ence emotion. Thus, an interaction develops out of a shared as-if reality in which each player reacts to the other in terms of the defined situation. Asch (1952) would speak of a mutually shared field that provides the framework for interactions. Each player's actions and reactions within the rules of the as-if reality maintain and further the other players' involvement in the fantasy-as-reality, To the extent that the structures of the role-playing situation, the rules of the game, resemble those of the real situation being modeled, true discoveries can be made. To the extent that the goals, barriers, and means available to the players resemble those of the real situation, the behaviors and feelings of the role players will be similar to those of people in the natural environment. So could students discover that the policemen are sometimes afraid. They discovered it as a result of being fully involved in the as-if reality.

The situation they were involved in was not a mimicking of reality but a different *created* reality. In a sense, Zimbardo, Haney, Banks, and Jaffe (1973) did not simulate a real prison, but created a special kind of prison. The authoritarian structure in Zimbardo's prison depended on Zimbardo's authority. The guards may have been reacting to the implied demands of Zimbardo. So, too, do prison guards respond to the implied demands of their superiors.

Role playing can be a tool that goes beyond the prior stereotyped images and attitudes of the role players. This occurs when the created situation involves the players fully in the as-if reality, generating an ongoing interpersonal process governed by conditions similar to those of the real-life situation being modeled in goals, barriers, and available means. Just as a person can become totally involved in a novel, or in a hypnotist's description of events, so too can people become fully involved in a created situation, and behave and react to one another as human beings as naturally as under similar circumstances. This happens only when the players are as involved in the as-if reality as fully as a good hypnotic subject is involved in the reality created by the hypnotist.

Every laboratory scientist creates conditions in the laboratory that he believes mirror the variables that are most significant in the natural setting. Then he must theorize about the extent to which the same variables are at work in the natural setting. Often we forget that such generalizing is theorizing. Similarly, someone designs a role-playing situation. The rules of the game resemble to a greater or lesser extent the conditions of the natural setting. There are limits to what conditions will be imposed, just as there are limits to what can be done to people and animals in laboratory experimentation.

Still, role playing is a tool through which discoveries can be made. This particular tool employs a strange as-if reality that can be created through active involvement in imaginings and maintained through an interactive process in which each participant's actions express the common assumptions of this as-if reality. As in all experimentation, theoretical considerations will establish the limits of generalization.

REFERENCE NOTE

1. Doyle, *C. Violence explored via role-playing in the classroom situation.* Paper presented at a meeting of the American Psychological Association, San Francisco, September *1968.*

REFERENCES

Asch, *S. Social psychology.* Englewood Cliffs, N.J.: Prentice-Hall, *1952.*

Banuazizi, A., & Movahedi, S. Interpersonal dynamics in a simulated prison: A methodological analysis. *American Psychologist, 1975, 30, 152160.*

Hilgard, J. *R. Personality and hypno*sis, Chicago, Ill.: University of Chicago Press, *1970.*

Orne, M. T. On the social psychology *of* the experiment: With particular reference to demand characteristics and their implications. *American Psychologist, 1962, 17, 776-783.*

Spanos, N. P., & Barber, T. X. Toward a convergence in hypnosis research. *American Psychologist, 19'14, 29, 500511.*

Zimbardo, P. G., Haney, C., Banks, W. C., & Jaffe, D. A Pirandellian prison. The mind is a formidable jailor. *New York Times Magazine, April 8, 1973, pp.38-60.*

CHARLOTTE L. DOYLE
Sarah Lawrence College

Another Look at Banuazizi and Movahedi's Analysis of the Stanford Prison Experiment

Ali Banuazizi and Siamak Movahedi (February 1975) presented a highly critical review of the now famous Stanford Prison Experiment conducted by Zimbardo and his associates (Haney, Banks, & Zimbardo, 1973). I do think a few general comments from someone not directly involved in that study are appropriate at this time.

Put simply, Banuazizi and Movahedi argued that the distressing behavior of the subjects in the prison study was not due to their response to a "psychologically compelling prison environment," as Haney et al. wished us to believe. Rather, their behavior was best viewed as a response to powerful demand characteristics in the experimental situation itself (Orne, 1962; Rosenthal, 1969). Not only was it possible that the experimenters successfully communicated their expectations to the subjects, but, more importantly, it seemed the subjects were "acting out their stereotypic images of a prison guard and, to a lesser extent, of a prisoner" (Banuazizi & Movahedi, 1975, p. 159). Ever since the "great awakening" produced by Orne and Rosenthal, such charges have been frequently leveled against social psychology *research (e.g., Page,* 1974; Schuck & Pisor, 1974). It is a serious charge. I do believe, however, that Banuazizi and Movahedi are incorrect in their assessment of the prison study.

Was the passivity, depression, helplessness, and even psychological

dysfunction displayed by the prisoners only a remarkable performance offered in response to perceived experimental demand? It is not always clear why critics think such contamination would necessarily lead to confirmation of an experimental hypothesis, but the question must be answered.

First, it should be pointed out that the questionnaire data collected by Banuazizi and Movahedi show there is no single stereotype of prisoner behavior. No consensus emerged when their subjects were asked to predict how others would behave in the role of "prisoner." Only one third of the respondents predicted prisoners would be passive or docile, while another third anticipated continual rebellion and defiance. Banuazizi and Movahedi are certainly aware of this fact, but seem to have overlooked it when devising their alternative explanation of the prison study results.

Second, the authors correctly pointed out in the first part of their article that Haney et al. did not have clearly defined hypotheses when they embarked upon their study. As Zimbardo (1973) has emphasized elsewhere, had the experimenters actually expected the severe apathy and psychological dysfunction eventually produced in some of the prisoners, the study would never have been run. Again, Banuazizi and Movahedi seem to have forgotten their earlier discussion of this point by the time their own conclusions are explicated. For the prisoners, at least, it seems unlikely that specific expectations could have been communicated to them by the experimenters. Banuazizi and Movahedi have themselves undermined this accusation.

The temporal flow of events during the 5-day prison study also seems to contradict Banuazizi and Movahedi's interpretation of the prisoners' behavior. If subjects were indeed "acting out" culturally defined roles, would it not be reasonable for them to show this in the first day of the study? I suggest that the violence and rebellion of the prisoners which occurred less than 2 days after the initiation of the experiment do reflect those subjects' stereotype of real prisoner behavior. Sporadic bursts of prison violence were frequently reported in the press even before Attica, reinforcing the impression that prisoners react to dehumanization and abuse of power with counterforce. The passivity and despondency of the prisoners in the study only occurred after the guards violently put down the rebellion using fire extinguishers, transformed the prisoners' rights into "privileges," played the prisoners against each other, and instigated systematic harassment of the prisoners. Does this mean that the prisoners subsequently learned what their "role" should be? In a sense it does, but it means much more than play acting, for although severe emotional disturbance might be easily simulated, psychosomatic rashes are not. It must be remembered, furthermore, that after the first prisoner subject was released, the experimenters did, in fact, become suspicious that his symptoms were faked (Zimbardo, Haney, Banks, & Jaffe, 1972). To me, at least, this suggests that the symptoms shown by the prisoners released in later days must have been serious, indeed, to convince the experimenters to release them.

If the prisoners' behavior cannot be dismissed as role playing, it is still conceivable that the "oppressive brutality" of the guards was only the acting out of a stereotype, and not a real response to the prison environment per se. What is the evidence on this second question?

First, the questionnaire data collected by Banuazizi and Movahedi do suggest that a single stereotype of guard behavior is widely held. Almost 90°0 of the respondents believed that other persons in the role of "guard" would be oppressive and hostile. This supports Banuazizi and Movahedi's interpretation of the guards' behavior during the actual prison study. Second, it is possible that the experimenters did, in fact, communicate specific expectations to all of the subjects concerning the guards' behavior. Before the subjects were randomly assigned to the role of guard or prisoner, all subjects were warned that "those assigned to the prisoner role should expect to be under surveillance, to be harassed, and to have some of their basic rights curtailed during their imprisonment, but not to be physically abused" (Haney et al., 1973). Thus, it is possible that the guards felt they had clear guidelines from both their cultural observations and the experimental instructions about how they should maintain order in the mock prison. Of course, it should be remembered that physical abuse did occur in spite of the experimenters' explicit instructions.

Haney et a1. (1973) were aware that some might try to "explain away" the guards' behavior by attributing their conduct to good playacting. What those authors sought to demonstrate, however, was that the behavior of the guards far exceeded what would be required of them were they merely responding to experimental demands. Although the evidence they offer in support of this contention might not sway Banuazizi and Movahedi, I find it persuasive: Harassment of the prisoners seemed to be greater when individual guards were alone with solitary prisoners, or out of range of recording equipment. Guards escalated their aggression against the prisoners even after the prisoners stopped resisting and complied with the commands issued to them. Some guards indicated a willingness to work extra shifts without pay (or is that, too, part of being a "good roleplayer"?). Unfortunately, Haney et al. did not report hard data to support their argument, so we must rely on their observational powers (and their integrity).

More importantly, we must ask ourselves what it means to say the guards were only "acting out" a common stereotype of how real guards treat real prisoners. In what sense is this different from saying the subjects were actually responding to a "psychologically

compelling prison environment," as Haney et al. have wished to argue? I would suggest, in answer to this, that guards in a real penitentiary could also be said to be "playing a role" when they first begin their jobs. Like the guards in the prison study, real guards are given little or no training. Beyond what they have seen in films or heard from the more experienced guards, they really do not know what to expect. Just like the guards in the prison study, they only know that they must be "tough" with the prisoners in order to maintain order. In what way, then, is the subsequent behavior of the guards in the study any less "real" than the behavior of novice guards in actual penitentiaries? Certainly, a qualitative difference does not exist. Like real guards, the prison study guards began slowly but, once disinhibited, soon freely aggressed against the prisoners as a matter of routine.

I recognize that a difference of opinion on the interpretation of the Stanford Prison Experiment will persist, in spite of the arguments I have made. However, I have hoped to show that the findings of the prison study should not be discarded out of hand. Banuazizi and Movahedi have raised important issues, but I argue that they have not presented a strong case in support of their critical stance. Although the study is not an "experiment" in the more formal sense, it remains a powerful demonstration which should be taken seriously. I am sure there are others who will agree with me.

REFERENCES

Banuazizi, A., & Movahedi, S. Interpersonal dynamics in a simulated prison: A methodological analysis. *American Psychologist, 1995, 30, 152160.*

Haney, C., Banks, W. C., & Zimbardo, P. G. Interpersonal dynamics in a simulated prison. *International Journal of Criminology grad Penology, 1973, 1, 69-9>.*

Orne, M. T. On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist, 1962, 17, 776-783.*

Page, M. M. Demand characteristics and the classical conditioning of attitudes experiment. *Journal of Personality and Social Psychology, 1974, 30, 468-476*

Rosenthal, R. Interpersonal expectations: Effects of the experimenter's hypothesis. In R. Rosenthal & R. L. Rosnow (Eds.), *Artifact in behavioral research.* New York: .Academic Press, *1969.*

Schuck, J., & Pisor, K. Evaluating an aggression experiment by the use of simulating subjects. *Journal of Personality and Social Psychology, 1974, 29,181-186.*

Zimbardo, P. G. On the ethics of intervention in human psychological research: With special reference to the Stanford prison experiment. *Cognition: International Journal of Cognitive Psychology, 1973, 2, 243-256.*

Zimbardo, P. G., Haney, C., Banks, W. C., & Jaffe, D. *Stanford prison experiment.* Stanford, Calif.: Philip G. Zimbardo, Inc., *1972.* (Tape recording)

WILLIAM DEJONG
Stanford University

Demand Characteristics Are Everywhere (Anyway)

A Comment on the Stanford Prison Experiment

In their recent criticism of the well-known Stanford Prison Experiment conducted by Zimbardo and his associates (Haney, Banks, & Zimbardo, 1973), Banuazizi and Movahedi (February 1975) contended that the prison experiment tapped only roleplaying dispositions rather than any situationally elicited "strategic, coping responses to an asymmetrical power situation analogous to that of a real prison" (p. 159). Thus, Banuazizi and Movahedi attributed the oppressive and tormenting behavior of the student-"guards" and the passive and depressed behavior of the student-"prisoners," as observed in the Stanford Prison Experiment, to the demand characteristics inherent in the experimental role-playing situation.

As evidence for their methodological challenge they offered the results of a questionnaire administered to 185 college students from the Boston, Massachusetts, area which asked for predictions about how college students in general would behave if placed in the role of guard or prisoner in such an experiment. Additionally, respondents were asked about how they thought they, personally, would behave in the two experimental roles. Their results showed fairly strong consensus (roughly 90% agreement) that the students who played the role of guard would behave oppressively, aggressively, hostilely, etc. In contrast, predictions about student-prisoner behaviors were more variable and fell roughly into three different categories of (a) rebellious, defiant, etc. (48%); lb) passive, docile (47%)); and (c) fluctuating, depending upon the behavior of the guards (46%). Banuazizi and Movahedi attributed the lack of consensus in predicting student-prisoner behavior to "the more diffuse nature of the popular *conceptions of* the role of prisoner" (p. 158). As for how the student-respondents felt that they personally would behave in the role of guard in a prison experiment, less than half predicted that they themselves would act aggressively or viciously toward the prisoners (47.1%), and only 14% predicted that they would act aggressively or rebelliously in the prisoner role.

Based on their questionnaire results, Banuazizi and Movahedi proposed an alternative interpretation of the outcome of the Stanford Prison Experiment. They wrote that

> In our view, the subjects responded to a number of demand characteristics in the experimental situation, acting out their stereotypic images of a prison guard and, to a lesser extent, of a prisoner. (p. 159)

They emphasized that the stereotyped role expectations that subjects bring with them into an experimental situation are in fact "mental sets" that *dispose* the subjects to act out role-defined behavior, in this case, that of guards and prisoners. Furthermore, they contended that such dispositional mental sets influence the actual observed behavior even

more strongly "when a concordance exists between these images of the subjects and the demand characteristics of the experiment" (p. 159). They concluded that role playing, as an experimental strategy, is an inappropriate technique for testing situational versus dispositional hypotheses.

We find this a rather weak challenge to the results of the Stanford Prison Experiment. To this point, one could ask whether there is any difference between the dispositions or "mental sets" college research participants might bring to a simulated prison study and the dispositions and mental sets real-life novice guards and first-time prisoners might bring to a real prison situation, particularly so if these dispositions are functionally equivalent to highly stereotyped and emotionally laden role expectations, which Banuazizi and Movahedi assert they are. Surely, if culturally conditioned dispositions and mental sets exist for guard and prisoner role behaviors, these roles should, by definition, be shared by most members of the population, whether they are college students or novice guards and prisoners. Obviously it is possible for different segments of the population to hold different predisposing mental sets (or role expectations)-a possibility liable, of course, to empirical verification. However, to the extent that such mental sets are in fact widely shared, it may be just these dispositions along with an accompanying system of situational and social supports that are responsible for the kinds of behavior demonstrated by the participants in simulated *and* real situations, where some participants have been assigned the role of powerless, criminal penitents and others the role of powerful, moral monitors of their deviant charges.

In a word, to dismiss the results of the Stanford Prison Experiment as merely the consequence of demand characteristics is to ignore the equally real demand

REFERENCES

Banuazizi, A., & Movahedi, S. Inter American Psychologist, 1975, 30, 152160.

Haney, C., Banks, W., & Zimbardo, P. personal dynamics in a simulated prison: A methodological analysis. Interpersonal dynamics in a simulated prison. *International Journal of Criminology and Penology,* 1973, 1, 69-97

STEPHEN THAYER
City College of the City University of New York

CAROLYN SAARNI
New York University

Movahedi and Banuazizi Reply:

We are pleased to see that our methodological analysis of the Stanford Prison Experiment (Banuazizi & Movahedi, February 1975) has raised some controversy, and we appreciate this opportunity to extend and clarify our argument. A pointby-point response to our critics' comments cannot be undertaken here because, in addition to the limitation of space, some of their points are not germane to their own arguments or to ours. We shall reply to Doyle (1975) first, and then comment on DeJong's (1975) and Thayer and Saarni's (1975) critiques, as the latter two raise a number of similar issues.

Doyle's primary criticism of our analysis is that we failed to "take into account the specificity and sequence of interaction in role playing." As an illustration for her argument, she presents a study in which five of her "gentle middle-class" female students, playing the role of the police, used brooms to prevent six classmates, who had been assigned to the role of demonstrators, from taking over the "Pentagon" (functionally represented in the experiment by a television set). Pointing to the intense involvement of her subjects in their respective roles, she concludes that true discoveries can involved in the "as-if reality" (as goad hypnotic subjects) and that the structure of the role-playing situation resembles that of the real situation being modeled.

It is true that we did not specifically refer to interaction sequences among the prisoners, intendent, and warden in the Stanford Prison Experiment. The notion of interaction, however, was certainly implied in our analysis, because without it our argument could have been reduced to the assertion that the behavior of the subjects in the experiment was no more than a series of automatic or rehearsed responses *20* a prompter. We do, nonetheless, seem to understand the notion of interaction rather differently from Doyle. Her concept of interaction, which is shared by many social psychologists, treats it as something emergent or transcendental, similar to the metaphysical notion of entelechy of the classical biologists. From our standpoint, interaction is not an emergent phenomenon but an integral part of all social processes. As such it is necessarily implied in any analysis of social behavior as a primitive, first-order construct.

Furthermore, we find it difficult to disagree with Doyle's main conclusion that "true discoveries" can be made in a well-simulated situation, provided that the role players are involved in the as-if reality as good hypnotic subjects. The statement represents, first, a modest and somewhat vague claim. Many things can be qualified as "true discoveries," particularly if one employs the term, as does Doyle, as a synonym for "understanding" in a pedagogical context. Role playing, as her study seems to show, may in fact be a useful pedagogical tool. But, more importantly, the question of discovery is logically quite different from that of justification. The statement that role playing under certain conditions can lead to "discoveries" is an empirical

grounds. In our analysis, we were not concerned with the question of discovery, but with the problem of what inferences could be made from data produced by role playing.

Doyle's contention that "role players are . . . involved in the as-if reality . . . as a good hypnotic subject" is neither an inherent characteristic of every role-playing situation nor can it be established by fiat. Such a claim, much like the "reality transformation" hypothesis of Zimbardo and his associates (Haney, Banks, & Zimbardo, 1993), should be established independently in every single instance by empirical evidence. The same is true of the claim of isomorphism between the structure of a role-playing situation and the real situation being modeled. Doyle, as many experimentalists in the social sciences, seems to treat such a claim definitionally. Often, phenomena such as "role," "status," and "power" are simulated in the laboratory or in role-playing situations, and it is assumed that they are, by definition, functionally equivalent to their counterparts in the real world. To establish such functional equivalence, however, one must demonstrate empirically that the two systems, the contrived and the real, contain similar relevant variables and that the same process laws are at work in both. Thus, it is not enough, as Doyle contends, for the investigator to "theorize" or speculate about isomorphism or the extent to which the same variables are at work in the natural setting.

A review of DeJong's comments indicates that he takes issue primarily with our interpretation of the prisoners' behavior in the Stanford Prison Experiment, and not so much with our interpretation of the behavior of the guards. Thus, he admits that "the violence and rebellion of the prisoners which occurred less than 2 days after the initiation of the experiment do pretation, DeJong does not offer more than a trivial argument with which we have already dealt in our original article. While acknowledging that "Haney et al. did not report hard data to support their argument," DeJong insists that we should instead "rely on their observational powers (and their integrity)." Such criteria for the evaluation of scientific claims are not, of course, methodologically compelling.

DeJong maintains that a role-playing interpretation does not adequately explain the *reported* behavior of the prisoner subjects. But so do we maintain this. For the same reason, we played down such an explanation with respect to this group of subjects. A variety of other confounding variables or conditionsnot fully described in a brief report of the experiment-could of course have provided the basis for other explanations. For example, we proceeded on the premise that the prisoner subjects could have obtained their release at any time by simply telling the experimenter that they no longer wished to remain as subjects in the experiment. Now, if this right had not been properly communicated to the subjects, the added frustration resulting from "real" imprisonment could provide a further clue for a fuller understanding of their so-called "pathological" reactions. Furthermore, our failure to adequately account for the psychosomatic rash of one of the prisoners, as suggested by DeJong, could hardly be construed as support for Haney et al.'s claim of having successfully simulated a prison environment. Psychosomatic rashes can occur in situations and for reasons that have very little to do with the social forces that operate in a stable prison structure.

DeJong's final argument contains the core of his, as well as Thayer and Saarni's, misunderstanding of our position. It represents, at the same time, a more general trend in sense is the proposition that "the guards were only `acting out' a common stereotype of how real guards treat real prisoners . . . different from saying the subjects were actually responding to a `psychologically compelling prison environment' "? The answer to this question depends entirely on how one chooses to interpret the expression "psychologically compelling prison environment." One can define this expression in such a way, of course, as to make a tautology out of the question, but that would not promote our understanding of the problem. The structural components of a real prison include not only its formal organization but a complex web of informal organizations that lie behind the facade of the formal one. The informal organizations determine and shape the implicit, but perhaps more significant, power structure of the institution, the inmates' system of stratification and leadership, the contingencies of reward and punishment, the mechanisms for the maintenance of order, and so forth. Explanations of the behavior of prisoners and guards-their responses to the psychologically compelling prison environment-should then be made with reference to these structures and processes. Viewed in such terms, there is a fundamental difference between saying that guards were simply "acting out their common stereotypes" and saying that the subjects were responding to a "psychologically compelling prison environment." Therefore, to argue that the behavior of real guards is the same as acting out common stereotypes, as have DeJong and Thayer and Saarni, is to advance a version of the dispositional hypothesis.

Continuing in his critique, DeJong asks, "In what way, then, is the subsequent behavior of the guards in the study any less `real' than the behavior of novice guards in actual penitentiaries?" The use of

are unreal, and hence even strict role playing is in no sense "unreal." Similarly, the behavior of guards in a role-playing study and the behavior of novice guards in an actual penitentiary are equally real. However, to posit that two behaviors, both of which are real, are equivalent responses to similar social psychological forces, is to commit the fallacy of assuming the identity of subjects based on the identity of their predicates.

REFERENCES

Banuazizi, A., & Movahedi, S. Interpersonal dynamics in a simulated prison: A methodological analysis. *American Psychologist,* 1995, 30, 152160.

DeJong, W. Another look at Banuazizi and Movahedi's analysis of the Stanford prison experiment. *American Psychologist,* 1995, 30, 1013-1015.

Doyle, C. L. Interpersonal dynamics in role playing. *American Psychologist,* 1975, 30, 1011-1013.

Haney, C., Banks, W. C., & Zimbardo, P. G. Interpersonal dynamics in a simulated prison. *International JourrlaL of Criminology and Penology,* 1973, 1, 69-97.

Thayer, S., & Saarni, C. Demand characteristics are everywhere (anyway) A comment on the Stanford prison experiment. *American Psychologist,* 1975, 30, 1015-1016.

SIAMAK MOVAHEDI
Department of Sociology University of Massachusetts, Boston

ALI BANUAZIZI
Boston College

The Ancestry of the Surrogate Mother

In discussing the problem of originality in science, Boring (1927) said:

> Certainly the historian is impressed by the fact that almost never does an idea seem entirely new. If it is a great idea that has helped to make a name or a date great, he looks for its previous occurrences. Generally he finds them. Not always can he be sure that the early instances actually fathered the great emergence, but often, when he is not sure of the fact of inheritance, he is also not sure of its absence. The bases of one's own present thought are often indeterminate; how much more difficult to form a judgement of the thought of another ten or a hundred years ago ! Thus the historian may come to suspect every case of apparent originality. If he realizes, however, the tedious process by which thought develops, and by which novelty, after flitting about for years in indirect vision, is finally fixed for a brief space in the fovea of science (new thoughts do not occur; they hardly emerge even; they evolve), then he becomes aware of the grounds for his suspicion. (p. 71)

Boring supported his thesis with several now well-known (by virtue of his subsequent books) examples from the history of psychology and physiology: the specific energies of nerves, the law of spinal nerve roots, localization of function in the brain, hypnotism, the context theory of meaning.

Another case in point has recently emerged. An unsigned article in the *American Psychologist* describes the awarding of the American Psychological Foundation's Gold Medal Award for 1973 to Harry F. Harlow. This well-deserved honor is based, in part, on Harlow's considerable contributions to developmental psychology and primatology, including the use of the surrogate mother technique. The unidentified author states: "A flash of insight led to the invention of the surrogate mother in 1957, and to the series of experiments on contact comfort" (American Psychological Foundation, 1974, p. 49).

The "flash of insight" was more richly described by Harlow, Harlow, and Suomi (1971), in words that are surely Harlow's:

> Many creative ideas have suddenly appeared in a flight of fancy, but the surrogate mother concept appeared during the course of a fancy flight. The cloth surrogate mother was literally born, or perhaps we should say baptized, in 1957 in the belly of a Boeing stratocruiser high over Detroit during a Northwest Airlines champagne flight. Whether or not this was an immaculate conception, it certainly was a virginal birth. The senior author turned to look out the window and saw the cloth surrogate mother sitting in the seat beside him with all her bold and barren charms. (p.539)

I suggest that this fruitful event was neither an immaculate conception nor a virginal birth. Harlow may well have been inseminated by some ideas read long ago and subsequently forgotten.

Alfred Russell Wallace, more than a century before the celebrated champagne flight, described his efforts to rear an infant Mias or orangutan whose mother had been shot. Observing the tendency of the infant to clutch various substances, including Wallace's beard and the newly washed and brushed hair of its own back, Wallace (1856) devised a kind of ladder to which the infant would cling for a quarter of an hour:

> It would hang for some time by two hands only, and then suddenly leaving go with one would cross it to the opposite shoulder to catch hold of its own hair, and thinking no doubt that that would support it much better than the stick, would leave hold with the other hand and come tumbling down on to the floor, when it would immediately cross its arms and lie quite contented, for it never seemed hurt by any of its numerous tumbles. I then tried to make a kind of artificial mother for the little creature by wrapping up a piece of buffalo-skin into a bundle with the long wooly hair outside, and hung it up about a foot from the ground. This suited it much better, as it could sprawl its legs and arms about wherever it liked, and always find some hair to catch hold of, which its little fingers grasped with the greatest tenacity. (pp. 388-389)

Wallace (1869) included a partially rewritten version of this episode, still containing the phrase "artificial mother," in his book *The Malay Archipelago.*

Although Harlow may well not have seen the 1856 or 1869 accounts of an artificial mother, it is probable that he was familiar with *The Great Apes,* by Robert M. and Ada W. Yerkes (1929). In that classic work, Wallace's book version is quoted at some length and reference is made to the earlier journal article. In a later work on chimpanzees, Yerkes (1943) said: