Archives of Sexual Behavior, Vol. 27, No. 6, 1998

Letters to the Editor

PHEROMONAL INFLUENCES

To the Editor:

Cutler, Friedmann, and McCoy (1998) report that an unidentified "pheromone" placed in aftershave lotion increased human sociosexual behavior during a 6-week treatment period (the pheromone apparently was "Athena Pheromone 10X" which is marketed by Dr. Cutler's Athena Institute [see http://www.athena-inst.com/10x.html]) Of the six behaviors recorded, they concluded that the group receiving the pheromone exhibited greater increases over baseline than the placebo group for two (sleeping next to a partner and frequency of intercourse). They also reported that after counting "[t]he number of individuals perceiving positive changes during any of the experimental weeks" (p. 7) a higher proportion of "pheromone" users perceived positive results during the treatment. Re-analysis of their data, however, raises serious doubt about all of these conclusions.

Consider first the subject's perception of positive results. Cutler *et al.* report 47% (8 of 17) of pheromone users and 24% (5 of 21) of the placebo group perceived positive changes and that this was a significant difference. Even in a one-tail, fair test of a difference between two proportions (which Cutler *et al.* apparently use in the remainder of their comparisons) it is not ($p > 0.10$; Fisher Exact One-Tail Test). While the test used by Cutler *et al.* (Z test of a proportion) may be appropriate to evaluate the significance of a single proportion against an anticipated result, e.g., the observed proportion of heads in n tosses of a coin versus the anticipated 0.50, it appears to be used inappropriately by Cutler *et al.*

Next consider sleeping next to a partner and petting. In Table III, the authors claim that 6 pheromone- versus 1 placebo-treated men exhibited an increase over baseline in sleeping next to a partner and 7 pheromone- versus 3 placebo-treated men had an increase in petting. These numbers were determined by requiring that the average value over the 6 weeks of treatment had to exceed the average value during baseline, and that a male's "highest weekly baseline score was exceeded at least once

0004-0002/98/1200-062 7$15.00/0

during the experimental period, if his baseline score was not already at maximum" (p. 7). Including this final qualifier effectively insured that two men in the pheromone group would be included in the group showing an increase over baseline for sleeping next to a partner and one would be included for petting. If the baseline scores are maximal, then it follows that it would be impossible for the mean treatment-value to exceed the pheromone. In all fairness, since Cutler *et al.* included individuals from the pheromone group because the subject's treatment values could not exceed baseline in one of their measures, they should apply the criterion across the other measure. Had this been done, two additional individuals from the placebo group would have been included for sleeping next to a partner (S11 and S27) and one (S11) for petting. After making these changes, the p values become, or remain even more, nonsignificant ($p = 0.13$).

This leaves only sexual intercourse as potentially affected by the treatment. It is notable that the frequency of intercourse for many men in both groups was zero during the 2 baseline weeks. For these individuals, a single intercourse during the 6 weeks of treatment therefore counts as an increase over baseline. In the placebo group there were no individuals among the 10 whose baseline scores were zero who had a single intercourse during the treatment phase. In the pheromone group there were 4 of 11. Interestingly, 3 of these 4 reported their only instance of sexual intercourse in the 6th and final week of the study. In a week-by-week analysis, Fisher's Exact one-tail test yielded $p > 0.10$ for all treatment Weeks 1 through 5 and did not reach significance until Treatment week 6.

With respect to the unidentified pheromone, in earlier work, axillary extracts were prepared (Preti *et al.,* 1986; Cutler *et al.* 1986) and analyzed only for selected steroidal components as described in Preti *et al.* (1987); however the complexity of these extracts precluded any a priori assumption regarding the identity of the components that may have caused alterations in the menstrual cycles (a primer pheromone effect). Neither Preti *et al.* (1987) nor any references cited therein present any evidence suggesting influences on sociosexual variables (releaser pheromone effects) resulting from application of axillary extracts or their components. How then could Cutler *et al.* suggest that "a synthetic version of a pheromone" was derived from the work of Preti *et al.* (1987) after "refining a proprietary formula, characteristic of heterosexual men"? To derive this formula (particularly one that applies to heterosexual men in general, a feat in itself), active ingredients would have had to have been isolated in a bioassay-directed, analytical effort. Nowhere in Cutler *et al.* are data or references provided

that would demonstrate that such activities provide the foundation for their choice of the unnamed, proprietary, active ingredient(s).

We therefore suggest that Cutler *et al.*'s (1998) "Pheromonal Influences on Sociosexual Behavior in Men" become "Phenomenological Influences . . . in Men."

Charles J. Wysocki, Ph. D.
Monell Center, and
Department of Animal Biology
School of Veterinary Medicine
University of Pennsylvania, Philadelphia

George Preti, Ph.D.
Monell Center, and
Department of Dermatology, School of Medicine
University of Pennsylvania, Philadelphia

REFERENCES

Cutler, W. B., Friedmann, E., and McCoy, N. L. (1998). Pheromonal influences on sociosexual behavior in men. *Arch. Sex. Behav.* 27: 1-13.

Cutler, W. B., Preti, G., Krieger, A. M. Huggins, G. R., Garcia, C. R., and Lawley, H. J. (1986). Human axillary secretions influence women's menstrual cycles: The role of donor extract from men. *Horm. Behav.* 20: 463-476.

Preti, G., Cutler, W. B., Christensen, C. M., Lawley, H. J., Huggins, G. R., and Garcia, C. R. (1987). Human axillary extracts: Analysis of compounds from samples which influence menstrual timing. *J. Chem. Ecol.* 13: 717-731.

Preti, G., Cutler, W. B., Krieger, A. M. Huggins, G. R., Garcia, C. R., and Lawley, H. J. (1986). Human axillary secretions influence women's menstrual cycles: The role of donor extract from women. *Horm. Behav.* 20: 474-482.

RESPONSE TO WYSOCKI AND PRETI

We find this letter particularly disturbing because it contains significant misstatements of the procedures used in both the research and the data analysis and reveals that Preti and Wysocki misread Table II. Their arguments then rely on such errors and their conclusions are based on false assumptions, only to be compounded by resorting to "post hoc analysis" to further their arguments.

Our Procedures

In accordance with accepted scientific practice, our hypotheses were developed before the protocol was established. The criteria for classification of subjects preceded the unblinding and examination of the data.

The study, data, and results were then presented to colleagues at the scientific meetings of the American Society for Reproductive Medicine (9/95) in Seattle, the International Academy of Sex Research (6/96) in Rotterdam, and the North American Menopause Society (9/97) in Boston for exposure and collegial feedback.

Perception of Positive Results

We reported "A significantly higher proportion of users perceived positive results during the experimental 6-week period than did the placebo users (47 vs. 24%; $Z = 5.05$, $p < 0.001$)." The Z test we used was appropriate as it is commonly used "for comparing proportions for dichotomous variables" (Vogt, 1993). As stated in our publication, the difference between pheromone and placebo users in perception of positive results was not nearly as substantial as the difference between the two groups in their actual behavioral response.

Wysocki and Preti's Errors

In their third paragraph, discussing Tables II and III of the Study, Wysocki and Preti state that "since Cutler *et al.* included (as increases over baseline) individuals from the pheromone group because the subjects' treatment values could not exceed pheromone in one of their measures, they should apply the criterion across the other measure." It is simply not the case that we applied this criterion inconsistently.

In fact (i) there was no subject "in the pheromone group included as an increase over baseline (whose) . . . values could not exceed baseline . . ." and (ii) contrary to their assertion both groups were treated identically. No subject who started at maximum was treated as an increase in *either* group.

Table II reveals that two married placebo subjects (S11 and S27) recorded "sleeping next to a partner" every day and, thus, were at maximum for both baseline and experimental weeks; one of these (S11) did the same for "affection/petting and kissing." This stable behavior rendered 3 cells of data incapable of demonstrating an increase over baseline: "Sleeping" for S11 and S27, "Affection" for S11.

Overall, 38 experimental subjects each recorded 6 behaviors generating 288 cells of data (38 × 6) which are available to search for an increase over baseline due to experimental treatment. Three of these 288 cells scored no change from the maximum at which they began.

Wysocki and Preti suggest that we score these 3 cells as if they were an increase over baseline. However, it is irrational to assign these 3 cells as an increase because there was no increase. In fact, inspection of the data of these men reveal no increase in *any* of the 10 cells of sociosexual behaviors recorded (5 sociosexual behaviors for 2 subjects); and those behaviors that did not remain stable actually decreased (e.g., sexual intercourse decreased for both men in the experimental phase). As appropriate to this double blind placebo controlled study, the definition of "increase over baseline" was applied equally to all cells of both groups. We did not score stable or declining averages as an increase.

In their examination of the sexual intercourse data from the placebo group (4th paragraph), they again did not read the data in Table II accurately. Wysocki and Preti state: "In the placebo group there were no individuals among the 10 whose baseline scores were zero who had a single intercourse during the treatment phase." Subject 48, in the placebo group, was clearly such as individual. His 8 weeks of intercourse data shown in Table II were 0 0 0 1 0 1 1 0: meaning two baseline weeks of "0," and one intercourse each, in weeks 4, 6, and 7.

We employed strict, rather than lax, criteria for scoring a cell as an increase in behavior by setting two criteria: (i) higher weekly average and (ii) at least one score exceeding the highest baseline score when mathematically possible. Wysocki and Preti are incorrect in stating "this requirement effectively insured that two men in the pheromone group would be included as showing an increase over pheromone for sleeping next to a partner." We can see no subject for whom this second criterion would have classified his cell as an increase in Table III (or II). We fail to understand the source of the writers' inaccurate reading of the data since they did not provide an example.

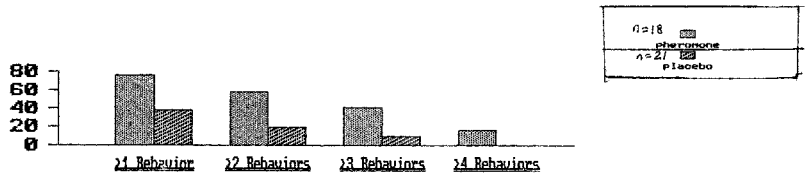

Fig. 1.

Wysocki and Preti state: "It is notable that the frequency of intercourse for many men in both groups was zero during the 2 baseline weeks." We agree and suspect that this was the reason the men chose "to enroll in a study designed to increase the romance in their lives."

On the Robustness of the Finding in This Sample

A detailed inspection of Table IIA actually reveals the robust nature of these data. Table IIA highlights 4 columns of data derived from the information already provided to illustrate the consistency and stability of the findings we have presented. The sequential records within each group are rearranged in order to reveal the cohesive nature of the pheromone's effect.

Table IIA shows that twice as many pheromone as placebo subjects recorded an increase in at least one sociosexual behavior (76 vs. 38%). Three times as many pheromone as placebo subjects recorded an increase in at least two behaviors. Four times as many pheromone as placebo users recorded an increase in at least three sociosexual behaviors. And, an "infinitely" higher proportion of pheromone than placebo users showed an increase in four or more behaviors because the placebo group had no (0) men who met this criterion. Figure 1 displays these data.

We conclude that pheromones not only produced increases in men's attractiveness to women but that the data are robust. As we stated in the publication, these initial findings need to be replicated.

Re: The Nonobviousness of the Formula

While not identified as such in the publication, Wysocki and Preti are correct that the formula tested was *Athena Pheromone 10X*^{tm} which is

Table IIA. Consistency Patterns in Behavioral Response

Subject	Status	Gain ≥1	Gain ≥2	Gain ≥3	Gain ≥4	Sexual intercourse	Sleeping	Affection	Informal dates	Formal dates
						Pheromone				
S06	M	x	x	x	x	22223232	71767777	75777777	33000000	00002000
S10	ND	x	x	x	x	00000001	00012001	23453345	22210221	01003001
S47	ND	x	x	x	x	00000001	00000001	00001011	00010001	00001011
S16	ND	x	x	x		00000010	00000010	10100111	00100010	10000101
S21	ND	x	x	x		00000000	00001000	01021010	00010110	01011000
S45	ND	x	x	x		01111231	30121221	00122221	43001001	00120010
S46	ND	x	x	x		00000000	00000000	00000001	00100111	00001001
S34	ND	x	x			00000001	00000000	00000000	00000000	00001000
S09	M	x	x			00011000	77576774	66045445	00000000	00000000
S39	KSC	x	x			42445445	66445445	64475223	00001002	66544443
S42	M	x				32233202	57655243	11111101	11021000	00010031
S02	M	x				31111122	75774777	33213312	00000000	00000000
S08	KSC	x				00000000	00000000	76777774	10112011	23301322
S07	M					21120221	76777774	00000000	00000000	00000000
S14	D					00000000	00000000	00000000	00000000	00000000
S36	D					00000000	10000000	10000000	00000000	10000000
S51	M					00000000	67263476	12000000	00000000	00000000
Totals										
n		13	10	7	3	8	6	7	6	7
%		76	58	41	16.7	47	35.3	41.2	35.3	41.2
						Placebo				
S33	ND	x	x	x		00000000	00000000	00000111	00001101	00000010
S48	ND	x	x	x		00010110	01100112	04242522	11020110	12113323
S18	D	x	x			22423434	25433535	06323534	12100001	10000503
S41	D	x	x			00000000	00000000	00002300	00000000	11123301
S13	D	x				11011000	00001000	23222241	20020121	33213120
S17	ND	x				01010000	00000000	01010010	01010000	00000010
S29	ND	x				00000000	00000000	00000000	00000000	00000001
S30	ND	x				01000000	00000000	01200000	00000100	01000010
S03	ND					00000000	00000000	00000000	00000000	00100000
S05	D					00000000	00000000	20000000	01111110	32120222
S11	M					24114410	77777777	77777777	00000000	00000000
S15	KSC					00000000	00000000	41411140	00000000	00000000
S20	D					22001102	34222343	54223303	22000000	32302030
S23	D					04200000	04000000	04000000	00000000	00000000
S24	ND					00000000	00000000	00000000	00000000	11000110
S25	ND					00000000	00000000	00000000	00000000	00000000
S27	M					12101011	77777777	13202202	00000000	00000000
S32	D					10100100	10000000	21211300	00000000	32421310
S35	ND					00000000	00000000	00000000	00000000	00000000
S38	D					20212210	20101110	414232221	20222221	21202110
S40	M					01010101	77777575	11010101	00000000	00000000
Totals										
n		8	4	2	0	2	1	3	2	7
%		38	19	9.5	0	9.5	4.8	14.3	9.5	33.3

marketed in support of Athena Institute for Women's Wellness, Inc. Wysocki and Preti opine (5th paragraph) that the proprietary formula is not obvious based on prior published work. Dr. Cutler agrees and notes that nonobviousness is a requirement for her pending patent application on the formula. She created this formula independently but derived it from their previous collaborative work through 1987.

Winnifred Cutler, Ph.D.
Athena Institute for Women's Wellness Research
Chester Springs, Pennsylvania 19425

Erika Friedmann, Ph.D.
Department of Health and Nutrition Sciences
Brooklyn College
Brooklyn, New York 11210

Norma L. McCoy, Ph.D.
Department of Psychology
San Francisco State University
San Francisco, California 94132

REFERENCE

Vogt, W. P. (1993). *Dictionary of Statistics and Methodology,* Sage, Newbury Beach, CA, p. 69.

INTERSEXUALITY: RECOMMENDATIONS FOR MANAGEMENT

To the Editor:

It is good to have long-term follow-up reports on the treatment and management of intersex conditions. Indeed, such papers as the recent study by Slijper *et al.* (1998) are needed to amass a collection of cases from which directions for future treatment can be extracted.

Unfortunately the paper is less helpful than it might be. First it incorrectly states my thinking and recommendations for dealing with the treatment of intersexed children. Second, it incorrectly reports some of my

Support for this research was provided by the Eugene Garfield Foundation of Philadelphia, Pennsylvania, and The Queen's Medical Center of Honolulu, Hawai'i.

findings. Third, it lacks clarity of presentation so one cannot be sure of the children's management nor treatment outcome. And fourth, the methods and discussion sections omit important considerations for the interpretation of their findings.

Slijper *et al.* (1998) state "Diamond (1996) is of the opinion that sex assignment and genital surgery should be delayed until the child can decide for itself. This means the child should neither be raised as a boy nor as a girl, but as an intersex person" (p. 142). Regrettably that is not my opinion and I have clearly written otherwise.

First, the reference to which they refer with their mistaken statement of my ideas does not exist. The paper to which they probably refer should read *Journal of Sex & Marital Therapy* rather than *Journal of Sex Research.* In that particular 1996 paper what I do say regarding intersexed infants is:

1. Management should not be decided solely on the size and nature of the phallus (p. 164).

2. "Postpone any cosmetic clitoral surgery until the individuals can themselves understand the situation well enough to participate in the decisions" (p. 165).

3. "Concern is only regarding cosmetic surgery. I have no hesitation about surgery for medically threatening reasons" (p. 166).

4. "When possible, the children have a say in any cosmetic surgery and absolutely [when possible] be involved in any sex reassignment" (p. 166).

Indeed, Keith Sigmundson and I (Diamond and Sigmundson, 1997a) explicitly say: "In rearing, parents must be consistent in seeing their child as either a boy or girl; not neuter. In our society intersex is a designation of medical fact but not yet a commonly accepted social designation" (p. 1047). We further indicate to which sex we think assignment of persons with different conditions would offer the best potential outcome. Such assignment does not necessarily follow the chromosomal sex or genital configuration.

The findings they misrepresent come from our paper documenting the John/Joan case (Diamond and Sigmundson, 1997b). Slijper *et al.* write: "Although raised as a girl, the patient . . . as an adult asked for sex reassignment" (p. 126). We had stated clearly that: "at age 14 years, Joan [an XY male that had been reassigned as a female] decided to switch to living as a male" (p. 300). This occurred on his own, against advice and despite intensive professional counseling by Money and others and parenting to have Joan accept life as a female.

This Slijper paper is confusing or misleading in other ways. For instance in Table I the major categories are listed according to medical diagnosis but in Table III the major categories are given in terms of genital

appearance. This makes comparing outcome difficult. Further many of the categories are compounded in discussion. This obscures a clear interpretation of findings. It would be better if each case were listed under a major category with the genital appearance, sex of assignment, treatment, and outcome given so the reader could better understand what occurred and judge for him/herself as to the significance of the findings.

Some 20% of the cases offered by Slijper *et al.* involve those with the complete androgen insensitivity syndrome (CAIS). With XY chromosomes and a female genital appearance without ambiguity, such persons are certainly intersexed. Since the work of the last dozen years or so, however, there is almost never any doubt as to assignment as female and the CAIS person seeing herself as female when adult. Slijper *et al.* say as much (p. 126). Thus, in a paper considering sex assignment for intersexed individuals with ambiguous genitalia this population would be better discussed separately. The subject population of 59 would therefore, for consideration, be reduced by these 12. The same can be said of the two individuals with Leydig cell hypoplasia. The baseline number for XY individuals whose sex of assignment might be in doubt as females then becomes, as a maximum, 40 (59 − 5 [those assigned as male] − 12 − 2 = 40). It is also questionable if the two individuals spoken of as "still too young to have their gender role behavior evaluated" (p. 137) should be considered since treatment outcome for them is far from knowable. The baseline would then be 38 rather than 59. Such considerations significantly change the outcome percentages.

It is also notable that the work of Slijper *et al.* does not consider XXY individuals since they are among the more common intersexed conditions. Were they not seen among their "10 years' work with children exhibiting a physical intersex condition" (p. 127)? More than a few of these individuals with Klinefelter's syndrome, although assigned as males, later switch to living as females and/or exhibit androphilic orientation.

Table III indicates that 7 individuals developed a gender identity disorder (GID) and 12 individuals developed a deviant gender role (DGR). Elsewhere we learn that "Deviant gender role behavior was not only exhibited by the girls with a gender identity disorder, it was noted in 25 (46%) of the total group of girls" (p. 137). And later read "Although 87% of the girls with a physical intersex condition developed in line with the assigned sex, 13% developed a GID, but only one (2%) failed to accept the assigned sex" (p. 142). How are these figures to be reconciled?

Understanding of patient management would also have benefitted from some additional information. Intersex categories are known for having degrees of manifestation. For instance, CAH phenotypes can range from individuals having minor clitoral hypertrophy alone to persons having an extensive penile phallus with accompanying labial fusion to form a scrotal

sac. The accompanying display of male-like behaviors can be great or limited for either extreme. The designation of partial androgen insensitivity syndrome (PAIS) can range from a designation of 1 (complete male-appearing) to 5 or 6 on a scale of 1 to 7 (complete female-appearing) (Quigley *et al.,* 1995). Because Slijper *et al.* have categorized the patients by etiology rather than phenotype, the reader has no way of knowing the degree of ambiguity involved in an individual case. This is crucial information needed to manage the child and predict outcome.

A interesting finding reported by Slijper *et al.* is that GID in their group become manifest at 3-5 years of age. This makes sense since children during preschool years certainly become aware if they disagree with their sex assignment (Diamond, 1997). It is also reported that psychopathology also developed as late as 27 years of age. Nowhere are we told, however, how old the individuals were at evaluation. Some were, I assume, still teenagers. Perhaps GID will be manifest later. Many intersexed (and transsexual) individuals don't change gender until in their 30s or after. Much depends on the options the individuals can consider and how much help is given by therapists, physicians or others. As one example, I recently reported on a hermaphroditic individual who, at the age of 28, transitioned from female to male only after a sympathetic counselor suggested this as a viable possibility (Diamond, 1997). The switch was then immediate and satisfying. Other physicians or therapists to whom the individual previously went for help refused to discuss sex change as an option and he himself didn't realize that reassignment could occur. Sex change can and does occur at any age. And sex reassignment can at any age be successful if done at the individual's behest rather than being imposed.

It is not clear at what age each of the subjects was questioned regarding genital surgery or sex assignment. It is reported "From the age of 4, the children were able to express their own opinions . . . about the length of the clitoris or its erectile function" (p. 133). While it is appropriate to discuss such matters with children, I do not believe it appropriate to base surgical decisions relative to the future value of a child's genitals on the impressions of a 4- or even a 10-year-old. Such children usually have little or no concept of erotic masturbation, orgasm, or mutual genital experiences from which to judge what loss of their born-with-genitalia might entail (Diamond, 1996). Also, at these young ages children are most susceptible to parental and professional pressures. They cannot at those young ages give informed consent. They are not aware of what they can lose (Chase, 1996).

The authors attempt to answer why, despite early surgery, and psychiatric counseling to parents and patients, there was still such a high degree of psychopathology in their sample. Perhaps the intersexed indi-

viduals were manifesting disparities and conflicts they saw in their lives which were not recognized by their family or therapists. They might have felt they could not easily and acceptably express their true feelings either at home or at the clinic and psychopathological behavior was the result. Many intersexed persons have reported being denied the opportunity to fully declare their own desires or have them respected. Many have been denied knowledge of their own histories (see e.g., Diamond, 1997; Diamond and Sigmundson, 1997b). Thoughts of intersexed persons are often labeled as misguided, deviant, or even psychotic since they don't follow expectations of clear male–or-female thinking or behaviors. Too often it is only when the intersexed individual adheres to the stereotyped gender constructs of their clinicians that they are considered "normal." I suggest, instead of asking the intersexed individuals to conform to these criteria we expand the clinicians' ideas and understanding so that the intersexed person is seen as normal within a wider set of parameters.

Can it be assumed that Slijper and colleagues were ready to allow their intersexed patients to easily express disappointment with their life lot or sex assignment? Would any such admission result in further psychotherapy to reinforce the original sex assignment regardless of whether that is what the individual desired? Consider: "intensive psychotherapy" was applied to those children who could not easily accept their assignment as females (p. 136). As Slijper and colleagues state: "The aim of the team was to prevent the development of cross-gender identification in children born with a physical intersex condition, especially in neonates born with ambiguous genitalia" (p. 127). While that might seem an understandable and commendable goal I offer instead that the patient be allowed to redirect the goal of the therapy, if that is his or her wish, and then receive help in the new direction even if it entails gross-gender identification. Otherwise the treatment can be seen as intimidating and "brow-beating." With such treatment the child might eventually seem to "go along" and have been convinced, but actually be resentful and only be waiting for the opportunity to pursue his or her own directions. Such was the case, for instance, with John/Joan (Diamond and Sigmundson, 1997b).

The high incidence of psychopathology seen in Slijper *et al.*'s intersex population might, in part, be due to the lack of support for the individual desirous of sex reassignment or cross-gender identification. It is my recommendation to "Allow the child free expression as to choices . . . . Do not obfuscate; knowledge is power, enabling the patients to structure their lives accordingly." (Diamond and Sigmundson, 1997a, p. 1048). If the individual prefers a cross-gender identification, that possibility should be explored and supported. This, too, is also the thinking of the Intersex Society of North

America (ISNA, 1994). Admittedly, there are not yet enough cases reported in the literature where these suggestions have been followed to know if, in the long run, the percentage of those being happier adults will be any better than with the management offered by Slijper and colleagues. I do, however, think I see greater contentment in those I counsel or for whom I am consulted.

Slijper *et al.* (1998) indicated "the team policy was to correct the virilization of the external genitalia immediately after birth or as soon as possible after the diagnosis was made so as to avoid cross-gender identification" (p. 132). While this aids the individual who prefers life as a female it mitigates against those who might later prefer to be male. I recently reported on one such hermaphroditic individual whose enlarged clitoris was taken from her at 18 months of age (Diamond, 1997). This too might foster psychopathology rather than reduce its likelihood. Many individuals become aware of genital surgery and see it as a bodily insult rather than an aid. It is well to keep in mind the experimental work of Goy, Bercovitch, and McBrair (1988). They demonstrated that androgenized primates could show genital masculinization without behavioral masculinization and behavioral masculinization without genital changes. Slijper and colleagues (Slijper, 1984; Slijper *et al.,* 1992) have found similarly for humans. This again is reason to withhold surgery until it is clearly desired by the individual. The surgeon has no way of knowing to which gender intersexed individuals of many etiologies will aspire. Early surgery reduces the options available.

Early and nonconsensual surgery also imposes another set of risks. For many intersexed individuals it confirms for them, consciously or not, that their status at birth is monstrous and automatically in need of correction. Slijper *et al.* mention psychopathological dangers in regard to vaginal dilation (p. 133) It should be recognized to potentially hold for all other surgeries as well. And certainly not of small consideration, genital surgery can damage future sexual functioning (Chase, 1996).

Further, along these lines, it has also been shown that the appearance of genitals, either their own or that of their peers, in the typical child is not crucial for classification of gender until about the age of 9 (Goldman and Goldman, 1982). At least for English-speaking children, they may be aware of genital differences but usually do not understand they are significant for gender assignment.

It is not surprising that the groups showing the highest incidence of GID and DGR were those of XY karyotype that had been exposed to the highest androgen titers yet assigned as female. Individuals with a transverse penis or 17-KRD or external cloacae typically have difficulties with prenatal

dihydrotestosterone production needed for male genital development but sufficient prenatal testosterone production for masculinization of the nervous system fostering male identification (Imperato-McGinley *et al.,* 1979a, b; Rösler, 1992; Rösler and Kohn, 1983). With development and maturity their masculine behavioral biases become activated and genital masculinization advances. It is my recommendation to assign these individuals as males (Diamond and Sigmundson, 1997a).

Lastly, the reasons individuals maintain a sex of assignment, despite feeling to the contrary, are many and diverse. It does not mean they would not have it otherwise (Diamond and Sigmundson, 1997b). I believe that the most ethical and correct way to treat intersexed individuals has two main rules. The first is that management should be in light of the diagnosis, not in light of the individual's genitals. When diagnosis and genitals coincide, all to the better. When they don't the prognosis should govern. The second rule is that the rights and thinking of the mature intersexed individual should have priority and no cosmetic surgery be performed until that individual's voice is heard.

I hope that Slijper *et al.* and others continue to study and review the area of intersex and publish their work. My only caveat is they do so with concern for accuracy, clarity, and consideration of a wider range of possibilities than demonstrated in this particular paper.

Milton Diamond, Ph.D.
University of Hawai'i — Manoa
John A. Burns School of Medicine
Pacific Center for Sex & Society
Honolulu, HI 96822

REFERENCES

Chase, C. (1996). Re: Measurement of evoked potentials during feminizing genitoplasty: Techniques and applications (letter). *J. Urol.* 156: 1139-1140.

Diamond, M. (1996). Response: Considerations for sex assignment. *J. Sex Marital Ther.* 22: 161-174.

Diamond, M. (1997). Sexual identity and sexual orientation in children with traumatized or ambiguous genitalia. *J. Sex Res.* 34(2): 199-222.

Diamond, M., and Sigmundson, H. K. (1997a). Management of intersexuality: Guidelines for dealing with persons with ambiguous genitalia. *Arch. Pediat. Adolescent Med.* 151: 1046-1050.

Diamond, M., and Sigmundson, H. K. (1997b). Sex reassignment at birth: Long-term review and clinical implications. *Arch. Pediat. Adolescent Med.* 151: 298-304.

Goldman, R., and Goldman, J. (1982). Children's Sexual Thinking: A Comparative Study of Children Aged 5 to 15 Years in Australia, North America, Britain, and Sweden. Routledge & Kegan Paul, London.

Goy, R. W., Bercovitch, F. B., and McBrair, M. C. (1988). Behavioral masculinization is independent of genital masculinization in prenatally androgenized female rhesus macaques. *Horm. Behav.* 22: 552-571.

Imperato-McGinley, J., Peterson, R. E., Gautier, T., and Sturla, E. (1979a). Male pseudohermaphroditism secondary to 5 alpha-reductase deficiency — a model for the role of androgens in both the development of the male phenotype and the evolution of a male gender identity. *J. Steroid Biochem.* 11(1B): 637-645.

Imperato-McGinley, J., Peterson, R. E., Stoller, R., and Goodwin, W. E. (1979b). Male pseudohermaphroditism secondary to 17B-hydroxysteroid dehydrogenase deficiency: Gender role change with puberty. *J. Clin. Endrocrinol. Metab.* 49: 391.

Quigley, C., De Bellis, A., Merschke, K. B., El-Awady, M. K., Wilson, E. M., and French, F. S. (1995). Androgen receptor defects: Historical, clinical and molecular perspectives. *Endocrine Rev.* 16(3): 271-321.

Rösler, A. (1992). Steroid 17B-hydroxysteroid dehydrogenase deficiency in man: An inherited form of male pseudohermaphroditism. *J. Steroid Biochem. Mol. Biol.* 43: 989-1002.

Rösler, A., and Kohn, G. (1983). Male pseudohermaphroditism due to 17B-hydroxysteroid dehydrogenase deficiency: Studies on the natural history of the defect and effect of androgens on gender role. *J. Steroid Biochem.* 19(1): 663-674.

Slijper, F. (1984). Androgens and gender role behavior in girls with congenital adrenal hyperplasia (CAH). In De Vries, G. J., De Bruin, J. P. C., Uylings, H. B. M., and Corner, M. A. (eds.), Progress in Brain Research: Sex Differences in the Brain (Vol. 61). Elsevier Press, Amsterdam, pp. 417-422.

Slijper, F. M. E., Drop, S. L. S., Molenaar, J. C., and De Muink Keizer-Schrama, S. M. P. F. (1998). Long-term psychological evaluation of intersex children. *Arch. Sex. Behav.* 27: 125-144.

Slijper, F. M. E., van dr Kamp, H. J., Brandenburg, H., de Muinek Keizer-Schrama, S. M. P. F., Drop, S. L. S., and Molenaar, J. C. (1992). Evaluation of psychosexual development of young women with congenital adrenal hyperplasia. *J. Sex Educ. Ther.* 18: 200-207.