Letters to the Editor

Response to *Who Among Us?* (Review)

In a recent review of a prostitution anthology, the author states that "It is not by accident that the greatest number of prostitutes in the United States is in Colorado Springs, the home of the Air Force Academy. Prostitution has become institutionalized there" (Fithian, 2000, p. 92). We are aware of no data in the reviewed volume or elsewhere supporting either assertion.

First, the greatest number of prostitutes *estimated* for any of the last 30 years in the Colorado Springs region (current population: about 500,000) is roughly 200. The annual mean number of prostitutes *observed* between 1970 and 1999 is 103, with a range of 57 to 174 (Potterat, Woodhouse, Muth, & Muth, 1990, p. 238; Potterat, Rothenberg, Muth, Woodhouse, & Muth, 1999). Many North American metropolitan areas have larger populations of prostitutes, both numerically and proportionally (Berry, Kanouse, Duan, & Lillard, 1992; Decker, 1979; Rossmo & Routledge, 1990; Weiner, 1996).

Second, although servicemen once constituted the largest group of "johns" in Colorado Springs, a region hosting 4 large military installations staffed by about 30,000 troops, they relinquished that dubious distinction more than 20 years ago. Advent of the all-volunteer Army during the mid-1970s attracted older, and therefore sexually more stable, recruits who were not as inclined to solicit prostitutes as (typically younger) conscripts. Our health department's continuous ethnographic observations since 1970 point to servicemen's substantially diminishing demand for prostitutes over time. More relevantly, ethnographic and epidemiologic evidence indicates a minor role in such demand by Air Force servicemen, with cadets rarely being implicated with use of prostitutes locally.

We trust that the multifarious reports published in the literature as a consequence of our work with prostitutes do not mislead readers into an exaggerated impression of the magnitude or direction of prostitution in Colorado Springs. Such a subtle effect of publication bias is distorting.

Faithfully,
John J. Potterat, B.A.
Tisha Dowe, M.D., M.P.H.
Devon D. Brewer, Ph.D.

References

Berry, S. H., Kanouse, D. E., Duan, N., & Lillard, L. A., (1992). Risky and non-risky sexual transactions with clients in a Los Angeles probability sample of female prostitutes. VIIIth International Conference on AIDS/III STD World Congress, Amsterdam, July 19–24, Abstract PoD 5604.

Decker, J. F. (1979). Prostitution: Regulation and control. Littleton, CO: Fred B. Rothman & Co.

Fithian, M. A. (2000). Who among us? (Review of *Prostitution: On Whores, Hustlers, and Johns*. Edited by James Elias, Vern L. Bullough, Veronica Elias, and Gwen Brewer. Amherst, NY: Prometheus Books, 1998). *The Journal of Sex Research, 37,* 91–92.

Potterat, J. J., Woodhouse, D. E., Muth, J. B., & Muth, S. Q., (1990). Estimating the prevalence and career longevity of prostitute women. *The Journal of Sex Research, 27,* 233–243.

Potterat, J. J., Rothenberg, R. B., Muth, J. B., Woodhouse, D. E., & Muth, S. Q. (1999). Invoking, monitoring, and relinquishing a public health power: The Health Hold Order. *Sexually Transmitted Diseases, 26,* 345–349.

Rossmo, D. K., & Routledge, R., (1990). Estimating the size of criminal populations. *Journal of Quantitative Criminology, 6,* 293–314.

Weiner, A. (1996). Understanding the social needs of streetwalking prostitutes. *Social Work, 41,* 97–105.

Response to Potterat

I wish to thank John Potterat, Dr. Tisha Dowe, and Devon Brewer PhD. in bringing to my attention that I had not included "number of prostitutes per capita" in the book review. I should also mention that the article came out in a publication about five years ago. I was in Colorado Springs when it came out and I read it while there. I had some question myself. Therefore, I made extensive inquiries among friends, family, and business people in the community and people I met as well as young women dating cadets. Their responses did not debase the article but I felt it did not cover all of the problems. I also was in social situations and had contact with about fifty cadets.

At an earlier date I had social contact with some of the families and friends of cadets. This is an entirely different population contacted than that of the study by Potterat et al. The people I had contact with were people in the area, who in one way or another were effected by prostitution in the community, mainly as it applied to the cadets. It was very unlikely that their concern was for the street prostitute. It is unlikely that a street prostitute would spend a weekend in Bear Creek or be asked to go to dinner and the theater in Denver. Wives and sweethearts were not happy about this. Sometimes behavior was in a group. A place would be rented for a weekend party. The above letters indicated that "Cadets and prostitutes don't mix." I would be delighted to learn if this was no longer the case. All of the above information was in relation to that source.

This was not a scientific study but this and subsequent information and longitudinal histories of Air Force families indicated behaviors and values that might have evolved at the time cadets were there. This is only privately gathered information which is why I suggested a look at this longitu-

dinally in a more scientific way, more for the long term effect on Air Force personnel and families as they continue their careers.

Since doing a research paper about 40 years ago on prostitution I have continued to read journal articles, books, newspapers, and other literature on the subject as well as interviewing prostitutes and call girls. I have had contact in social situations with a number of them and am close friends with authors of books and articles on prostitution. Some prostitutes have come in for therapy. I also covered the topic in teaching classes at the University. Only about one percent of those men and women that I had contact with were ever street prostitutes or arrested. Most tended to be better educated, have other skills, and were more concerned and careful about being identified. They tended to become involved in safe relationships. If they had other jobs they were ones often where they had considerable contact with males where they could make "safe" arrangements.

The journal article by the authors of the letter, "Estimating the Prevalence and Career Longevity of Prostitute Women" (Potterat, Woodhouse, Muth, & Muth, 1990) was an excellent article. The source of information was gained through the police department and health services in Littleton, Colorado.

This means that this was a street prostitution sample, which is only the tip of the iceberg wherever prostitution exists. Most sex workers are not arrested and go to their own private physician for services when needed. As reported in the book on prostitution they tend to use or have their partner use contraceptives and are careful in their selection of partners. I find no fault with their article as it applies to street prostitutes, which are the most visible and which we know the most about.

Over the last 30 years I have taken 2- to 7-hour sex histories from males, many former servicemen. I have gone into their exposure to past experiences with prostitution where it occurred. Histories of former Air Force personnel were very limited, mainly due to fear of loss of job since often they become commercial pilots when leaving the Air Force. Wives were more apt to talk to me on a nonprofessional basis about what they saw to be a problem. The behavior of some of these men seemed to be quite different and more institutionalized than that of other servicemen. The behavior was reinforced by a recent several-year contact with an Air Force family stationed overseas and others who were no longer in the military.

Some behavioral problems with Air Force personnel were reported in the newspaper and a book a few years ago.

Marilyn A. Fithian, Ph.D.

References

Potterat, J. J., Woodhouse, D. E., Muth, J. B., & Muth, S. Q. (1990). Estimating the prevalence and career longevity of prostitute women. *The Journal of Sex Research, 27,* 233–243.

Response to *The Woman as Final Arbiter: A Case for the Facultative Character of the Human Sex Ratio*

The authors offer some propositions which I take to be logically flawed. These may be summarized as follows:

(A) the authors write in their abstract '(1) sex determination is random, or (2) sex determination is facultative or biased', (B) the authors claim (1) there is evidence against Weinberg's Rule—that among human dizygotic (DZ) twin pairs, there are almost exactly equal numbers of same-sexed (SS) and opposite-sexed (OS) pairs—and that (2) this evidence constitutes grounds that support 'the biased or facultative position' (their p. 169).

I shall deal with these points in order.

Point A

In this context the word 'facultative' may be taken to mean "tending to confer reproductive advantage as e.g. in producing more grandchildren." There are many examples of offspring sex ratios of subpopulations which differ significantly from those of their parent populations, such as the offspring sex ratios of men exposed to a number of deleterious chemicals (e.g., dioxin: Mocarelli et al., 1996; and boron: James, 1999); or men destined to suffer from various diseases such as prostatic cancer (James, 1990) or testicular cancer (Moller, 1998). However there are no grounds for supposing that these sex ratio biases are facultative: indeed it is invalid to infer (without further argument) a facultative function for an established sex ratio bias. The point has been satirized by Gould & Lewontin (1979). It is only from documented reproductive advantage that one may infer facultative sex ratios.

Point B

Most of the data in the authors' Table 1 was reviewed in the most recent study cited in that table (James, 1992). It is worth explaining why the evidence in that table (which at first sight looks overwhelming) remains indecisive.

In estimating the proportion of DZ twins which are SS and OS, it is usual to observe the following rules in regard to a randomly ascertained sample of twin pairs.

(1) All opposite-sexed twins pairs are DZ.

(2) For practical purposes, all monochorionic twin pairs are MZ.

When these two categories of twins are set aside, one is left with the subsample of same-sexed dichorionic pairs. These may be either MZ or DZ. These twins of unknown zygosity are then subjected to a series of tests by genetic markers (e.g., of blood groups). Each pair which is discordant on any marker is diagnosed DZ. However, after the testing, the remaining twin pairs (which are concordant on all markers) contain all the MZ pairs *plus* some undiagnosed SS DZ pairs which (by chance) happen to be concordant on all the markers so far employed. It is, in principal, possible to estimate this number of concordant SS DZ pairs by applying the same set of markers to the *opposite*-sexed pairs

(which are known to be DZ). However, though the OS twins in many of the cited studies were tested on such markers, it is clear that this testing was, in general, not so rigorous (viz on not as many markers) as that on the SS pairs. This is for a good reason. The main purpose of the markers is to establish whether a pair of twins is MZ or DZ. But that is already known in the case of OS pairs! Hence the number of SS DZ pairs concordant for all markers tend to be over-estimated.

So, *pace* Coney and Mackey, no firm conclusion may be reached from the data in their table. With modern genetic techniques, the relative numbers of SS and OS DZ twin pairs will be established—but, as far as I know, accurate evaluation of the ratio has not yet been published.

In short, there is, as yet, no compelling evidence against Weinberg's Rule. And even if there were, that would not affect the status of the proposition that (some) sex ratio variation is facultative. The two propositions are logically independent as I noted (James, 1997a).

I should like to make some observations on whether (some) human sex ratio variation nevertheless *is* facultative and, if so, whether it is controlled by the "woman as final arbiter."

Coney and Mackey (1998) cite a number of papers suggesting facultative variation of sex ratio in a number of non-human species, and it would seem likely that there is some such variation in human beings too. However, I would suggest strongly that some human sex ratio variation is not facultative but is incidental to other processes. There is now very substantial evidence that mammalian (and among them, human) sex ratios at birth are partially controlled by the hormone levels of both parents around the time of conception (James, 1996). The relevant hormones include testosterone, estrogen, and the gonadotropins—which themselves are markers of health. For that reason there is some initial suspicion that *in general,* human sex ratio variation would reflect the health of parents and thus have facultative consequences. However the profiles of these hormones differ with different disease processes. Consider the T/G ratio where T and G are a man's standardized levels of testosterone and gonadotropin. This ratio is high in men destined to suffer prostatic cancer (Bosland, 1988) and low in men destined to suffer testicular cancer (Petersen et al., 1999). In conformity with my hypothesis, these men reportedly have significant excesses respectively of sons (James, 1989) and daughters (Moller, 1998). Yet in the absence of direct testing, it would seem unlikely (if only because of their opposite skews) that these sex ratios are facultative. Rather they would seem incidental to a general process which has overall facultative functions.

As to whether the woman is the "final arbiter," the examples cited above (of sex ratio variation with paternal diseases and chemical exposures) suggest that the father makes a direct contribution to the "decision" about the sex of offspring. Accordingly, I have suggested a mechanism for sex selection in which *both* parents have a role (James, 1997b).

William H. James

References

Bosland, M. C. (1988). The etiopathogenesis of prostatic cancer with special reference to environmental factors. *Advances in Cancer Research, 51,* 1–106.

Coney, N. S., & Mackey, W. C. (1998). The woman as final arbiter: A case for the facultative character of the human sex ratio. *The Journal of Sex Research, 35,* 169–175.

Gould, S. J., & Lewontin, R. C. (1979). The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London 'B', 205,* 581–598.

James, W. H. (1989). Parental hormone levels and mammalian sex ratios at birth. *Journal of Theoretical Biology, 139,* 59–67.

James. W. H. (1990). The hypothesized hormonal control of the human sex ratio at birth – an update. *Journal of Theoretical Biology, 143,* 555–564.

James. W. H. (1992). The current status of Weinberg's differential rule. *Acta Geneticae Medicae et Gemellologiae, 41,* 33–42.

James, W. H. (1996). Evidence that mammalian sex ratios at birth are partially controlled by parental hormone levels at the time of conception. *Journal of Theoretical Biology, 180,* 271–286.

James, W. H. (1997a). Weinberg's rule and facultative sex ratios. *Mankind Quarterly, 37,* 437–441.

James, W. H. (1997b). A potential mechanism for sex ratio variation in mammals. *Journal of Theoretical Biology, 189,* 253–255.

James, W. H. (1999). The sex ratio of offspring of people exposed to boron. *Reproductive Toxicology, 13,* 235.

Mocarelli, P., Brambilla, P., Gerthoux, P. M., Patterson, D. C., & Needham L. L. (1996). Change in sex ratio with exposure to dioxin. *Lancet, 348,* 409.

Moller, H. (1998). Trends in sex ratio, testicular cancer and male reproductive hazards: Are they connected? *A.P.M.I.S. (Acta Pathologicae, Microbiologicae et Immunologicae Scandinavica, 106,* 232–239.

Petersen, P. M., Skakkebaek, N. E., Vistisen, K., Rorth, M., & Giwercman, A. (1999). Semen quality and reproductive hormones in men with testicular cancer. *Journal of Clinical Oncology, 17,* 941–947.

Response to James

The title of our article is: "The woman as final arbiter: A case for the facultative character of the human sex ratio."

The (primary) human sex ratio can be considered either to be a function of purely random chance, such as a 50/50 split, or to be a function of systematic responses to environmental events, such as the odds of conceiving a son (XY) versus daughter (XX) are biased. Weinberg's Rule argues for independence (of sex) for each conception. *Facultative* means adaptive responses to varying environments. Accordingly, even though primary sex ratios are difficult to measure, if (secondary) sex ratios are found which differ from a 50/50 split, then Weinberg's Rule becomes suspect. If a theory can be aligned with the biased sex ratios, then the "independence" upon which Weinberg's Rule is founded also becomes suspect. We presented five separate data bases which reflect a skewed (secondary) human sex ratio. We also present theories by Fisher (1930) and by Trivers & Willard (1973) which would successfully predict the direction of the bias.

Their theories suggest that the mother-to-be "reads" her environment, and the "reading" would tend to bias a conception/birth more toward a daughter or more toward a son. Given the mammoth differentials in investments between the father and the mother in the gestation of the child and in the nursing of the child and in the (primary) caretaking of the child, the physiology of the woman

would be the better candidate to execute the bias rather than that of the man. It is difficult to make the reverse case.

James writes "In this context the word 'facultative' may be taken to mean 'tending to confer reproductive advantage as e.g. in producing more grandchildren'." Our use of the word "facultative" is its definition, not any implication: to wit—"adaptive responses to varying environments." James then proceeds to give evidence for a facultative human sex ratio. He cites chemical agents aligned with biased sex ratios. He cites diseases aligned with biased sex ratios. He cites hormone differentials which are aligned with biased sex ratios.

James argues that our survey of studies which illustrate the over-representation of like-sex dizygotic twins (DZ) versus unlike-sex dizygotic twins (DZ) could be explained by systematic false positives (i.e. like-sex DZ are overestimated). Such might be the case for each of the 19 of 23 studies which did show a ratio of like-sex DZ to unlike-sex DZ to be greater than 1.00. The mean ratio of the sample of 23 studies was 1.24 ($p < .01$; 2-tailed). That is a lot of false positives.

If the twin study is waived aside, that still leaves the world wide (secondary) sex ratio of approximately 105 males born per 100 females (the primary sex ratio is invariably viewed as higher than 105; how much higher depends upon which author is cited), the sample from the *Who's Who* survey, the sample of U.S. President's children, and the sample of polygynous Mormons of the 19th century. These sex ratios are biased. The bias occurs in the direction that the Trivers and Willard and the Fisher theories would predict.

James suggests that both parents have a role in the mechanisms for sex selection. We agree. But we suggest that the father's influence is indirect in that he influences the mother. It is difficult to conceptualize the man's input at or beyond the fallopian tube. He is part of her total environment which she "reads." The mother is more direct and has last "tags." She is the final arbiter.

Wade C. Mackey
Nancy S. Coney

References

Fisher, R. A. (1930). *The genetical theory of natural selection.* Oxford, England: Oxford University Press.

Trivers, R. L., & Willard, D. E. (1973). Natural selection of parental ability to vary the sex ratio of offspring. *Science, 179,* 90–92.