usually hide themselves when they have indentured workers. They do hide themselves to an extent when enforcement efforts are directed against them, as in the case of children. Exterior screening of customers means that the potential customer is screened prior to entrance to the brothel, and the casual person walking down the street may not know the brothel is there or that he is being screened, because the brothel with something to hide, such as children, does not announce their presence. It may have no appearance of an open business, with no lights, no apparent activity, and possibly chains and padlocks on its doors, and possibly with a “permanently closed” bill slapped on its front door after a raid. It can often be found because it is in a known brothel area. Additionally, nearby residents will sometimes know what goes on inside. A legitimate business nearby, perhaps a small drugstore or grocery store, often with a better view of the street than the “hidden” brothel, will operate as a lookout, along with one or more outside runners, and it will have a vision slot near the steel door.

Lookouts recruit customers. With unknown potential new customers, an unsuspecting appearing person in or near the lookout establishment may attempt to speak to the person being screened. In the case of a moto driver there will be little question concerning trust, just a question concerning ability to pay. The moto driver can answer this sincerely by stating his purpose to ask about what is available, a common question. If the customer, moto driver or otherwise, approaches the door directly, and is known to the business, or is known to frequent other sex work businesses nearby, a person outside will appear, apparently from nowhere and often not from the lookout business, with a key to the padlock. Whether he unlocks the door will depend on what is said and will be a decision made from inside, or from a higher-level person with a cell phone, often in a position to observe the interaction. While moto drivers would not be able to afford the price of certain services in some such places and everyone involved knows that, they also know that moto drivers occasionally front for customers or may have other business there, and the person with the key, as well as someone listening inside, will hear him out.

We never had a problem with a moto driver being admitted to common brothels. The smaller the city, the higher the likelihood that any given moto driver knew the location of such places. In towns and villages drivers would always know. But aside from cities there is not a sufficient economic base to create the critical mass of customers needed for illegal activities such as young children. Such activities occur in or near cities. Underaged workers found in towns are in their upper teens. In higher priced venues such as those connected to expensive gambling ships on the Mekong or expensive land venues, our researchers approached them by expensive car, not by moto or on foot.

Thus, Swingle and Kapoor's argument confuses discreteness with ability to locate, and their *modus operandi* point concerning illicit businesses around the world refers to high level and in transit situations, which they misapply to point-of-sale situations.

The Phantom Cities of Cambodia

4. Steinfatt does not count the number of prostitutes in three of the ten largest population centers in Cambodia. . . . As of 2003, Prey Vang was the 5th largest city in Cambodia, Pousat was 8th, and Takeav was 9th. Steinfatt offers no reason for excluding these cities. For someone claiming to count the number of prostitutes in all of Cambodia, we can see no logic for excluding three of the 10 largest population centers in the country (Swingle & Kapoor, 2004b, pp. 7 – 8).

Contrary to Swingle and Kapoor's quoted statement above this is simply not true. Anyone can make a mistake, and a website providing city population statistics made an incorrect assumption about Cambodian population statistics provided by the Ministry of Planning. The problem for Swingle and Kapoor occurs in that we discovered this after we had made the same error a year earlier. We stated this quite clearly and openly, explained it on p. 7 of Steinfatt (2003), **one of the two papers Swingle and Kapoor are critiquing**. Thus, these authors had our explanation of their criticism, together with the citations to verify it, in front of them prior to writing their critique, but they apparently did not read what they were critiquing. Or perhaps they read it but did not understand it. How could that be possible? There *is* no proper city of Prey Veng, Pousat, or Takeav. These are figments of a website's imagination, and also of Swingle and Kapoor's.

We had a similar misunderstanding of the rural Cambodian countryside when we produced Steinfatt, Baker, and Beesey (2002). In 2002 we had only been in Phnom Penh, not the countryside, and we collected data in and around it as we said in our report. In 2003 when we went into the countryside and subsequently spoke with population statisticians at the Ministry of Planning, we saw that what were listed as large cities by many websites but were in fact villages, were simply misinterpretations by the websites of Ministry of Planning figures. These "cities" were in fact just small villages or towns. This occurs because the Ministry of Planning reports the entire population as a centroid, as though that population is located at a single point, a point which is defined by MoP as the largest center of population in a district or even province. The location exists, the village named as a city on the websites exists, but the actual population living at the point is only a small fraction of the number listed on the websites. The importance of cities in sex work, as opposed to smaller places such as towns and villages, is that the extent of sex work at any point is a direct function of, among other factors, the population density around that point.

Here is what we said in Steinfatt (2003), one of two of our papers Swingle and Kapoor were critiquing and had in front of them:

While data collection for the census was done professionally and carefully, the definitions used in reporting the census make the results difficult to apply to the present study. All persons residing within the surveyed section of the traditional geographic boundaries of an area designated as a district, commune, or village were counted in the census as residing in that demographic unit. Thus, census designations such as "village" do not necessarily refer to collections of dwelling units in close proximity and visible to the eye, but often to a

sizeable geographic area with widely scattered dwelling units. The census report definitions make the census difficult to use in locating and in designating sex work venues, since the location of such venues is sensitive to population density, not to the number of dwelling units scattered across a sizeable geographic area.

In addition, the entire land area of whole provinces such as Kep, Pailin, and Sihanoukville is classified as “urban” in the census reports, in keeping with these provinces’ classification as “towns.” “Town” and “city” are not census defined terms, and are not used in the census reports aside from noting, for example, that three entire provinces – Sihanoukville, Kep, and Pailin – are defined as “*Krong*” (towns) (NIS, 2000, p. xv), and in a reference to “provincial headquarter towns” in explaining that the entire districts containing these towns will be treated as “urban” (NIS, 2000, p. xvii); Steinfatt (2003).

While this explanation may not be clear to the general reader, it was not intended for the general reader. It was intended for persons familiar with urban sociology and population statistics. That would seem to include Professor Swingle if not Kapoor. But the “cities” referred to by Swingle and Kapoor *are not there*: they are phantom cities that do not exist. And we indicated this in the very article Swingle and Kapoor say they are critiquing. How can they claim to be carefully critiquing my work and that of my colleagues when obviously they either did not read this section of what they critiqued, or despite their training did not understand it?

A principal problem with Ivory Tower research methods is that if the persons doing the data analysis do not get out in the field and get their hands dirty with the data collection, they will miss very important aspects of the data when they try to interpret it.

We conclude from this that Swingle and Kapoor, as well as Somaly Mam and Pierre Legros who approved release of their paper, are unfamiliar

- (a) with what these areas of rural Cambodia look like;
- (b) with the standard methodology used by the Ministry of Planning of Cambodia to report census data; and
- (c) with the fact that web sites (in this case *World Gazetteer*, 2002) occasionally present completely incorrect information as though it were rock solid. The only other possibility would seem to be that Somaly Mam and Pierre Legros do not read or care about the accuracy of written materials they publish in AFESIP’s name. We strongly suspect that they care, but are quite unfamiliar with the countryside of Cambodia, to the point where they are willing to believe that there are large cities in Cambodia where no such cities or anything remotely approximating them exist. And how can that be when Somaly by her own account has been traveling throughout Cambodia rescuing trafficked women and children for many years?

Good researchers go to the source, in this case the Ministry of Planning which creates the reports on which these “city sizes” are based, and to observation, the rural Cambodia landscape and its villages, where Somaly Mam claims to have rescued large numbers from their trafficked status. Depending on a web site alone can lead to problems, as we were also reminded.

In sum, there *are no cities* of Prey Veng, Pousat, or Takeo, fifth largest or otherwise, and the Cambodian census does not claim that there are. It is that simple. These are merely Phantom Cities. Prey Veng and Pousat are small towns that are not much more than wide spots in the road. Takeo covers a bit more land area but with a similar lack of population density. Ta Mok’s house is its most distinctive feature, and that is almost a kilometer away from the other buildings across a swampy lake. They appear large on paper only because the census counts every living soul in the entire district in which they are located – districts that cover a substantial portion of the totally rural land area of their respective provinces – and labels that population “urban.”

Misleading Report Titles

5. *Destination, versus source and transit counts and “misleading” report titles.*

Swingle and Kapoor state that my report titles are misleading because they do not include the word “destination” and because only trafficking of women and children for sex work is included. We cover this point in the 2002 paper under the heading of Double Counting. Here is what we said:

While there is evidence of trafficking of Khmer children to neighboring countries for employment as beggars and low skilled labor, we suggest that trafficking should be measured within individual countries in a way that accurately reflects the total number of trafficked persons. If Khmer persons trafficked to Thailand, for example, are to be counted in the number representing Cambodia, then how is the count for Thailand to be composed? Are trafficked persons to be counted twice, once within their country of origin and once within the country to which they are trafficked? Since Vietnamese persons trafficked into Cambodia are included in the Cambodian total presented above, we suggest that they should not be counted again as trafficked in the Vietnam totals. To do so would double the estimate of the number of persons trafficked across international borders since all such persons would then be subject to double counting.

For the same reasons, we suggest that any trafficking of persons shown to exist that uses a given country as a transit point between other countries, be counted in the totals only for the country of final destination. To do otherwise, such as counting persons coming from one country and traveling through five countries to reach a final destination country as having been trafficked within each of these countries, would overestimate the number of trafficked persons by 700% since the same person would then be counted seven different times. Separate statistics maintained within each country on the numbers in transit, numbers originating in a country, etc., are of course appropriate and necessary categories provided that they are not used as the basis for double or triple counting.

Given this reasoning, we conclude that our best estimate of the total number of trafficked persons in Cambodia for all purposes is 2,488, since we see no good evidence of significant numbers of persons brought into Cambodia, or trafficked within Cambodia, for non-sexual purposes. To the extent that such persons may be shown to exist in future empirical studies, our estimates should be increased by that amount. Similarly, our best estimate of the upper-bound limit for the total number of trafficked persons in Cambodia for all purposes is 2,743 using the same reasoning. Persons wishing to challenge these estimates must present a clear, complete, replicable, scientific method for the way they obtained the higher estimates (Steinfatt, Baker and Beesey, 2002, pp. 14- 15).

I stand by this statement. I count Cambodians begging in Thailand as part of Thai destination trafficking, and it is clear that such persons exist in fairly large numbers to anyone who observes the ethnicity and language of beggars on the streets of Bangkok. The IOM has data on this. We have no numbers on such persons and do not claim to have such numbers.

As to misleading titles, this is really stretching for an argument. My title means exactly what it says: *Measuring the Number of Trafficked Women and Children in Cambodia*. Not from Cambodia, not passing over or through Cambodia, but in Cambodia. It is quite a reasonable title for this work since that is exactly what we measured and report.

Reliabilities “too good to be true”

B. Undercounting sex workers within establishments

1. Reliability and validity

Steinfatt’s reliability is almost too good to be true. Why? Because based on what Steinfatt reports it could never have been the case that three co-managers reported 37, 40, and 43 prostitutes in their establishment . . . Steinfatt’s managers were doing some extremely consistent counting that virtually defies belief, a consistency all the more remarkable when one considers some of the fairly large, crowded dimly-lit prostitution venues in Phnom Penh attracting ‘free lancers’ not under the direct supervision or employment of management. Steinfatt’s reported reliabilities are fundamentally problematic . . . Statisticians would typically use a different measure of reliability than the one used here by Steinfatt but his conveys the same idea (Swingle & Kapoor, 2004b, p. 9).

Swingle and Kapoor’s response assumes that much of our data was collected in indirect venues such as the several large bars in Phnom Penh which they are describing in the quotation above. Venues with “37, 40, and 43” workers are bars of one type or another in Cambodia. In those establishments one might easily observe 30 or 40 females present, and a majority of these women are often sex workers, but not all. There is no need to ask a manager about the number of persons available as workers in such places since all are visible, ordering drinks, talking with men or each other, and listening to the music. None work for or are paid by the bar except waitresses, and management would have little better idea of the number of available females present than any other observer. The personnel and numbers change frequently in a given evening as in any bar in a Western country. Our concern in this study was primarily with trafficked women and children, and the reliabilities we present are for brothels: direct workers. The great majority of trafficking cases are found in brothels – places with direct sex work where partner selection is made by the customer prior to any communicative interaction with the selected partner. Though it is possible for one or more of the women present in a bar to be trafficked, bars involve indirect sex work: customers of the bar engage in communication interactions with persons in whom they are interested, come to an understanding with them, and leave together.

Swingle and Kapoor are conflating indirect sex work here, as in the bars they describe above, with the direct sex work of brothels. We use moto-drivers for brothels, not for bars. And there is no need to ask managers concerning numbers of women in a bar. Rather, *we* count them, as described in detail in Steinfatt (2002). Bars cater to Western foreigners and someone with my demographics or Swingle’s demographics would be one of the data gatherers for bars.

Does their argument fare better on reliability statistics? Consider Swingle’s example: 37 sex workers in a brothel? 40? 43? This never happens. So it is irrelevant if one could or could not have reports of 37, 40, and 43 brothel workers since brothel numbers we observed were never that high. Numbers that high in a single brothel do not exist in our entire data set. Their confusion here is that the number of workers in the range they cite is more typical of a few very large indirect venues (bars) found mainly in Phnom Penh, as discussed above, and that may be what they are thinking about here. Indirect venues such as these are not brothels. Brothels operate with direct workers. Women who work bars are free to come and go as they please, and most are freelance, not paid by the bar. And a few are often Western women in their 20s and 30s who are talking with freelance workers in the bar.

We wonder if this is truly AFESIP’s conception of what sex work looks like in Cambodia. They claim to be experts on sex trafficking but write as someone would who does not understand how it works. The reliability figures given in our papers (2002; 2003) refer to direct sex venues, which is clear from the context discussing brothels, but is not specifically stated. We apologize for a possible lack of clarity, but had offered to explain anything they wished and the offer was refused. What we specifically stated is that “The

methods used follow closely those described in Steinfatt, Baker, and Beesey (2002), and Steinfatt (2002)” (2003 p. 6). And again: “The research methods detailed in Steinfatt, Baker, and Beesey (2002), and in Steinfatt (2002), were employed to obtain the data” (2003 p. 8). Steinfatt (2002) is a 400+ page book that contains an exceptionally detailed summary of over a dozen of my studies of indirect sex work in Thailand. These were conducted over a 12-year period, and the book contains the basic methods used in studying indirect venues, as referenced.

Brothels outside of Cambodian cities and towns typically have 3 to 5 workers. In towns and cities, 3 to 10 workers are common, more than 10 is unusual. Managers of brothels know how many people work for them. The number does not change often. Everyone knows what it is. If one has only 4 workers one does not say “3” or “5” or “none” or “72,” nor does one’s brother who is the other manager. The usual cross-respondent reliability for multiple questions concerning the number of workers, number of Vietnamese, etc., when that number is in fact small, is 100%. That is not “too good to be true” but a function of the size of the workforce in most brothels. Would the reader call two managers who each report four workers “extremely consistent counting that virtually defies belief?” Small numbers of workers and small numbers of category members produce higher reliabilities (usually 100%) than larger numbers of workers.

In one instance of a very large brothel in Phnom Penh, one manager reported 19 workers, and the other reported 20, producing an agreement percentage of 95% across the managers. In two other cases in Phnom Penh one manager reported 10 while another other said 11, and in one other place one said 9 while the other said 10, producing agreement percentages of 91% and 90%, respectively, as reported in Steinfatt et al. (2002, p. 5). The agreement percentages for smaller places were each 100%. That is, all moto drivers independently reported the same number to us. Reliability of size of workforce-related estimates deteriorates slightly with increasing size of the workforce. For 2003 (p. 10), we summed the eight estimates, each gathered from the largest brothel in town, seven of which were 100% reliable on each piece of information requested, with a total of 55 workers reported across the seven brothels. In the eighth, the largest brothel, one manager twice reported 11 workers while the other said 10. We treated this in terms of the total number of workers reported across two sets of management estimates, which was 65 versus 66, or 98%. The agreement percentages for the other categories such as ‘number indentured,’ in that case 40 versus 41 reported indentured, were calculated in the same way.

Our reported reliabilities are accurate. Labeling them with phrases such as “almost too good to be true,” and “virtually defies belief,” is clearly uncalled for and borders on the unethical: These statements constitute a not very thinly disguised way of calling someone a liar. We believe these erroneous and unsupportable charges by Swingle and Kapoor are not based on problems with our data or methods, but in their rather surprising lack of knowledge of sex work establishments in Cambodia and their overly rigid belief in the ‘there’s only one right way to do it’ school of thought.

Agreement Percentages versus Correlational Methods of Reporting Reliability

With respect to statisticians using “a different measure of reliability,” Swingle and Kapoor may be thinking of reliabilities calculated by formulaic and correlational methods such as KR20, KR21, Coefficient alpha, and similar correlational techniques. Such techniques refer to the internal consistency reliability of sets of items and are generally inapplicable to the form of single repeated measurements that we use. Swingle should know that. The agreement percentages we use are both informative and a standard way of reporting this type of reliability. One does not need to understand the fine points of correlational assumptions of biserial versus point-biserial versus tetrachoric versus a phi coefficient as the item estimates, in order to understand this method of reporting reliability. The reliability numbers we reported were sometimes *lower* than they would have been using a correlational reporting method where possible, as well as being more easily understood by the non-technical reader.

Swingle's issue of the independence of estimates for calculating reliability is valid, but irrelevant. Managers do indeed report the number of workers they have, not some pre-rehearsed false number as Swingle and Kapoor imply. The IJM numbers compared with our numbers provide one demonstration of that, as discussed above. If Swingle and Kapoor have data showing that brothel managers practice giving out consistent but false numbers, let them publish these findings, along with their method.

Even More on Motodop Drivers

We cannot simply assume that members of the secondary research team – local men and taxi drivers – are in a position to estimate the number of prostitutes working there. Such counts strike us as inherently unreliable. Perhaps that is why Steinfatt never reports a reliability for them. (Swingle & Kapoor, 2004b, p. 10).

This claim assumes that counts by moto drivers were our primary data, while our report makes clear that management reports of the numbers fill that function (Steinfatt, 2003, p. 7). The issues of training and qualification of moto drivers were asked and answered above under the headings “Motodop Drivers – Training” and “Motodop Drivers – Qualifications,” the first time AFESIP raised this issue.

While it is certainly interesting to have Swingle and Kapoor share their personal feelings about how things strike them, why should we care if something strikes them as anything, including inherently unreliable? How would they know what was unreliable? On what basis do they come to this opinion? They hypothesize unrealistically large numbers of workers in brothels such as 37 to 43 since they apparently have not been in enough brothels to know what the actual numbers would look like. Do they have any form of data whatever, any form of coherent reasoning, any statistical arguments, or any basis aside from their own apparent unfamiliarity of brothels and misunderstandings of competent research to back up their personal feelings and beliefs?

With respect to “never report[ing] a reliability” for moto drivers, it seems surprising that Swingle and Kapoor do not recognize that their statement is incorrect. When using observers or reporters in social research in the usual case, the reliability of the reporter is inherently confounded with the reliability of the respondent. If two interviewers report data from observations or interviews with the same respondent made at different times, or with different respondents, whether in a laboratory or field setting, absent independent data on both, the researcher cannot know whether any differences in the reports stem from observer-unreliability or respondent-unreliability – both must be suspected. Our reported reliabilities are for moto drivers reports of management estimates. If two or more drivers report the same number for the same brothel, which they often do, then the moto drivers are presumed reliable and the managers are presumed reliable. Both reporter and respondent could of course be unreliable to exactly the same degree in opposite directions at the same time, but the probabilities of occurrence of such events, especially for multiple drivers and multiple brothels, usually rule them out as alternative explanations for 100% reliabilities.

It seems that much of Steinfatt's primary data is made up of random estimates based on fleeting observations by unqualified and untrained individuals with unknown motivations. Such gathering of data is the equivalent of recording rumors and hearsay and presenting it as reliable accounts of the truth (Swingle & Kapoor, 2004b, p. 10).

Swingle & Kapoor's statements regarding “random estimates” and “rumors and hearsay” are simply unsupported assertions, unsullied by reasoning, evidence, or apparent understanding of this form of field research. Their two sentences, above, are not the beginning of an argument that they intend to construct. Rather, their statements are what they represent as a summary and conclusion of what they hold to be true based on their erroneous and uninformed interpretations of the reliabilities presented, and on our use of the moto driver method, which has been recognized by an international symposium of scholars as excellent

method. Their charges of “rumors and hearsay” presented as “reliable accounts of the truth” are incompetent. They do not tell us:

- (1) Why what they are discussing is of any relevance to the discussion of trafficking numbers, nor
- (2) Provide any reasoning that what they say is even accurate or to be believed if it were relevant,
- (3) Let alone present any evidence of their own for the point in question.

2. *Methodology guarantees an undercount of sex workers in some venues*

Indirect Workers

Swingle and Kapoor give brief consideration to indirect venues, while I spend many pages detailing our methods used in indirect venues, as described in detail in my 2002 book *Working at the Bar*. It is specifically concerned with research on indirect venues. I reference this research not once but twice in the 2003 paper in discussing the methods used to obtain the data, as I mention above. Swingle and Kapoor’s concerns each stem from:

- a failure to read our papers thoroughly,
- a failure to distinguish between indirect sex work in bars and direct sex work in brothels
- a failure to read the methods portions of my 2002 book as referenced,
- a failure to request information about points where they needed more information,
- a failure to accept such information when offered, and
- an unusual apparent fixation in their comments on their conception of the ‘one right way’ to conduct research, a claimed knowledge set of which they apparently believe themselves to be the keepers.

Once again, I point out that our 2003 report says:

The methods used follow closely those described in Steinfatt, Baker, and Beesey (2002), and Steinfatt (2002) (2003 p. 6).

And:

The research methods detailed in Steinfatt, Baker, and Beesey (2002), and in Steinfatt (2002), were employed to obtain the data (2003 p. 8).

Swingle and Kapoor could not have carefully read the research methods in the reports they were criticizing, as discussed above, and make the statements that they make. Instead they chose to assume poor methodology on our part (Swingle and Kapoor, 2004b, pp. 10 – 12), and then to create fallacious straw man arguments, poorly constructed arguments at that, so their created arguments could easily be knocked down by them based on their false assumptions.

I spent 12 years gathering data on indirect venues in Thailand before I began to collect data on trafficking in Cambodia in 2002. I considered far more of the nuances and complexities of data gathering in such venues, and published many my considerations of them in my book, than do Swingle and Kapoor in their brief comments. It is quite clear in Steinfatt (2002) that in indirect venues we do not use moto drivers, nor send them into indirect venues to ask the manager how many available females are present. That would be absurd. We use direct observation as described in the methodological reference given (Steinfatt, 2002) and a complex system of counts as detailed on pp. 132 –143 of Steinfatt (2002) in the chapter on employment statistics and pp. 363 – 378 of the appendix on data collection methodology.

On Reading the References and Asking for Clarification

It is neither possible nor desirable to repeat multiple pages of complex methodology on indirect venues alone in brief research reports such as Steinfatt, et al. (2002) and Steinfatt (2003). That is why authors often refer to the methods sections of other reports. Massive methodology sections are a turnoff to the general reader and require large amounts of space. Any readers of this critique who are still with us at this point must have noticed that the discussion of each detail of the research method leads to a rather lengthy and boring report. My reports are intended for the same audience of persons concerned with trafficking in persons as the previous reports I cite, and they contain sufficient details of the methods, and references to the remaining methods, so that they are replicable, unlike most reports on trafficking in Cambodia published prior to 2002, with the exception of the National Assembly study.

Methods that seem clear to a writer may seem less clear to others. When other researchers who read the methods and references and then want to replicate the research cannot find sufficient information in the referenced material to conduct the replication, or to understand more fully what was done as sometimes happens, they usually contact the authors and ask for clarification. Both Madden and Wiebe did exactly that when they had questions about my work during meetings of the Asia Foundation seminar. We sat down and discussed areas of my reports that were not clear to them and I explained what was done in greater detail. Swingle and Kapoor made no such effort. All they needed to do was ask, even if they were not going to bother to read what I referenced in the papers they were critiquing, which they apparently did not do.

E-mails and Offers of Clarification

And it was not just a failure to *ask* for clarification on their part. In November and December of 2003 I received e-mails from Swingle requesting data. Data gathered or studied by university researchers in the U.S. are subject to a number of privacy restrictions, and major universities have had their entire Federal funding temporarily suspended for failure to follow relevant Federal guidelines on such issues. Swingle must know this. He asked for a “list of places of prostitution” among other data items, none of them methodological in nature, though he stated that his concern in requesting data was with my methods. Yet one cannot learn about methods from examining data alone. With respect to data, I will not allow my research to create, or be used to create, a map or list of sex work venues, and there are privacy concerns in the data as well. While anyone who wants to find these places can do so, I will not be party to a process that might assist that search. I will assist research organizations and researchers with mapping information who can demonstrate a need-to-know, and can assure confidentiality and non-disclosure.

I responded to Swingle that I thought it was AFESIP’s place to release a summary of their data rather than requesting mine, since they constantly claim to have data but never publish summaries of it or allow anyone outside AFESIP access to it. I also stated my policy on releasing data, and asked for reciprocity, in that if I provided them with data that I could share, then they would agree to provide to others the data that AFESIP could legally share.

I then received e-mails from Kapoor, Swingle again, and Pierre Legros, AFESIP’s Regional Coordinator in Cambodia, each asking for something different. The release of any summary of AFESIP’s data as reciprocity was declined. I had asked if I could disseminate their e-mails to other persons. Kapoor replied:

As for our emails, there is nothing private about them and you have our permission to send them to whomever (Kapoor e-mail to Steinfatt, Swingle, and Legros, 22 Dec 2003 17:03:25)

Swingle asked me to “reconsider our request,” presumably for the venue location list and related data he had requested earlier but that was not specified. Kapoor demanded that I release my entire original data set, arguing that my publication of data summaries in my research papers implied that the entire original

data set was thereby open to public scrutiny. I advise her to check the U.S. laws and U.S. Federal policy on research at American universities on this point. Legros asked for “details of your methodology,” but did not specify anything about what methods of what studies he was requesting, or what details he wanted more information about. His message to me reads in part:

Today, AFESIP is asking for details of your methodology. Why ? We do not agree with your conclusions and think that number (sic) are really underestimated. The political impact can be disastrous for Cambodia and can give an erroneous scientific approach (Legros e-mail to Steinfatt, 22 Dec 2003 12:03:35).

I believe Legros’ statement above speaks to the relationship between AFESIP’s desired results and their desire to criticize the studies that produce the results. Their desire to criticize my methods stems directly from the **results** of my research, not from the **methods**. And not just any results: the results that the number of women and children sexual trafficking victims in Cambodia was likely less than 3,000, which was not as high as AFESIP thought the numbers should be. Yet previous guesses such as 80,000 to 100,000 trafficking victims, represented as truth, did not bother AFESIP enough to write anything about them at all.

Despite their continued adamant refusal to provide any summary analysis of their data to anyone, I responded that I would send what data I could if AFESIP would guarantee that that was the only data they needed, and if I could legitimately send that data to them. I wanted a final list so that this did not become a constant fishing expedition by AFESIP, provided I could in fact release what they were requesting. In my e-mails I specifically offered to provide *any* details of my methods that they needed, reminding them that my concerns were for data privacy, not methods. My message sent to the three of them several days later reads, in part:

Perhaps the best way to go from here is to determine what it is that the three of you actually are requesting. If you re-read each other’s messages you will see different things being requested. Pierre wants methodology. Joe and Aarti want data, but the form of data requested by each is different.

With respect to methodology, I have no problem whatever in telling you anything you want to know about the methods used in the two papers I sent to you. I believe that the methods sections of those two papers are reasonably detailed, but please tell me what you are not sure about. I will try to clarify it to the best of my ability. That should answer Pierre’s question.

May I suggest that the three of you talk together and determine specifically what it is that you want from me. If it is as you suggest Pierre, that you want more information on details of my methods, then I will be happy to oblige. If on the other hand as Joe and Aarti seem to suggest, you want my data, then that is more problematic, both because there are a number of restrictions on what I can provide, and because you should be publishing summaries of your own findings as the standard remedy for questions about someone else’s findings (Steinfatt e-mail to Kapoor, Swingle, and Legros, December 28, 2003 7:07 PM; underlining added).

No Time to Read E-Mails on Offers of Methodology

Swingle responded by sending me the American Sociological Association Code of Ethics statement on data sharing. He requested only data with no specifics as to what type or what variables. He was not interested in my offer of methods, and never requested any information about my methods though he was writing a critique on my methods. He concluded by saying “I’ve wasted too much time writing long e-mails and skimming long e-mails about this” (Swingle e-mail to Steinfatt, Kapoor, and other e-mail address, December 28, 2003 14:13:56). He was referring to the second of only two e-mails I had sent at that point.

Kapoor responded that she agreed with Swingle and also did not “have time to waste on long emails” (Kapoor e-mail to Steinfatt, Swingle, and Legros, 29 Dec 2003 09:25:34). I sent an additional e-mail to Swingle a few days later reminding him of my previous e-mail offer, to which he again did not respond. Neither

Swingle nor anyone from AFESIP ever responded to my offer of methodological details. While Swingle and Kapoor did not have time to read my e-mails, they did have the time to send e-mail to me, and in fact initiated the e-mail contact. Had they read my e-mails and asked about any details of my methods as offered, they could have saved the time it took them to invent their own version of my methods as illustrated above, and then to construct arguments against their own creations, and they might not have had to read this much longer paper.

Publication Outlets

Swingle and Kapoor go on to suggest that my research should be published in a peer reviewed journal (2004b, pp. 15 - 16). That suggestion fails to consider the issues of audience and timeliness. The few academics who read such journals are not my intended audience for these reports. The Steinfatt Reports are responding to the many reports cited in Steinfatt (2003, pp. 2 -5) none of which appears in a journal. I issued my reports to and for that audience through dissemination of my 2003 paper through USAID at the Embassy of the United States of America in Phnom Penh, which sponsored it. In addition, MSN.com has placed my papers on its Slate Magazine site where they are available to the public. The time lag to hardcopy publication in social science journals guarantees that work submitted to such outlets will not be published in a timely fashion. My work is intended to inform NGOs, donors, and policy makers now, not Western academics two years from now. Future publication of portions of my work will occur in academic forums as well.

More on Indirect Workers

Swingle and Kapoor did not read the methodological references as noted above, and instead created hypothetical numbers that purport to show a large undercount in indirect venues (2004b, pp. 11 – 12, Figure 1). Had they read those references, they would have noticed that I discuss the counting of indirect workers in great detail under Employment Statistics (Steinfatt, 2002, pp. 134 – 144). In specific recognition of their point prior to their making it, I had said:

We determined empirically that the number of barworkers in any given bar on any given evening was about 75% of the total number of barworkers actually employed by that bar at that time. Thus, enumerating the available workers actually on the floor or otherwise in the bar on several evenings at different times, and dividing by 0.75, gives a reasonable approximation of the total number of available workers (Steinfatt, 2002, p. 140)

This result occurred in indirect foreign oriented Thai bars where the workers were employees of the bar, but were working indirectly and were free to leave at any time. Our initial thought was to divide by the 0.75 ratio to estimate the number of likely workers in indirect Cambodian venues, but actual observation of those venues suggested that this would produce a considerable **overcount**. We observed significant overlap among the indirect sex workers available on any given evening during 2002 data collection at various venues to the extent that counting numbers in many indirect venues for an hour or more and then moving to another indirect venue often produced evidence of the same workers moving through a pattern of locations. This movement pattern is unlike the Thai indirect bar pattern, and is similar to our findings on freelance workers in Thailand, who often follow a location-to-location pattern.

The number of late arrivals and the number of indirect workers leaving with customers was far smaller than that suggested by Swingle and Kapoor's hypothetical illustration in their Figure 1, and correcting upward from the observed number using the 0.75 division would have produced a distinct overcount. On that basis we did not apply the 0.75 correction to the observed number of indirect workers but reported the actual observed number at the venues, as these numbers themselves were producing an overcount since they were not specific point-in-time estimates but data collected and summed over a period

of an hour or more at each venue on a given day, in the observation order such that the number of indirect workers was reaching its peak at these venues. Since trafficking rarely occurs in indirect venues, the number of indirect workers is essentially unrelated to the number of trafficked individuals and affects only the number of sex workers.

In sum, Steinfatt et al. (2002) and Steinfatt (2003) accounted for both the factors of late arrival and of early leave times suggested in the discussion of Figure 1 of Swingle and Kapoor. Rather than reading what we wrote, Swingle and Kapoor created hypothetical numbers here that are irrelevant to our research.

Motodop Driver Interactions with Managers

As argued extensively above under *Motodop Drivers – Training*, better data are obtained when data gatherers do not need to “act.” Swingle and Kapoor (2004b, p. 12) are correct that if the moto driver were “instructed to pose as the representative of a prospective customer” as they suggest he was, that this might present a problem. We did not so instruct them, but there is a misprint in Steinfatt, et al. (2002) that could lead a reasonable reader to believe we did, and we are indebted to Swingle and Kapoor for bringing it to our attention. My original version reads ‘He presented this number to the manager and stated . . .’ as a description of what happened during the data collection. This was changed at some point in the editing process, not by Baker or Beesey but by me, to “He was then to present this number to the manager and state . . .” (Steinfatt, et al., 2002, p. 5). The original correct version above is a description of what moto drivers did according to their reports to us. The quotation from our paper above reads as though we told drivers to do this, which we did not do. Thus, this misinterpretation of our method is my fault and not that of Swingle and Kapoor, or of Baker or Beesey. Responding to our offers of clarification could have avoided this misinterpretation however.

Swingle and Kapoor (2004b, p. 12) have great fun lampooning the moto driver/manager interaction. I found their version quite amusing to read myself. We are left with the feeling, ‘What dunce would ever use moto drivers to collect information in this way?’ In the normal method of our actual research, rather than in Swingle and Kapoor’s representation of it, the specific moto driver actually gathering data is directed as to which venue is to be entered and when. His job is to bring us to the brothel or area, which we have already seen with other drivers or other locals. He knows only that we are interested in brothels and sex workers, something he considers quite natural for men. He has seen that a hundred times before. He has *not* been instructed to pose as the representative of anything. Moto drivers do not ask questions of their customers concerning their motives, only about what it is they want him to do. We ask him to repeat the information we want, and we pay him, dismiss him, and find another driver if he cannot repeat it easily.

When he exits the business he gets on his moto and comes back to our location some distance away, a place where we can move into position to see that he actually enters that specific brothel without being observed. We are in our out-of-sight location, and this is often where the driver records the data on paper for the first time, in Khmer. We do not give notebooks or any writing or recording instruments to drivers. Some drivers borrowed pen or pencil while in the brothel, and wrote the information on their hand or arm. A very few wrote it on a piece of paper they had or borrowed from the brothel. Those who felt confident to give it to us as recorded in memory had the same reliability – usually 100% – as those who recorded it on forearm or paper. We had the only notebook and recorded the information provided in it. That could have and should have been stated more clearly in our 2002 Part-I report, but Swingle and Kapoor were offered the opportunity (see email exchanges above) to ask about the specifics of the procedure about which they were unsure. They did not respond to our offer.

If the next brothel was within sight of the first, we used a different driver at a later time. If not we sometimes used the same driver. That occurred mainly in small villages where zero was the usual number of sex work outlets, more than one was highly unusual, and there was often only one moto available.

The manager has no reason to be suspicious since the driver gives him no reason to be suspicious. Had we “deputized” the driver, given him a union card, told him he was now researcher-in-chief and that he should give his all for the cause of research and trained him thoroughly, there is no telling what we would have gotten, but it likely would not be valid data. But we did not do that. In the very rare cases where a village had sex work but no moto driver, a local man obtained the data using the same method as a moto driver. Would managers be suspicious? Why should they be? In many brothels, moto drivers are constantly hanging around inside. The few local men we used had a similar relationship to the brothel. Moto drivers make a living like any cab driver, both by knowing locations and providing information about things available, then taking customers there. It is standard practice for brothels to pay drivers to bring customers to them. Drivers who want information for prospective customers are quite a natural occurrence in the brothel, provided that the driver believes that he is gathering information about the brothel for a potential customer.

Swingle and Kapoor say that we instructed the driver to “tell the manager that ‘some customers were particularly interested in being with such workers’.” While we did not instruct the moto drivers how to ask questions and obtain information, our misstatement, mentioned in the first paragraph of this section above, could lead readers to that conclusion, and we take responsibility for that misstatement and apologize again for it. We told the driver what we wanted to know, and drivers constructed their own way of asking the question. We state in that quotation the typical response of moto drivers’ descriptions of how they obtained the information we requested, not what we told drivers to say.

If Swingle and Kapoor want to continue arguing their reliability/validity position they need to present evidence that when managers are asked about indentured women absent the other manager, they respond by providing an answer that, through collusion, they have agreed to give, such as the current temperature perhaps, or whatever it is that Swingle and Kapoor are contending that managers do. They must also explain how all of these pairs of managers each agree to state a specific false number ahead of time just in case someone should ask about the number of indentured persons. It must be rather hard for them both to remember such an agreed on number, as opposed to just giving the actual number.

Swingle and Kapoor also want to know if “legitimate” customers might ask this question about indentured workers and how managers might interpret it. They question why a moto driver might say this. We did not ask the moto drivers why they chose this method. Many of them apparently thought it was a natural question to ask. Swingle and Kapoor could ask the drivers and managers if they actually want to know.

More on Moto Driver Interactions with Managers

Then Swingle and Kapoor make a statement that seems to make sense to them but makes no sense at all to me.

We believe management estimates would be substantially less than the true figures, especially in the case of enslaved women and child prostitutes . . . brothels operate on the margins of Cambodian law. They are subject to the whims of law enforcement and can be raided at any time. . . . So brothel managers have an incentive to be cautious when answering questions . . . especially when a stranger is asking questions about . . . forced sexual slavery and child prostitution. It seems reasonable to think that they might underreport the number of prostitutes in their brothel if they fear this stranger might be someone other than a prospective customer or the representative of one. To avoid exposure in the newspaper, an NGO coordinated raid, or police prosecution, these brothel managers might think it a good idea to convince this suspicious man that they are small time operators and that there are worst (sic) offenders in the area (Swingle & Kapoor, 2004b, p. 13).

Swingle and Kapoor make several independent errors here.

- (1) *Assuming whims of law enforcement.* Brothels needing protection, those with child workers and on occasion those with indentured workers, usually have a connection with the local police. Sometimes the local police run them. If not, they almost always pay protection money to the local police. Brothels are not subject to any such whims of when to raid by local law enforcement. They will normally be raided only when a police force not based in their locality initiates a raid. This is quite rare, and usually occurs only when an NGO or other outside force pressures a national, provincial, or district police force to conduct a raid. Unfortunately, in these rare cases the local police or some other source often tips off the brothel about the raid.
- (2) *Strangers asking questions.* The moto drivers are not strangers in brothels. That is one reason why we use them.
- (3) *Manager suspicion leads to under reporting.* Do Swingle and Kapoor really intend to argue here that a person, a manager, who is harboring and selling the labor of enslaved women and children, will freely admit to having a small number of such individuals but not to having a larger number? That argument is specifically what they present here. What conceivable rationale would there be for that on the part of the manager? ‘Perhaps I will only get 20 years instead of 30?’ If the manager suspects the customer, he is not going to admit to or provide evidence of illegal activities. If he trusts the customer he is going to produce a number he can substantiate to any customer who wants that many girls for whatever purpose. It may be higher than, for example, the number of children he has at his place, since managers often have connections with local area girls who come in when called. But it would not be systematically lower. The principal threat to the number being lower would come if we entirely missed a business that had children or indentured workers, not from managers under-reporting their numbers. We made every attempt not to miss such businesses. To believe Swingle and Kapoor’s position, one would have to posit and construct a complex ‘trust variable’ within the manager such that he trusts on a sliding scale and reports an extent of illegal activity on a scale correlated with that hypothetical complex trust scale. Complex explanations of events are always suspect in science when simple interpretations explain as well or better.
- (4) *To avoid exposure in . . . an NGO coordinated raid . . . brothel managers might think it a good idea to convince this suspicious man that they are small time operators and that there are worst (sic) offenders in the area* (Swingle & Kapoor, 2004b, p. 13). Now that is quite amazing information. Is AFESIP telling us that they too ignore small operations with enslaved women and children and only go after the larger operations? Do they have specific evidence that this is the policy of other NGOs? On the one hand Kapoor is on record as stating “Even five victims, or 100 victims, or one victim, is too many” (Cochrane, 21 May 2004, p. 10). That certainly is my position. But now, is she telling us that NGOs will not propose a raid unless there are many enslaved women and children in a brothel? Do they mean to say that a few such victims *is* an acceptable number they can live with? I find that an amazing statement. Or can they possibly be saying that they understand the inner workings of the minds of brothel managers to the extent that they know brothel managers believe that NGOs will leave small operations alone? If brothel managers do in fact believe that, is AFESIP arguing that NGOs promote such policies by their behavior?

When *Many* equals “One”

Swingle and Kapoor (p. 14) claim that Steinfatt’s previous “studies” are just one study, his own.

That is an interesting interpretation. Using that logic, perhaps we should begin telling our graduate students never to codify their research and to keep making it available only in drips and drabs, here and there, so it will be hard to find in unified form. Codifying many studies into a coherent major book, by their logic, will be considered “just one study.” While Swingle and Kapoor appear never to have published anything whatever on human trafficking, they are quite willing to misstate the number of studies I have conducted, condensing dozens of studies each with sizable N’s into “just one study.” It is quite normal in science for an expert in an area to cite his or her own research and methods. It seems unbelievable that Swingle in particular as a college professor would be unaware that scholars who publish the results of their most recent studies always provide references to and often cite the results of their prior work in the same area.

Notebooks, Areas, Sexual Activity, and Questions about Brothels

We are skeptical, however, that many potential customers or their representatives show up with notebook in hand, visit every brothel area, engage in no sexual activity, and ask the questions . . . asked (Swingle and Kapoor, 2004b, p. 14).

- (1) Our interviewers did not show up with notebook – or paper – in hand: Read our references.
- (2) Whether every brothel area is visited is irrelevant to the validity in each *specific* brothel visited. Since such visitation is not information that is available to the individual manager at the time of response, it is not a relevant variable.
- (3) Moto drivers are known to ‘hang out’ in their favorite brothels. Sex is an occasional, not an every time activity. During slow afternoon hours, chatting and bantering with attractive young women in the front room is a common way to pass time for some drivers and other young men.
- (4) People in brothels talk about and ask questions about things related to brothels. It is natural activity.

(Swingle and Kapoor, 2004b, list no point “V.”)

VI. Summary and Conclusion

Swingle and Kapoor’s Summary and Conclusion contains two points that require comment. First, they revert to the straw man form of argument:

Steinfatt’s response to our objections might take the form of “Well, my less than perfect data are better than no data at all” (Swingle and Kapoor, 2004b, p. 14 - 15).

I do not appreciate the placing of full quotation marks around something I “might” say, as though I had said it. Our data are gathered in the most valid way possible in field circumstances. While no data can be perfect, mine are well above the standards held for most data gathering. Social science data often comes as responses to “paper-and-pencil” measures where the respondent guesses what he or she would do on a seven-point scale if he or she happened to be in a situation that is described. Such paper and pencil responses to ‘*what would you do if you were in this kind of situation?*’ questions, provide less valid and hence less valuable information than direct observation of the activity actually exhibited in the real situation, especially in field research settings in less developed countries. No data set is ever perfect, so Swingle and

Kapoor would have to agree that if they ever took the time to produce an empirical data set on human trafficking that it would also clearly be ‘less than perfect but better than no data at all.’ Contrary to Swingle and Kapoor arguing from their position of claiming to know a great deal about human trafficking without having any analyzed data of their own to back it up, we have excellent data that clearly support our position on trafficking numbers. We also have the judgment of a high level commission created by the Asia Foundation that supports our work as providing strong data.

Second, Swingle and Kapoor end their report by repeating their unsupported and really quite vicious charge that our data are based on “guesswork” through methods they label “ethically objectionable” (Swingle & Kapoor, 2004b, p. 15). It is certainly not *our* ethics that are in question here given this intemperate statement by them. We have neither a preconceived bias as to what the numbers should be, nor any vested interest in a predetermined result. Our goal is accuracy. We gain nothing and lose nothing whether the numbers are high or low. Swingle, Kapoor, and AFESIP cannot say the same.

Detailed Critique: **KAPOOR (2004)**

Part II: Definitional Problems

by Aarti Kapoor of AFESIP

(The order below follows that of Kapoor.)

On *True* Definitions

I. Introduction

Kapoor (2004, p. 2) states that the definitions used in the two Steinfatt studies are not the “true” definitions of trafficking. She believes that I have not used the “right” definition of trafficking (p. 1), and that we need a study of the “actual” issue of trafficking. Similar language use is interspersed throughout her response.

One problem with Kapoor’s approach is that it assumes the existence of some unitary activity called “trafficking” for which there is a “true” definition that we must ‘discover.’ Kapoor’s language further indicates that she alone is in possession of this one true definition, and that she will explain it to the rest of us. An alternative approach to definition, the one typically used in science, is that definitions are neither right nor wrong and cannot be true or false or discovered. Scientists create definitions, they do not discover them. *Discovery of true definitions* assumes a degree of set-structure to the social universe that most scientists, myself included, are unwilling to accept. There are many ways to structure our conception of natural social events, and science seeks the most **useful** ones, not ones that are “right” or “true” or “actual” according to someone’s preconception. Definitions are not knowledge claims about the world, as Kapoor assumes, but optional ways of cutting up the real world so it can be investigated. Thus definitions are either useful or not useful to some specifiable degree, rather than right or wrong, true or false. While an atom may be defined as the smallest building block of matter, when protons, neutrons, neutrinos, etc., are considered, the definition of an atom may be changed since it is no longer useful, or retained if it is useful for some purposes even though there are obviously smaller particles. Kapoor does not inform us how she came to learn “the truth,” or why her definitional truth is better than that provided by others.

II. Definition of “Trafficking”

(a) The Victims of Trafficking and Violence Protection Act 2000

Kapoor questions our definitions of trafficking:

The definition of trafficking for sexual exploitation is wrong. The second quote actually refers to a “severe form of trafficking” (Kapoor, 2004, p. 2).

Kapoor is correct that this definition comes from the ‘severe form’ definition. It also is a definition used for trafficking for sexual exploitation, as I stated.

She states correctly the U.S. definition for sex trafficking as “the recruitment, harboring, transportation, provision, or obtaining of a person for the purpose of a commercial sex act.” She then applies this definition to suggest that:

“the true and wider US definition of trafficking would encompass more than just those prostitutes who were found to have been indentured” (Kapoor, 2004, p. 2).

Our measure of indentured status included whether the workers were or ever had been indentured, or unable to leave the establishment freely and not return (Steinfatt, et al., 2002, p. 5; Steinfatt, 2003, p. 9), and Kapoor does not give a reason why that method would not find those who were trafficked. Yet Kapoor’s statement is accurate to an extent, largely because there are serious usage problems with the definition of SEC. 103 (9) of Public Law 106-386, the *Victims of Trafficking and Violence Protection Act of 2000*, as applied. This is the section Kapoor is referring to above which lists the recruitment, harboring, transportation, provision, or obtaining of a person for the purpose of a commercial sex act. People and groups who oppose trafficking usually oppose it because it involves force fraud or coercion and thus removal of choice from the trafficked individual, which can subject these individuals to severe degradation. Kapoor’s use of the definition would include all women working as sex workers in both direct and indirect venues of their own free will as trafficked individuals. This inflated number could and would then be used to illustrate the extent of “trafficking” (meaning free will sex work), when persons hearing the term in use, usually assume that “trafficking” refers to force fraud, or coercion.

This usage problem allows for **equivocation** on the definition of a trafficked person, from force, fraud, or coercion, which essentially everyone opposes (trafficking in the first and usual sense), to people who freely choose to have sex with others with an exchange of something of value (“trafficking” in the second sense). That equivocal usage has been, can and will then be used to mislead others concerned with trafficking in the first sense, but who are not into the nuances of legal definitions. In addition, if all women working of their free will in direct and indirect venues are defined as trafficked, then this will dilute the effort of groups opposed to trafficking in persons, away from those who need and want help and toward those who neither need it nor want it. The definition proposed by Kapoor thus would further threaten the safety of trafficked persons.

When the US Congress, in its collective wisdom, passes a bill that is signed by the President, it becomes law in the United States. It does not become chiseled in stone. It is similar to the relationship between an operational and a conceptual definition. In science, if the conceptual definition is held to be in identity with the operational definition, then criticism of the concepts involved becomes impossible. The concept then *is* the operation. Operationally, intelligence may be defined as an individual’s score on an IQ test. If the epistemic correlation between the concept and its operationalization is held to be 1.00 (if the operationalization *is* the concept) then no theoretic critique of the concept of *intelligence* can be given

suggesting that IQ may not be a good operationalization of *intelligence*, since the IQ score is *intelligence*. Such a state of affairs cannot be allowed in an atmosphere of academic debate and intellectual freedom.

All collective political decisions involve a degree of compromise, and the definitions provided by such collective action are more designed to create something that can be agreed upon and passed, rather than something that is well thought through in terms of what will happen when the definitions in an act are applied to the real world. When they don't work, we talk about them and change them.

Kapoor's argument comes from the Trafficking Act of 2000 which defines sex trafficking:

(9) Sex trafficking.--The term "sex trafficking" means the recruitment, harboring, transportation, provision, or obtaining of a person for the purpose of a commercial sex act (U.S. Public Law 106-386 SEC. 103 (9)).

Effect of Section 103 (3) of Public Law 106-386 on Definition of Trafficking

If Kapoor is going to argue on the basis of the U.S. trafficking law, which she does, she cannot then pick and choose sections of the law to support and other sections to ignore. Section 103 (3) is also relevant to her discussion since it defines a principal term in Section 103 (9): the definition of a commercial sex act. That term is defined in 103 (3):

(3) Commercial sex act.--The term "commercial sex act" means any sex act on account of which anything of value is given to or received by any person (U.S. Public Law 106-386 SEC. 103 (3)).

With all due respect to Congress, that is not a workable definition either with respect to trafficking-in-persons or in general. It defines essentially all sexual acts with another person as commercial sexual acts. Surely that cannot have been the intent of Congress. All dating in which one person of any gender pays for food or drink or gives a present to a girlfriend or boyfriend and sex occurs even partially as a result, then becomes commercial sex. It is difficult to conceive of a sexual act between two persons that does not occur in some sense on account of or in accord with giving or receiving something of value. Perhaps some unselfish acts of passionate romantic love might be allowable under SEC. 103 (3), but nothing else. Now clearly that is not what Congress could have intended, but that is in fact what 103 (3) says. Congress will, I am sure, reconsider the unintended implications of such a broad definition soon. I consider some of these definitional and policy issues regarding commercial sex acts in detail in *Working at the Bar* (Steinfatt, 2002, pp. 3–4, 14–16, 341–362).

So if Kapoor wishes to base an argument against definitions upon the U.S. trafficking laws, she is saddled with the entire law, not just the portion of it she wants to defend. Specifically, she cannot simply exclude the sections of the law that define the important terms of the section she quotes. As she must know, that is why most people involved in trafficking research use the "severe forms definition:" It is clear enough about what is being proscribed that it provides a practical guide to action. For while 103 (8) also refers to a commercial sex act, its interpretation and application are not dependent on the meaning of the overly broad definition of 103 (3).

Overly Broad Definition of 103 (3) vs. the Practical Guide to Action of 103 (8)

There is no definition of trafficking *per se* in the trafficking act. But severe forms are defined in 103 (8):

- (8) Severe forms of trafficking in persons.--The term "severe forms of trafficking in persons" means--
- (A) sex trafficking in which a commercial sex act is induced by force, fraud, or coercion, or in which the person induced to perform such act has not attained 18 years of age; or
 - (B) the recruitment, harboring, transportation, provision, or obtaining of a person for labor or services, through the use of force, fraud, or coercion for the purpose of subjection to involuntary servitude, peonage, debt bondage, or slavery (U.S. Public Law 106-386 SEC. 103 (8)).

In other words, in order for Kapoor to attempt to use the U.S. trafficking act (U.S. Public Law 106-386) as she does to insist *on the basis of the definitions of that act* that voluntary sex workers be included in the definition of trafficked women, she must also include all persons beyond voluntary sex workers who engage in a commercial sex act, or she is violating the definitions and provisions of the act itself. She cannot both rely upon the definitions of the act to support her point, and then argue against the application of other definitions in the act. In fact, that is what she is inaccurately accusing me of doing. But *I* make no legal argument based on the act. *I* do not rely on the act, in the legal sense. She does. I simply state some of its definitions for conceptual purposes and then operationalize trafficking in women and children in a way that can be measured and is in accord with the commonly accepted notion of trafficking. Madden (2004, p. 1) supports this method and reasoning process in social research.

Kapoor thus cannot include only voluntary sex workers as trafficked women, but must argue for *all* women and men who receive anything of value – anything – on account of that act, as trafficked women. If she does not, then she is **cherry picking** from the act and her legal basis for her argument dissolves into inconsistency. Kapoor thus has no consistent legal position she can present that supports her position based on U.S. Public Law 106-386, the trafficking act.

(b) Types of Trafficking

Kapoor (2004, p. 2) states that one should not call source, transit and destination trafficking *types* of trafficking but rather *elements* of trafficking since *types* implies that "they can be seen as independent from each other." Perhaps, but that is also quite specifically what *elements* means. This is simply meaningless quibbling by Kapoor.

III. Defining and Counting Trafficked Persons

(a) Ethnicity

Kapoor states that ethnicity should be defined by place of birth rather than by culture, language, and lineage. She does not tell us why this would help the discussion or measurement of trafficking: what is the relevance? Everything I have observed over the past 15 years of working on trafficking related issues indicates that ethnicity as I define it, is a major variable in human trafficking and human interaction. When the Khmer say they want the Vietnamese to "go home" they do not limit their thoughts to persons born in Vietnam. When it is said that someone does not care about trafficked Vietnamese, it is meant that they do not care about persons of Vietnamese ethnicity trafficked within Cambodia as well as Vietnamese brought to Cambodia from Vietnam. That is important information to know when dealing with trafficking. Ethnicity, not place of birth, is a controlling variable in the choice behavior of brothel customers, and thus of great relevance to the discussion of trafficking. Kapoor tells us that we *could* define it differently, but not *why* anyone *would want* to do it that way.

Language Ability as a Measure of Trafficking Status

Kapoor goes on to say that:

The recognition of a likely trafficking victim can surely also be done by discovering that a prostitute is unable to speak the local language (Kapoor, 2004, p. 3).

While single language ability will be a possible indicator of trafficked status under some circumstances, such as the first few months in the country, it is both an imperfect measure of that status and a highly time consuming measure to obtain. Given a small number of individual cases, such as the women sent to AFESIP for temporary care after their removal from harm by other anti-trafficking workers, it should be considered as one factor. But a direct question to the manager is both a better indicator of trafficked status and possible to obtain. Checking the language skills of thousands of sex workers across Cambodia would take a bit of time. The suggestion that it should be done raises questions concerning Kapoor's familiarity with practical social science research, and with brothels as they normally operate.

(b) Age

Kapoor wonders in print why I present a detailed discussion of the measurement of age. The measurement of age relates to whether a person is legally a child or not. Whether one is or is not legally a child has a direct bearing on whether a voluntary worker is counted as trafficked in enforcement of the U.S. trafficking laws. It relates directly to whether a Western conception of age can be exported whole, to sex work in Southeast Asia. It is also fundamentally important to understand when we are speaking of children, whether we are talking about a 17-year-old or a 7-year-old.

Kapoor then states that "age is not such a relevant factor in quantifying trafficked victims." Such a belief flies in the face of the U.S. definition of trafficking, part of a law on which her legal position relies, which regards all persons under 18 engaged in sexual activity for profit as trafficked, regardless of other factors. If Congress were to change the definition of a commercial sex act (U.S. Public Law 106-386 SEC. 103 (3)) to a more reasonable and enforceable standard as I suggest, then voluntary workers under 18 would not be defined as trafficked if we accept Kapoor's reasoning that age is not a relevant factor. Perhaps it is not that relevant with older teenagers and persons capable of making a free choice. But younger children are clearly not in that category and so any sexual work on their part is rightly considered to have been made without their choice. That is why identification of actual chronological age by the Western definition of age is important. I present the argument in detail (Steinfatt, 2003, pp. 20- 23).

Age can be gauged quite accurately in much of Asia from a simple question about the individual's birth animal (Steinfatt, 2003, pp. 20- 23). Whatever Kapoor's perception, or the perception inside Cambodia, the outside world is specifically interested in the extent of the involvement of children in sex work in Cambodia. It is difficult to see how Kapoor could fail to note that point. Additionally, the discussion of age is intended to inform policy makers and law enforcement officials of the difficulties involved in measuring the age of a specific individual in Southeast Asia who might be engaged in sex work.

(c) Contracts

After quoting at great length my explanation of some of the *modi operandi* that bring women and girls into brothels, Kapoor refers to my explanation as an argument. It is not an argument. It is a description of several methods by which women and girls are brought into brothels. Any researchers who actually observe the process, rather than relying on widely circulated myths of how the process works, will see how it works for themselves. Kapoor may or may not like the way it works, but that is the way it works. She then leaps headlong into a discussion of how this type of "argument" – it is a description of observations, not an argument – could be used to justify FGM, claiming that I am implying that because the process works as I have described it, FGM is an acceptable process. That is simply another absurd contention. Kapoor

twists what is on the page to make it read the way she would like it to read. I do not imply it, I do not say it, and I do not think it acceptable. It is simply one way that the process works. If a police officer or an expert on police matters describes how some murders occur, would Kapoor assume that the police thus support the murders or the rationale behind murder used by murderers?

Kapoor questions how a mother bringing her daughter to a brothel can be considered anything but selling. I suggest she ask that question of the mother, not of me, since it is the mother's perception we are concerned with at that point, not mine, as stated in the passage that Kapoor quotes at length. The expressed perception of many persons bringing relatives to work in the brothel is that an advance payment for work to be performed has been tendered, and now the work must be done since payment has been received. Kapoor then somehow leaps to the conclusion that since some people's reported perception of the advance payment is different from her perception, that this is like saying that bonded laborers in India and Pakistan are not enslaved. The logic behind that contention escapes me completely. Reporting of perceptions in social science research does not imply acceptance of those perceptions by the researcher. That should be so obvious that it need not be said. Much of Kapoor's writing at this point of her response is not intelligible to me.

The Effect of Contracts and "Rescues" on Younger Victims

In Steinfatt (2003, p. 24), I point out the potential effect of "rescues" of indentured workers on younger sisters who may be forced to take the place of the "rescued" girl, describing how and why that happens. I believe that that statement is the first time that this point appears in print, though I cannot be sure. Swingle and Kapoor acknowledge that contribution on my part (2004b, p. 14) in the area of understanding how contracts and debts apparently work together with "rescues" to bring younger victims into the brothels. Yet Kapoor (2004, p. 4) asks if we are ignoring the fact that "a younger sibling" of a rescued girl "may have to take her place" in the brothel. Given that that is my point, not hers, and that she acknowledged that it was my point a few pages earlier, it seems odd that Kapoor now writing alone would question it. She appears to base this on the assumption that the definitions she prefers are sacred cows that cannot be attacked, but I remain unsure of the point she is trying to make here or of its relevance to the discussion.

Then in another of the out-of-the-blue *non sequiturs*, Kapoor says "The argument that this is not a trafficked victim . . ." Kapoor (2004, p. 4). Now where would Kapoor get the idea that such a person is not being counted as trafficked in our studies? Of course such a person is counted as trafficked, as is made clear in our definitions.

And then Kapoor invents, completely out of context, another argument she claims that I make. In the context of discussing how women and girls are brought to brothels, I point out that one common way this occurs is for a relative to transport the girl and ask for money at the door of the brothel, rather than to make a deal prior to arrival. Kapoor takes that sentence out of context and suggests that I am claiming that Vietnamese people in general, not those who send daughters to brothels as is quite clear from the context, engage in debt bondage. She says "to say that this type of debt bondage is common in Vietnam is a huge generalization that is often used as a stereotype" (p. 4). At the University of Miami I teach intercultural communication and cultural diversity. It is what I do. I have a co-authored text discussing such stereotypes (Rogers & Steinfatt, 1999). That is clearly not what I said, and I find it difficult to believe that Kapoor can take something directly out of a clear context, fabricate a point I did not make, and then set it up for criticism as straw man.

Yet then, Kapoor admits that I did not in fact say what she says I said, and apparently withdraws the charge. ("Although Steinfatt did not go as far as saying this was a justification . . ." Kapoor, 2004, top of p. 5). I am somewhat mystified by this form of argument, and simply leave this to the judgment of the reader.

(d) Indentured Prostitutes vs. Consenting Prostitutes: an Erroneous Dichotomy

Kapoor (2004, p. 5) states that to believe a woman could “voluntarily decide to enter an indentured contract is a falsity.” She supports this by arguing that consent is “irrelevant” since “These types of contracts are illegal.” My research is not concerned with legal issues, but rather with the actual state of the world. Of course a woman of legal age can consent or not consent, regardless of how a particular law construes such consent. She may also refuse to consent within herself, while feeling unable to refuse outwardly, regardless of how Kapoor may believe that laws construe that process. While UN and US officials will and must regard research results according to applicable laws, I have no intention of slanting my results so that they either support or attack such laws. I report what our research shows.

In early May I signed an agreement to do contract work in Miami relating to educational qualifications of visa applicants, and I was paid upfront before the work was even begun. The person contracting with me for the work has done business with me before and trusts me. I can be regarded as an indentured worker with respect to that contract. I am obligated to either do the work or return the money. I do not consider myself trafficked and I am not trafficked. Similar examples abound in all societies. It is not uncommon for trafficked women finishing an indentured contract in a brothel, women who *at that point* are free to leave and return home, to ask the manager if they can stay to earn more money. Prior to that point, indentured workers are clearly not free to leave, unless they have the funds to pay all that the brothel owner says that they owe. Some women stay and work longer and some do not. We count even women who actively choose to stay, regardless of the reasons they give themselves for doing so, as *trafficked* since our definition of trafficked persons for these papers is ‘ever trafficked persons,’ not just currently trafficked. Kapoor states that this request for a contract by the woman constitutes trafficking on the part of the brothel owner in and of itself, and she attempts to create an issue out of it. She may be right. Yet there is no issue since her choice does not affect the count we obtained in any way. Readers may make their own judgment as to whether they would regard an individual – previously trafficked or not previously trafficked – who requests work in a brothel and an upfront payment for it, as trafficked *by that act of request*, or not.

(e) Review of General Methodological Concerns

Kapoor (2004, p. 5) states that my study has “too many estimations and assumptions which result in a loose and generalized quantification of the victims.” Aside from the portions of the theory of sex work we state and the statistical projections from observed data, the main “assumptions and generalizations” alleged to exist in the Steinfatt Reports appear to be the inventions of Swingle and Kapoor relating to our work.

Miscellaneous Repetitions of Points Made in Swingle and Kapoor, by Kapoor

Quick counts again. Kapoor goes on to say that “Low time consumption methods resulted in quick counts of villages in (sic) towns in a matter of hours or sometimes minutes” (Kapoor, 2004, p. 5). That is absolutely correct. Villages can often be surveyed in well less than an hour and towns in less than a day – this was asked and answered in my critique of Swingle and Kapoor, above. Recall that villages very seldom have sex work. Whether they do or not takes little time to discern. If it does exist we spend as much time as needed to gather information. If not, we leave. Since most villages have no sex work, the average time spent in them is brief.

Moto drivers again. She then questions again the use of moto drivers and locals as informants and data collectors, followed by the statement “It is contended that such counts can be regarded as accurate” (Kapoor 2004, bottom p. 5). We presume Kapoor means the opposite of what she says here, or perhaps she simply means to say that this is our contention. Kapoor has engaged in this argumentative form above – first contending that there is a problem with my studies and then stating that there is not – and we are never certain specifically what she is trying to say. Whatever it is, for or against my position, she is simply making unsupported assertions here – she presents no reasoning as to why moto drivers and locals would not be

good sources of information on local establishments, and certainly presents no evidence on the point. It is a naked assertion. Would Kapoor need a Western trained research team in order to inquire about the existence or location of a grocery store or what kind of items it had for sale? Would such teams find these stores with greater validity and reliability than a local person asking the same question? Can she demonstrate that with any form of data? While brothels are lower class than grocery stores, every adult knows whether there is one in a village and if there is, where it is located. Only men will likely tell you where it is.

The great majority of Cambodian villages do not have sex workers. Those that have them usually are larger villages that have perhaps one or two women who will accept male guests into their home or some other location during certain hours. These women usually have no manager and will often congregate at one location, presumably to create a greater draw for potential customers, and can sometimes be spotted by the existence of two or three women sitting in front of a hut who continue their gaze at male passersby a bit longer or smile a bit more than common for Khmer eye contact and smiling norms. One can usually learn of the existence or non-existence of such a place in a very few minutes, and then get data from it in a few minutes more. This requires a native rural Khmer male to speak discretely – out of the hearing of women – to several local males, ideally between 20 to about 55, who are often moto drivers. In many villages too small to support a moto driver, males of the same age group walking along the street can be asked about the existence of sex work and directions to it. Out of the hearing of women, men are forthright in response to such questions. Local women usually know the locations as well, but getting the information out of them is both difficult and presents an embarrassing situation for the Khmer researcher. We noted that while the correlation was not perfect, villages without a moto driver were usually smaller and also had very little, if any, sex work. We presume, based on our experience in villages across Cambodia, that the population density required to support a moto driver is related to that required to support a sex worker.

Reference to Evidence Not Made Public

It is after studying with trained investigators that AFESIP has realized the real depth of trafficking for prostitution purposes and also the dynamics of the sex sectors (Kapoor , 2004, p. 5, bottom).

This is a very arrogant statement. It makes multiple knowledge claims, while evidence for none of the claims is presented. By this accounting no one gets to see that evidence except AFESIP, and they get to tell the rest of us how they have learned everything everyone else does not know about trafficking. This occurs through their mysterious sources from above.

Who are these trained investigators? How did they get their training and knowledge? What are the credentials of these trainers re sex work in Southeast Asia? Who trained the trainers, and how did those who trained the trainers obtain their knowledge of sex work and trafficking? How and why was the trainer selected? Was the person who made this selection qualified to do so? How and why was s/he trained and in what ways? What methods were used in this training? What methods were used in the research that provided the knowledge cited about "the real depth of trafficking?" What did that research find? Where is our copy of it so we do not have to rely on AFESIP claims that it in fact exists, was well done, and found what AFESIP says it found?

It does AFESIP great discredit to argue in this fashion, and I submit that AFESIP's academic credibility is exactly *zero* in this regard. It is also a selfish form of argument to claim to have information containing great and important knowledge that would certainly advance our understanding of trafficking if it existed and was produced, but yet to fail to produce even one publication based on it. It lacks intellectual courage to claim to have information that gives one "real depth" of knowledge on trafficking, but to fail to state its nature and produce that information for scrutiny.

Where are AFESIP's papers or publications on this great depth of knowledge that they possess and have learned from these unknown agents? For approximately the past decade AFESIP has been gathering data but has never produced a single publication concerning that data. They have received substantial funding from multiple sources not just for caring for persons brought to them, but specifically earmarked for engaging in competent research which produces results useful for reducing future human trafficking. They produce nothing.

In response to my formal question to the AFESIP representative at the 11 May 2004 roundtable, he responded that there were two such data sets in existence, but that no date could be given for any possible publication of any data based paper concerning these alleged empirical data sets. It is my understanding that for many years the US State Department, USAID, and the Asia Foundation, among others, have been requesting at least one publication based on AFESIP's alleged data that could be examined for content.

I have personally made that request to AFESIP in every contact I have had with every representative of AFESIP, beginning with my initial meeting with Pierre Legros and Somaly Mam at their AFESIP Office in Phnom Penh in March 2003. In fairness, at that meeting Pierre placed an inch-thick loosely bound set of computer printouts on his desk and invited me to look through them. He represented these printouts as data on human trafficking. It was difficult for me to see what was on the printouts as I was seated across the desk from him, and he was flipping through the pages. I asked to look at the frequency distributions or other data summaries in a manner in which I could read the figures. He did not allow this. Pierre stated, in essence, that more work needed to be done to provide any conclusions. I offered to assist with the data analysis and he responded that another American professor had already agreed to assist. Without naming the professor, Legros related a story of the reaction and the shock expressed by the professor upon entering a brothel at the activity occurring on within. Thus, Legros implied that data on human trafficking existed in the AFESIP office at that time. But presenting a printout that I could only view sideways from a distance and was not allowed to examine, does not indicate clear evidence of anything except the existence of a printout of an unknown something.

I publish papers based on my data and bring my work into the public forum for scrutiny. AFESIP is willing to argue from conclusions they claim are based on analysis of what they claim is their own data, and willing to cite this alleged data as evidence. But AFESIP will not open either the alleged data or those presumed analyses to useable scrutiny by any outside observer. It is acceptable to have unanalyzed data. It is not acceptable to refer to that data as evidence.

Even More on Indirect Workers

Kapoor then says we ignored "the obvious and direct places of prostitution that were closed at those times" and all indirect sex work venues. This is another method invented by Kapoor that does not represent our actual methods. Also, Kapoor and AFESIP appear not to know that direct venues with indentured workers are always open, even when everyone inside is asleep. One of the many human rights problems with such brothels is that the workers can and will be awakened at any time when a customer appears. We investigated all known direct and indirect venues, both on and off the street. We did not investigate permanently closed places. As we stated (Steinfatt, 2003, p. 9), we investigated all places not open 24 hours, either during the times they were open or through location of a manager or owner when the place was closed. Owners and their families usually live in the shop where they work. Failing these other methods, we asked the moto drivers or other informants "how many workers he had personally seen at the site, their ethnicities, whether there were younger workers, and whether the women were free to leave the establishment" (Steinfatt, 2003, p. 9). This was necessary in less than a dozen locations.

More on “Hidden” Brothels

Kapoor (2004, p. 6) discusses my contention that so called “hidden” sex work locations cannot be so hidden that no one can find them. This point was asked and answered under Swingle and Kapoor above, but is perhaps in need of an additional statement. I contend that, in essence, hidden sex work venues involve a contradiction. This is so because the motive is money. No customers, no business, no money. Repeat customers who never talk to anyone else are almost never sufficient to support a business when the greed motive of the owner is considered. Yet Kapoor says “the hidden places of trafficked victims cannot be underestimated.” It seems likely that again she means the opposite of what she says literally, that they *can* in fact be underestimated but *should* not be. But then we would be putting words, or thoughts, onto her mouth.

If Kapoor is contending – and the reader should not have to guess the meaning – that a few women are held for short lengths of time at unknown locations prior to being moved into sex work locations, then that is possible. Or if she contends that there are a few places where a few women are held and never moved to a location where they will be noted and counted, that is possible but highly unlikely. But she and AFESIP only allege that this is true – they produce no evidence. If it is true, then the longer they are held there and the more women who are involved, the greater the expense and financial loss to the owner. Owners do not like high expenses and lack of income on top of it, so they would move such women, if they existed in their supply chain, into a working venue or higher income-producing venue as soon as possible. For that reason I regard such in-transit or “hidden” cases as a negligible factor in the numbers, and seriously doubt that AFESIP can provide any actual evidence to the contrary. If there were large numbers of women in transit or temporarily “hidden,” then we should see a constant strong increase in the number of workers in sex work venues. The only reason this would not be true is if AFESIP has additional data to show that workers are retired or are moved out of Cambodia at about the same massive rate that Kapoor contends exists among in-transit workers. We do not see evidence of any such trend in our observations, either in retirement rates, in increases in the numbers of sex workers, or in large numbers or even small numbers, of Khmer women shipped abroad for sex work. If AFESIP has such evidence let them present it. Thus Kapoor’s contention re “hidden” locations is a completely unsupported assertion that appears to be incorrect and without merit.

IV. Conclusion

Most of Kapoor’s conclusion is a summary and adds nothing not already discussed. I am still amazed by her belief that a research paper on trafficking should not discuss how trafficked victims can be identified. She refers to that as “irrelevant information” (p. 6). Had we followed her advice, then that would have been an actual argument against my study – failure to state how we defined, and thus counted, the number of trafficked women.

One apparently new item is Kapoor’s contention that my paper might have been good if I had just stopped with showing that previous very high estimates, such as 80,000 to 100,000 trafficked women and children per year in Cambodia, were based on pure speculation: “It is felt that a criticism of the previous statistics would have been sufficient” (Kapoor, 2004, p. 6). By this line of thinking it would be acceptable, or politically correct, to say that really high numbers are wrong, but not OK to pin that number down to a reasonably exact smaller estimate. This statement by Kapoor seems to support Legros’ statement quoted above, that AFESIP’s concern is with the results, not with the methods. And the concern with results is that they must show large numbers so AFESIP can have a rationale to continue to push donors for large amounts of funding. This style of argument is also supported by Swingle’s repeated requests for data, while claiming to be concerned only with methods, yet ignoring details of methods when offered.

As her final point, Kapoor repeats an erroneous premise from her response with Swingle, suggesting that arguments saying that my research is “better than nothing” are incorrect. This is a straw man: I make no such arguments. My research is of high quality and my estimates are accurate. If Kapoor, Swingle, and

AFESIP want to argue them in the intellectual and research arena they will have to provide evidence of their own which demonstrates the truth of their arguments, in addition to competent argument connecting that evidence to their conclusions, and competent discussions of the research of others. They do not seem ready or able to do that at this point.

A FINAL SUMMARY OF THE CRITIQUE

In summary, the concern of the Steinfatt studies is to assess the number of sex workers and sexually trafficked women and children in Cambodia, which in employing our method also provides information on locations and methods of trafficking in persons. Knowing the numbers allows the government to assess and set trafficking policy in the most efficient manner and evaluate the effects of trafficking reduction programs. The use of moto drivers as informants provides one of the best possible ways of obtaining data on trafficked women and children. We selected this method after careful study and it produces data that are reliable and valid. Recognized experts in research methods have examined our methods at a formal meeting of experts on human trafficking created for that specific purpose. Both the experts and we believe our methods are innovative and produce accurate data.

AFESIP's detailed concern with the methods used in our research contrasts with their total lack of concern with the methods of any other report of trafficking numbers for Cambodia, and their absolute refusal to publish any of what they claim to be their research. Why would AFESIP publish their concerns with our sound methods now, when they published no concerns whatever about fabrications of erroneous estimates of trafficking numbers spread by several NGOs, including AFESIP, as addressed in Steinfatt (2003)? AFESIP produces no actual research while receiving funding for research. At best AFESIP appears to collect data on occasion but leaves it unanalyzed. The explanation may lie in the contrast between the two different rationales AFESIP gives for producing their response to our research. Swingle and Kapoor (2004b, p. 2) state that "the deficiencies in Steinfatt's research methods highlight a need for a high-quality enumeration of the prostitute population in Cambodia under the expert guidance of a demographer or sampling statistician." Their stated goal is to produce a response to the Steinfatt studies because Swingle and Kapoor claim to question my methods. Legros, who with Somaly Mam sponsored their response, says that the purpose of their response is political, that it was written because AFESIP does "not agree with your conclusions and think that number (sic) are really underestimated." I leave it to the reader to decide which of these rationales stated by AFESIP is the more accurate: a true desire to question methodology, or a desire to obscure carefully conducted research results likely to harm them financially.

AFESIP does not appear to understand the nature of human trafficking or the landscape of rural Cambodia. AFESIP principals did not read and understand our papers, nor did AFESIP understand the landscape of rural Cambodia, or its villages, cities, and towns outside of Phnom Penh. Their response is based on a misunderstanding of how trafficking for sexual purposes operates in Cambodia. In AFESIP's published response, Swingle and Kapoor argue that there are large cities in Prey Veng, Pousat, and Takeo, yet such cities do not exist, as we point out in the papers they were critiquing. How can Somaly Mam have conducted the very large number of rescues of trafficked women and children she claims to have conducted outside of Phnom Penh when she appears not to understand what that countryside looks like? AFESIP's response also demonstrates a general lack of knowledge of and experience with actual field research and with data collection methods in less developed nations. They do not consider the many pitfalls involved in their advocated form of training of interviewers, and they fail to grasp the quality of the data that can be produced by observers intimately familiar with a place and its customs. AFESIP refused my offer of any methodological help or clarification they needed. Yet in many cases, the arguments AFESIP makes are not arguments against our actual methods, but against methods they created that they claim to be our methods. They falsely attribute poor methodology to the Steinfatt studies and then attack their own inventions and not our actual methods.

The AFESIP respondents also did not read and understand the listed methodological references in the Steinfatt studies. Yet based on their creation of methods that they attribute to me, they imply that *my* work is ethically objectionable. Such conclusions and statements are simple *non sequiturs*. Their argument that all sex workers, voluntary and otherwise, must be regarded and counted as trafficked women, based on language in the *Victims of Trafficking and Violence Protection Act 2000*, ignores sections of the act that nullify their conclusion. If adopted, their interpretation would move resources away from trafficking victims in Cambodia. We do thank them for their statement that called our attention to a misstatement in our 2002 report.

AFESIP consistently refuses to publish any study based on their collected data, yet they refer to their findings in these unpublished studies as *evidence*. Given that AFESIP stops referring to unshared studies, to unknown and unnamed experts, and to unanalyzed data as *evidence*, perhaps that failure to publish is ultimately a good choice. Their data from several years past are already old and largely of historical interest except as entries in a time series with current data. And based purely on the quality of their response to my work, Somaly Mam, Pierre Legros, Joseph Swingle, and Aarti Kapoor, individually and collectively, appear ill equipped at their present level of knowledge to conduct and analyze field research on sex work and trafficking in less developed nations. AFESIP's willingness to house Vietnamese victims of trafficking, when other NGOs cannot or will not, is admirable. Let them continue to do what they may do well, and defer their research attempts to a later time.

Out of all of the objections they raise with the Steinfatt studies, none of these objections remains standing in the face of scrutiny. None. Each of their objections to the Steinfatt Reports is either invented or irrelevant, save the minor misstatement in our 2002 report, for which I apologize. The numbers of sex workers, and of trafficked women and children in Cambodia, are well represented by the numbers presented in Steinfatt, et al., (2002) and Steinfatt (2003).

And a final reminder: The Asia Foundation International Roundtable of May 2004, composed of internationally known scholars and experts on human trafficking discussed above, also reviewed Swingle and Kapoor's critique of our research. They did not find its points convincing. Our method is the recognized standard for the type of sex trafficking found in Cambodian brothels until a better method is tried, tested, and can be shown to produce valid results. It is not, nor is it intended to be, a method necessarily applicable to all human trafficking in all cultures and countries at all times. Such methods must be empirically determined and tested for specific cultures and types of trafficking, as was our method for Cambodia.

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