The critique that van Poucke makes of sociobiology in general and my approach to it in particular repeats a set of standard arguments, some ideological and polemical, some substantive and worthy of extensive discussion, so that the areas of agreement and disagreement can be clearly defined, and the disagreements resolved from empirical evidence. In a brief reply I can only hope to dispose of the ideological polemics, and to make a few general pointers to the resolution of bona fide misunderstandings and disagreements.

First, the polemics. They are to be found in the title of van Poucke's article ("Het neo-sociaal darwinisme: een theorie over mensen zonder menselijke kenmerken"), and in his accusation of sexism (pp. 126-127). Sociobiology is a direct application to behavior of the neo-Darwinian synthesis of evolutionary theory and modern genetics. Social Darwinism, on the other hand, is a totally discredited doctrine of Herbert Spencer in Britain and William Graham Sumner in the United States. Their theories, based as they were on loose analogies of societies as "superorganisms" and on group selection, had an immense impact on both early 20th century social science, and indirectly on social policy, but they never had any resonance in the mainstream of biological thinking, and they are emphatically rejected by practically all contemporary biologists as a complete misunderstanding of evolutionary theory. To call sociobiology "neo-social Darwinism" is thus either a monumental piece of ignorance about intellectual history, or a cheap semantic trick to serve polemical ends. Sociobiology is neo-Darwinian, not neo-social Darwinian.
As for sociobiology being "a theory about humans bereft of human characteristics", van Poucke merely repeats the cute aphorism that human ethology is the study of people as if they could not talk. By now, many of the leading theorists in sociobiology (Alexander, 1971; Barash, 1979; Dawkins, 1976; Trivers, 1971; Wilson, 1978) have written extensively about humans, and none of them takes the position fallaciously ascribed to them, namely that they dismiss the unique features of our species, including symbolic language and culture. Even some of the more honest (but adamant) critics of sociobiology (Harris, 1979) are beginning to concede that this charge is false, and that the area of disagreement between sociobiologists and social scientists is much narrower and much less categorical than the latter initially assumed out of ignorance of evolutionary biology.

As for sociobiology being sexist and male-biased, this polemical assertion hinges on the definition of "sexism". If it is sexist to say that males and females play complementary but asymmetrical roles in reproduction, and, therefore, differ in reproductive and parental behavior, then it is not only sociobiology which is sexist, but the vast bulk of the biomass of this planet, which reproduces sexually. However, van Poucke's specific charge that sociobiology sees men as active competitors and women as passive instruments is complete nonsense. It is true that the sexual asymmetry in parental investment makes females (of our species as of countless others) a scarce resource for males rather than vice versa, and that therefore, there is more male than female competition for mates (Trivers, 1972). But females also compete with each other for the best mates, and with males for a multiplicity of resources. Females, too, are manipulative and capable of treating other humans as objects. There is absolutely nothing in the sociobiological paradigm to support a simplistic view of males as active and females as passive; but there are systematic and predictable differences in which males and females maximize fitness, and these differences are explainable in terms of the biology of sexual reproduction.
So much, I hope, for the polemics. Now, let us turn to the substance of van Poucke's criticisms, some of which are interesting and deserve a reply. I shall concentrate on three of van Poucke's assertions:

1. That symbolic communication, that is, language (whether spoken or written) is the basis of human sociality and reciprocity.
2. That there seems to be an inconsistency in my suggestion that coercion in human societies came sequentially after reciprocity as a mechanism of sociality since reciprocity seems much less well documented than coercion in other animal societies.
3. That human society and culture are not reducible to individual action, and that they have emergent properties.

1. "Zonder een gedeeld universum van betekenissen is menselijk sociaal leven ondenkbaar" writes van Poucke (p. 125). I quite agree. Indeed, I make precisely the same assertion myself on p. 202 of my article: "Omdat de mens (...) het enige dier op deze planeet is dat spontaan, onvermijdelijk en algemeen gebruik maakt van een ingewikkeld communicatiesysteem bestaande uit symbolen met willekeurige betekenis(...)". Recognition of the importance of symbolic language in transmission of information, and hence in adapting to environmental conditions does not in any way threaten the sociobiological paradigm. At the broad level of evolutionary theory, however, that statement is a low-level description of a particular adaptive mechanism of a single species. It is a statement of the same low order of theoretical importance as: "Without the shared universe of information-sharing communicated in the honeybee dance, honeybee social life is unthinkable".

Neither van Poucke nor I are honeybees, so the latter statement does seem trivial to us both because it affects our fitness only trivially (e.g., through the presence of a honeypot on our breakfast table). Human language seems colossally important to us both, not because of its intrinsic theoretical import, but because it drastically affects our fitness. Without language, we would
be treated as catatonic schizophrenics in some lunatic asylum instead of being securely ensconced academics exchanging complex symbols in the pages of this journal, something which, by all appearances, we both seem to enjoy enormously.

It is one of the hallmarks of science, however, to be able to distinguish the theoretical from the existential import of a proposition. Symbolic language is tremendously important to us as humans, and no sociobiologist will deny that we must take it into account to understand human behavior. But, a. not all human behavior is verbal, b. the relationship between verbal and nonverbal behavior is far from simple and direct, and, c. the simple utilitarian paradigm of fitness maximization explains more verbal and nonverbal behavior more parsimoniously than any competing paradigm.

One more point concerning human speech: it seems to be a relatively recent development compared to the other specifically human features of our organism. That is, for at least three of four million years, hominid species have existed without anything we would recognize as human speech. Speech is probably not over a quarter of a million years old. As for written language, on which van Poucke puts so much stress, he better not make that a defining characteristic of humanity because it was invented only some 6000 years ago, and because well over 90 per cent of humanity managed quite well without it until about a century ago. Writing co-evolved with the rise of large states, and was principally developed as an instrument of coercion by emerging state bureaucracies (for record keeping purposes in censuses, tax collection and the transmission of orders). The benign view of writing as the liberator of the toiling masses is singularly out of touch with historical reality, though in keeping with the Panglossian view of the world shared by so many academics.

2. On the role of reciprocity and coercion in human affairs, van Poucke and I are in basic agreement, and sociobiology is fully compatible with much of what social scientists have long been saying. Where we differ is that van Poucke suggests that
language is the basis of human exchange systems whereas I consider language an important facilitating mechanism for reciprocal exchange. Silent trade, for example, shows that exchange relationships can take place without language. The analogy which van Poucke draws between language and exchange, namely that language is an exchange of symbols in which we "trust" others to reciprocate with mutually agreed upon symbols confuses the means and the ends. This is easy to do, especially for an academic, because in our daily life speech itself can become the reward. All the same, speech is usually a means to an end. As for the "trust" involved in speech behavior, we agree to understand each other so long as it is mutually beneficial to do so, but, otherwise, we manipulate symbols in the service of interests (to wit, the present exchange).

If van Poucke's dictum (p. 124) that "het prototipo van wederkerig gedrag lijkt mij communicatie" is accepted, then reciprocity is well established in many animal societies. Indeed it is completely wrong to state as does van Poucke that communication is "altijd gemedieerd door symbolen" (p. 124). There is plenty of communication that is transmitted through signs, not symbols; this is true not only of non-human animals, but of humans as well. When a baby cries, when embarrassment makes us blush, when we tremble in fright, we communicate information through signs, not symbols. All social animals have a communication system, almost by definition, since there can be no genuine interaction without communication, nor sociality without interaction. If communication is the basis of reciprocity, then reciprocity is not a human monopoly. Even in the more narrow sense of conscious expectation of return, I suspect good field and experimental studies will establish reciprocity (as distinguished from kin selection) in primates, and perhaps other intelligent mammals, like social carnivores and cetaceans.

This brings us to van Poucke's argument about the evolutionary sequence in which coercion and reciprocity appeared in humans (pp. 125-126). There he simply misreads me. Coercion, especially based
on sheer size differences (which, in turn, are correlated with age and sex), is common in many animal societies, including early hominids. So I said in the article van Poucke criticizes. The issue is that the human level of organized, premeditated group coercion is unparalleled in any other species, and is a recent development of the last 10,000 years or so, linked with the domestication of plants and animals, then of small states, then of large, bureaucratic literate states. The enormous growth of coercion (and hence of intraspecific and intrasocietal parasitism) in recent human history has no remote counterpart in other animal societies. Interspecific "slavery" is common in ants, but systematic intraspecific and intrasocietal exploitation of labor to enhance the fitness of the few at the expense of the many through social organization of the means of violence, the technology of communication (including writing), and so on, is a human monopoly, and indeed a recent and ever escalating human achievement. All of this I made quite clear in the original article (pp. 216-218).

3. Finally, we come to the problem of emergent properties of human culture and society, and to the question of reductionism. On rereading my article I can see that here I have not made my position as clear and as explicit as I should have, and I take this opportunity to expand on these points.

To advocate a strategy of reductionism is not to deny a priori the existence of emergent properties. What I meant to suggest was a hierarchy of levels in the organization of matter, such as that the higher levels include all of the lower levels, plus some emergent principles specific to the level in question. Thus, much of the structure of living matter is reducible to biochemistry and biophysics, although life has some irreducible properties of its own, such as reproduction. Still, a biology divorced of chemistry is unthinkable, and has become increasingly unthinkable over the years.

Every new scientific discipline dealing with a
specific level of organization of matter has initially stated as its *raison d'etre* the irreducibility of its subject matter to the principles of the discipline specializing in the lower level of organization. Chemistry has stood in that position vis à vis physics, biology vis à vis chemistry, psychology vis à vis biology, sociology vis à vis psychology. Yet, nearly all the scientific advances have been in the reductionist direction, i.e. in the direction of showing the reducibility of much that was hitherto thought irreducible, to simpler and more fundamental processes. Claims to irreducibility and emergence have steadily been whittled down as understanding of the physical world improved.

The whole development of biochemistry and biophysics in relation to biology is a case in point. This is not to say that genes and chromosomes, for example, are not a radically different level in the organization of matter than complex inorganic molecules. For a long time Mendelian genetics made spectacular progress without much reference to biochemistry. The great breakthrough of the discovery of DNA-RNA, however, reduced much of genetics to biochemistry, and the geneticist of today must be a sophisticated biochemist as well.

I am suggesting that the social sciences will inevitably undergo the same process of gradual integration into biology, a specialized branch of biology, to be sure, dealing with more complex levels of organization, but all the same an integral part of the corpus of the natural sciences. We are now witnessing a desperate attempt to draw the line against reductionism at the human psyche and culture, and to assert ever more shrilly as the desperation mounts, "le matérialisme ne passera pas". But there is every indication that it will pass, in fact, that it already has heavily invaded the social sciences. Just in the last 15 years since the neo-Darwinian approach to the study of behavior has begun to crystallize as a paradigm, it swept the field of animal behavior, incorporating older traditions such as ethology and behaviorism, and challenged squarely the social sciences.
In the last five or six years since social scientists have acknowledged the nature of the challenge, reactions are already moving from heated, impassioned, ideological rejection, to serious, though still skeptical attention. In another five years, I predict, the contributions of population and behavior genetics, ecosystem theory and, more generally, behavioral biology to an understanding of human behavior will be routinely accepted and incorporated into anthropology and sociology textbooks; what yesterday was greeted with invective, and today with guarded skepticism, will tomorrow be shrugged off with a "what-else-is-new?"

The threat of sociobiology to the social sciences is much more imaginary than real, for there will always be a demand for specialists who know enough about human behavior in general, and specific human cultures and societies in particular, to apply intelligently the insights of sociobiology to our own species. But the room for mystification and reification in the study of human behavior will be sharply reduced.

Thus, when van Poucke says (p. 128): "Een maatschappij, of beter, de cultuur die zij produceert, overleeft haar leden", he is using the word "overleeft" metaphorically. A society or a culture does not live, except in a loose analogical sense based on the old social Darwinist reification of the society-as-an-organism. Life is self-reproducing matter. A society or a culture is principally non-material, and even its material products do not reproduce. The Mona Lisa will never mate with the Blue Boy to produce little Gainsvincis.

We have thus come full circle. The real social Darwinist is not I, but van Poucke and his fellow social scientists who would have us believe that societies and cultures live because they are like organisms; that they mysteriously survive the mere carnal vectors that "carry" them; that they obey to elusive laws that completely transcend the actions of these carnal vectors; that they exude conscience collective, etc. Durkheim thought it clever to pronounce God a symbolic representation of society. The joke is on him, however. As a good
atheist, Durkheim made society his god. If social scientists must believe in God, all well and good. If they must make "society" and "culture" their représentations collectives, who is to stop them? I merely want to suggest that science is not built on reifications and mystifications.

"Society" is a short-hand term to mean a group of interacting conspecifics. "Language" means a set of signs and/or symbols used by organisms to pass on information about their environment. "Symbol" means an agreed-upon meaning between two or more organisms. "Culture" is socially transmitted learned behavior. "Population" means individuals who inter-breed. "Power" is one or more organisms forcing other organisms to do things they would rather not do. "Science" is an information retrieval system about the environment shared by individuals. Norms, values, beliefs, laws, ideologies, all have meaning only in reference to the behavior of people. Even the most material of cultural artifacts, say a stone axe, would cease to exist as a cultural artifact if it were not recognized by a human being as something made by another human being.

Now, all these complicated things that we do as human, singly and collectively, and that we categorize under all these reifying labels do have important consequences for our evolution as biological organisms, indeed for the evolution of the entire planet. Obviously, our "culture" and our "technology" enable us to track environmental changes and to adapt much faster and efficiently than if we had to rely exclusively on natural selection "acting" on random mutations. The cultural component of our evolution make us change not only faster than the genetic component thereof; culture change is also fundamentally different because it is Lamarckian rather than Darwinian.

No sociobiologist advocates ignoring any of the special characteristics of our species. Sociobiology is merely an evolutionary paradigm which states that observable behavior is the product of a complex interplay of genotype and environment. Culture of man (nee, Mijnheer van Poucke, "mens", niet "man") is an important part of the environment,
the man-made part of it. But culture grows out of genetic evolution and continues to interact with it, even though it has important autocatalytic properties of its own. The independence of culture from genes and from the physical and biotic environment is only partial. Human behavior can only be understood in the total context in which it evolved. And all we do and produce is reducible, in the last analysis, to the behavior of individuals. No piece of culture can survive without at least one individual "carrier". The wooden mask discarded after a religious ceremony becomes food for worms, unless it starts a second career on a living room wall a continent away, in which case, it once again becomes a cultural artifact even though its meaning has changed drastically. Without the individual, there is no culture.

The philosophical message of sociobiology is thus, it seems to me, highly humanistic and individualistic. It is only people like Mr. van Poucke who would have us believe that Culture, Society, the State, loom over us like superorganisms, and that we are only the messenger boys (and girls) of Kulturgeschichte. Of us two, who, pray, is the Social Darwinist?

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