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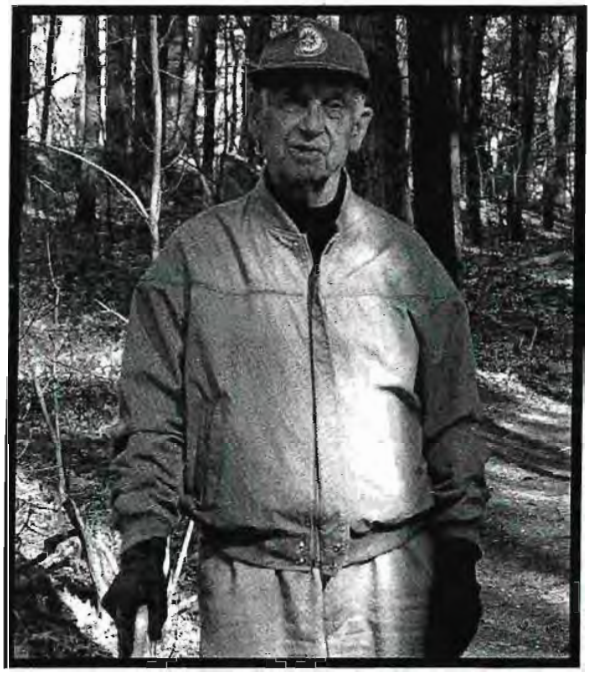

INTERVIEW OF ERNST MAYR

By Bill Charlesworth
P.O. Box 18
Stockholm, WI 54769 US

It is widely agreed that if there were a Nobel Prize for Biology, Ernst Mayr would win it hands down. His achievements are about three standard deviations above the mean--over 691 journal articles, sole author of 12 books, 16 honorary degrees, 39 special awards (an unprecedented triple of the top awards in biology--the Balzan Prize, the Japan International Prize for Biology, and most recently the Crafoord Prize from the Royal Swedish Academy), and at least 53 Honorary Society Memberships and Offices.

Born in Germany in 1904, Ernst Mayr got his Ph.D. in zoology from the University of Berlin in 1926, spent over two years on expeditions to various parts of New Guinea (collecting birds for the Rothschild Museum and the Solomon islands, served as Curator for the American Museumof Natural History in New York and as Director of the Museum of Comparative Zoology at Harvard, and was the Agassiz Professor of Zoology at Harvard beginning in 1953 until "retirement" in 1975. Currently, he is working on three books, carrying on an extensive worldwide correspondence with colleagues and admirers, as well as gracefully enduring numerous interviews.

The following is an excerpt from a day-and-a-half interview (including walks in nearby woods) in mid-April this year.

BC: Ernst, your class on Darwin at Minnesota overwhelmed many of us by its clarity, comprehensiveness, and scholarship. What was most informative was your emphasis upon defining and understanding fundamental concepts in biology and how they fit with each other. Will you tell us about these fundamental concepts and their significance?

EM: The most important concept in biology--a very basic one without which you can't have any discussion about anything in biology--has to do with the distinction between typological thinking and populational thinking. If you think typologically [categorizing natural phenomena in terms of ideal types] you can never understand natural selection or, for example, courtship displays. The moment you

think populationally [viewing natural phenomena in terms of individuals and populations] everything in biology makes perfectly good sense. Of course, the concepts have to fit with each other, otherwise if they were in conflict with each other they wouldn't make any sense.

We can start with functional biology. I am not particularly well-qualified to discuss functional biology. My interest from the very beginning as a young naturalist was in evolutionary biology--in contrast to functional biology. There is a big difference.

BC: As you say in your latest book, This is Biology, keeping this contrast continually in mind is very important.

EM: Exactly. Too many people have not made the distinction between these two kinds off biology. In functional biology you deal with proximate causations and proximate explanations of such causations. Functional biologists are in close contact with the physical sciences- particularly chemistry. In evolutionary biology, however, you're dealing with entirely different problems. You're looking for ultimate causations, ultimate explanations. This involves a completely different method.

In functional biology, you can do a great deal with experiments. In evolutionary biology, usually you cannot do experiments. You can't do experiments on the extinction of dinosaurs. You use an entirely different technique: you use a "historical narrative". You develop a scenario that represents a probable explanation of what you're after. Then you test this scenario with all sorts of means. You have to be constantly testing.

As for the extinction of dinosaurs, there were a number of explanations but they didn't make much sense. Then when the Alvarez hypothesis was developed --the impact of an asteroid-- from that point on everything that was studied and investigated fit perfectly into the scenario. For example, there had to be some trace of the asteroid impact--traces of quartzite and other things--around the shift from the Cretaceous to

the Tertiary. Everything that was subsequently found and investigated confirmed the historical narrative of the Alvarez impact.

BC: In your book, Toward a New Philosophy of Biology, you mentioned that biology can make a significant contribution to understanding the three aspects of causality that were singled out by Nagel--(1) explanation (2) prediction, and (3) teleology. Is it important for us to consider what is meant by these three elements?

EM: The first, yes, but the second and third no. The physical scientists--that's what Nagel was-- were particularly confused about teleology. They always liked to explain something by saying "Well, that's the teleologic aspect of the universe." I wrote several papers demonstrating that the word "teleological" was applied by philosophers and theoreticians to four different natural phenomena. I have expanded that number recently to a fifth one. Four of these five can be explained in a very mechanistic, materialistic way--no conflict at all with the laws of chemistry and physics. For the fifth one, cosmic teleology, which Kant and people like Nagel were flirting with, there is simply no evidence. There is no built-in development of a life force that leads the world to a betterment...to perfection...to progress. There is simply no mechanism to do that.

We can provide mechanisitic explanations for four of the uses of the word "teleological" --the first, to any process that leads to an end as a result of natural laws. Take dropping an object--the object moves by the force of gravity until it hits the floor. This is an instance of the operation of a natural law. I call this a teleomatic process.

The second is teleonomic and that is anything that is controlled by a program--during embryogenesis, for example, genes produce the right kind of proteins that eventually produce a body. The same with all [social] displays of organisms. They are purely mechanical and will be explained by genes and how they produce the proteins in the body. We can provide a completely mechanistic explanation. Again, nothing supernatural, nothing metaphysical. Strictly mechanistic, materialistic--that's teleonomic.

The third is what Kant referred to as "zweckmassig" [purposeful, expedient] He said incorrectly that the eye has the purpose to see, the heart has the purpose to push the blood through the vascular system. Well, that is nonsense. A heart does not have a purpose; only a thinking organism can have a purpose.

Up to now I think purposeful activity has only been recorded in warm-blooded organisms--mammals and birds--none in lower animals. Birds and mammals have what we call really intelligent behavior. The only way to account for the eye and heart is to see them as adaptive structures, the result of natural selection. There is nothing teleological in all of this.

As for cosmic teleology, it simply does not exist. And so the sum total of my investigation of the concept of teleology is that it refers to a mixture of a great number of things.

BC: What about the Nagel's concept of "prediction"?

EM: Now when it comes to prediction, in physics there is almost total prediction. Today, however, modern philosophers restrict this in terms of stochastic processes.

When it comes to evolution, prediction in most cases is almost totally impossible. Who could have predicted at the beginning of the Cretaceous that the dinosaurs would become extinct?

When it comes to speciation, no one can predict which of the many isolated populations would become successful species. Predictions in evolutionary biology are very limited.

BC: What about the Grant's work on the Galapagos finches?

EM: Well, you can make some predictions about the fate of populations of Galapagos finches. But let us remember that the Grants worked on an extremely small island with a very small population, and you get all the difficulties of working with very small populations. For

example, one species went extinct. Well, if you had a large island this wouldn't happen. This is a very important point.

Jared Diamond and I have been working for about 30 years on a book on the speciation and ecology of birds in northern Melanesia, that is the two island groups of the Bismarcks and of the Solomon Islands. The Grants deal with small islands, we deal with big islands. Many of the things described by the Grants do not fit the big islands. So we should not say that what the Grants described, which is quite legitimate and very solid work, is the last word on the topic. We are dealing here with biology where we always have trouble generalizing from one set of facts to another set of facts.

BC: What about the concept of natural selection?

EM: I have emphasized two points--the first, is that it is a population phenomenon and you will never understand it unless you realize that selection deals with individuals--some are favored, some are not. Typologists will never understand natural selection.

The second thing, that I have added only in recent years, is that natural selection is a two step process. The first is the production of variability; the second is the winnowing out of this variability, culling away all the inferior individuals until you have the material that produces the next generation.

Such questions as, "How can you say that pure chance can make such a perfect organ as the eye?" shows that the guy doesn't understand the nature of the two steps. He only talks about the first step--the production of variability which is a chance phenomenon. But in the second step you have a very definite deterministic process.

I am one of the first to realize that natural selection is not a truly selective process at all. It is an elimination process. eliminating in each generation all the less fit, less successful individuals, not selecting the best ones.

BC: Individual organisms?

EM: That's right.

BC: I ask this because I want to talk later about levels of selection.

EM: I might say in parenthesis there are two other things [besides individuals] that are targets of selection. They are almost totally ignored in evolutionary biology-- gametes (which egg, which sperm) are the target of selection. And the third one is...and this is where Williams was all wrong, the social group that has internal cohesion and cooperation. A social group, as a whole, has selective value.

Take hominid hunter-gatherer groups. Their success depends on how group members cooperate with each other. If you had a group that was disharmonious it would go down like nothing at all. Groups that have individuals who believe that "I'll do what is best for the group, not just for myself."--these are the succeeders.

That is the basis of human ethics! And that is overlooked by those who say that the origin of human ethics cannot be explained by natural selection.

As I emphasize in my last book, the social group is also a target of selection, not only individuals.

BC: What is the distinction, then, between gene and individual selection?

EM: The gene itself is not the target of selection. The carrier of the gene is the individual and the individual is the target of selection. That is important because-- Dobzhansky is one of the earliest persons to show it--the same gene in different genotypes can have different selective values. Therefore to say that the gene is the target is misleading. The carrier of the gene is the target. There is no such thing as gene selection.

BC: What about Dawkins and those who talk about gene selection?

EM: Right now in the human species natural selection deals with practices that have very

little to do with our status as human beings. Natural selection weeds out disease genes and weaknesses in inherited constitution, but there is no evidence whatsoever that natural selection gives any premium to higher intelligence--in fact there is evidence opposed to this. Highly intelligent people have, on the average, fewer children than people with low intelligence.

BC: But this was not the case in the human past.

EM: Absolutely not. [back then, selection for intelligence was at a premium]. Oh, yes, even today in the primitive tribes in New Guinea. I spent time in the mountains, I'd always found in every little group or village a few people who had more than one wife and a few who had no wives at all. Just by talking with them, my impression was that those who had more than one wife were the outstanding people in their tribe. The village idiots, to put it in extreme terms, had no wives.

BC: But wasn't there selection for the bright ones?

EM: No. You haven't gotten it yet. Natural selection is not for. Selection is an elimination process.

BC: The concepts of adaptation and natural selection are often viewed as involving very different processes that may conflict with one another. What is your view on this?

EM: There is no conflict between them. You can not have adaptation without selection--I do not know any adaptation that is not due to selection. Some biologists (mostly Marxist) are opposed to adaptation. I don't understand this. It doesn't make sense to me. The famous spandrel paper of Lewontin and Gould just makes no sense.

Of course, not everything in an organism is an adaptation. You know the story of the horses getting higher teeth when they shifted from browsing to grazing because the silica crystals in the grass abraded the teeth very fast so there was a premium on having high teeth. The latest paper shows that afterwards there was a period of high humidity and horses shifted back to browsing which, after all, provided more nourishing food than grasses.

Now comes the big thing. After the return to browsing, natural selection did not affect the high teeth, did not pare them down again. Selection does not necessarily destroy organs that become useless just because they may produce a certain cost to the individual.

I see no conflict between adaptation and natural selection. The Marxists make this mistake I think because they are reductionists--in this case at least. They only look at individual structures and behaviors. While, for me, the individual as a whole is the target of selection. A particular adaptive structure only makes a contribution to the survival of the individual; it only adds to the probability of survival. It isn't an all-or-none business.

BC: One of your most significant conceptual contributions to the field, in my mind at least, has been the concept of "open and closed programs". Would you comment on this?

EM: Konrad Lorenz and Heinroth discovered this concept but they never fully understood the importance of it. A closed program means you have a fixed genetic trait. Whenever you have a particular gene, then you will show a particular behavior. [No particular environmental input is necessary.]

Now the open program, in contrast to the closed program, involves a very different process. Imprinting, for example, is a learning process based on an open program which requires a particular kind of object in the animal's environment. The animal needs the object to learn how to behave in an adaptive way.

BC: Like ducklings quickly "learning" to follow their mother who is usually the only one available who will protect them?

EM: Yes. Also, keep in mind that the open program is very important for ethics. Children are [genetically programmed] to be very anxious to have an ethical education, but parents have to be around to give it to them. Waddington has written the best account of this in his book, The

Ethical Animal. This is probably the main trouble with the lack of morals in big cities where there is no education in ethics--other than the peer group which usually teaches the wrong things. I've always said what we need is more ethical education and we need to start at a very early age--as early as two years, and constantly thereafter reinforcing ethical principles. Remember, both open and closed programs are innate. [They operate differently, however, in their interaction with the environment].

BC: Let us turn to the concept of altruism. It has been a hot topic for some time now.

EM: Well, altruism has created a problem owing to the idea, first of all, that only the gene is the target of selection. And therefore only selfish tendencies could possibly be selected for. Now the minute you adopt my idea that the social group is one of the targets of selection, then altruism can be a target of selection because the welfare of the social group depends on the harmonious interaction of the members of the group and their cooperative behavior. For this reason altruism is very definitely a target or goal of selection because it helps the survival of the social group.

I have some other ideas on altruism. The definition of altruism in the [conventional scientific] literature is usually doing something for someone else at your own disadvantage. That is a very narrow definition because very often you do things for others without any noticeable disadvantage to yourself. Having such an extreme definition has made it very difficult, then, to come up with an explanation of "altruistic" behavior that could be of selective advantage.

BC: What about reciprocal altruism?

EM: I think that is the wrong term. There isn't too much altruism in it because there is not too much disadvantage involved which according to their own definition [is necessary]. Of course, reciprocal altruism is a very important part of our nature [human behavior], but it isn't altruism. It is to your advantage to be helpful. It should be called reciprocal helpfulness.

BC: I have two questions on the founder effect.

EM: To begin with, the founder effect was not invented by me. Bernhard Rensch had already had it in his writings. But Rensch never appreciated its real importance. If you have epistatic interactions among genes in a small population it will produce (sometimes) unusual phenotypes (the founder population helps getting away from the norm). So you may get some unusual combinations of genes which may or may not be of use. The founder population may fall by the wayside.

BC: What about applying the founder effect to human evolution?

EM: Right now the human species doesn't have it any more because we have mass societies. But Eskimos most likely were started by some founder population.

BC: What about the role of behavior in evolution?

EM: Most of [species'] specializations started as behavioral changes. The person that said this before me was Wolfgang Wickler. In one of his papers he said that behavior is the pacemaker (Schrittmacher) of evolution. In other words, all the major niche occupations are started behaviorally and then the organism gradually acquires the necessary genes to make this [these occupations] more perfect. Baldwin had previously noticed this.

BC: I would like to get back to open and closed programs again. Will you address the environment conditions associated with their evolution?

EM: The trouble with the [early] ethologists was that they concentrated on very special species-specific behaviors, particularly courtship, and they didn't pay enough attention to other behaviors like feeding behavior [which can be very flexible behavior].

BC: In other words, adapting to social environments requires rigid unlearned behaviors--for example, automatic recognition of and behavior toward mates. Potential mate

behavior doesn't change. However, adapting to the physical environment requires flexible behavior: food sites and environmental cues change. In response to such cues, particular behaviors have to be learned and unlearned.

EM: [nods]

BC: What about punctuated equilibrium? Does it pose any real threat to evolutionary theory?

EM: You don't know the story behind punctuated equilibrium. That theory was proposed by me in 1954 in considerable detail when I explained the gaps in the fossil record. Actually, I explained the whole theory then. The only thing I didn't include (because I am not a paleontologist) is that when a species finally gets through the bottleneck and has been formed, it then may go on and on successfully millions and millions of years. That was added by Eldredge and Gould.

The major problem then was that Gould decided to make the theory very different from the Darwinian theory and made it into a saltation theory; he even said Goldschmidt [who claimed evolution involved macromutations--"hopeful monsters"] was right. But when Gould realized that that didn't get him anywhere--you can't reintroduce essentialist thinking back into Darwinian biology. Then he gave that up and went back to populational thinking...as I already had done in my 1954 paper.

BC: Are there any major remaining problems for evolutionary theory?

EM: Usually, these problems deal with properties of complex systems. One is the details of the development of the embryo from fertilized egg up to the adult stage. Some of the individual processes are understood, but in many stages of development you have a very complex interaction between several genes, control genes, and other genes. These complex interaction processes are by no means understood.

There are all kinds of genes we know about today. For fifty years DNA has been the dominating feature of that kind of evolution. But, now in my opinion proteins will come back.

BC: Are there any areas where evolutionary theory is vulnerable?

EM: There are very few places where our theory is vulnerable. Let us not forget that when you come down to spelling out the theory, you will find that every point of it goes back to Darwin. Darwin's original theory was very robust. Sure, he didn't know much about variation, but he did know that variation was crucial for natural selection. Since then a great many of details of what variation involves has been filled in.

BC: Let's say something crazy like, we find a hominid in a geological stratum of 20 million years.

EM: You never will. You never will find a hominid there. You see the point is for the last fifty years one author after another has come out with with a book in which they tried to correct evolutionary theory. And all of them fell flat on their face. The number of attempts that have been made to come up with an alternative to the Darwinian explanation is very large, but all of them are inferior to the original Darwinian process.

I think that the basic evolutionary theory, as we now have it, is not going to be changed. But there are an awful lot of details to be filled in where we now have black boxes. This [filling in black boxes] is particularly true for functional biology, not evolutionary biology. Take, for example, the working of the brain. We know a great deal about the working of neurons. But a phenomenon like me remembering something that had happened 70 years ago--we are a very long way from explaining that.

BC: What about those who argue against chance not being able to account for such things as the brain or the eye?

EM: It's not all chance! These people forget that natural selection is a two-step process. The first step is the production of genetic variability where chance plays a major role. But it is selection which sorts out [the

variants] during the second step. This process is not chance; it is deterministic.

BC: One of my personal concerns right now for human ethology is that human ethologists are not doing enough naturalistic (field) observation. What are your ideas about the role of such observation in the biological sciences?

EM: It is very difficult for a person to acquire true evolutionary thinking unless that person has been a naturalist. I noticed this with many scientists that if they really didn't see what was going on in nature, they had a good deal of difficulty really in understanding it.

The important thing is that you have to be an observer. [That is] You have not only to look and see but you have to be able to see the things that no one else sees. That was one of the great things about Konrad Lorenz. He could look at a group of displaying ducks and find some details in their courtship that others, who also saw the ducks, never saw.

Did I ever tell you the story of Konrad Lorenz missing a class he was teaching at the University of Munster? This is a wonderful story. When he was staying in Buldern, he went by motor cycle to reach the university. One day he left at the usual time. After quite a while, his assistant called up Mrs. Lorenz and said that Dr. Lorenz had not appeared and asked what was the matter. She said he left at the usual time. He must have had a motor cycle accident. So the assistant said he'd jump on his bicycle and she should jump on hers and they would meet. She hadn't left the park when she saw the motorcycle lying on the side of the road and Lorenz lying flat on his belly with his field glasses studying some ducks. She said, "You've forgotten your class." He said, "Oh yes I did! But this was so interesting, I just couldn't miss it."

There's no doubt that Darwin was a wonderful observer. I recommend to you his report of his voyage on the Beagle. On every page, you will find some interesting observation--whether he was studying slavery in Brazil, the Indians in Patagonia, or a revolution in Peru, or whatever it was, he immediately saw all sorts of things.

Now comes the second point. These observations immediately raised questions in Darwin's mind. So that he didn't just take the observations as they were, but they posed a problem for him.

BC: What about doing research in a singles bar?

EM: People usually make observations in general. Before a person goes to a singles bar, he has observed males and females interacting and may have concluded tentatively that males are more aggressive. Once he has obtained this tentative answer--you can call it a conjecture like Popper does or a working hypothesis--then he has to test it, test it, and test it.

BC: Will you discuss your major contributions to biology?

EM: O.K. I think the major contribution that I made was that I came to the conclusion that all the others [at the time] who talked about biology tried to fit biology into the picture of science that was made by physics and mathematics. Then they just added some biological aspects to this picture.

This was a big mistake. My view of biology is much more radical. I say biology is simply a different science from physics. You just can't use those usual laws like those used by the Vienna School (Hempel, Nagel or Popper) or even Kuhn, and then give us these laws as a straight-jacket and put biology into it.

This approach doesn't work. You have to start from scratch. Starting from scratch, the first thing you encounter is that you are not dealing with laws; you're dealing with concepts. Every theory in biology is based on concepts. Another thing, biology is a science in which you have two sets of causations--physical laws and a genetic program. Every single action of every single living organism is controlled by both of these causations, something everybody missed before but already knew it, but never stressed it.

A number of other things are characteristic of biology--of course the time factor. In

evolutionary biology everything is on a time scale. In physics you don't have that. I have even said--and I believe it is true--that, basically, evolutionary biology, as a science, is closer to history and historiography than it is to physics.

If I want to be shocking, I would say that the border line between the so-called exact sciences and the humanities, social sciences (Geisteswissenschaften) coincide with the border line between functional biology and evolutionary biology.

I even sometimes go so far to shock my philosophical friends by saying that modern biology is basically an inductive science. Of course, you have to change your definition of induction. But you make observations and these observations lead you to conjectures which you then test. This is an inductive process.

Now, what is different from the induction of the philosophers is that five different biologists, all facing the same observation could theoretically come up with five different conjectures. It is not a deterministic induction. It is a heuristic induction.

BC: Thank you very much.

ISHE MEMBERSHIP RENEWAL

ISHE MEMBERS! It is past time to renew your membership for 1999 if you have not already done so. Membership is by calendar year, so dues are to be paid by the first of the year. **If the date on your mailing label is earlier than the current year, it is time to renew your membership.** Please report any errors, change of address, etc. to the Treasurer, BARB FULLER, see officer box for address Current dues and directions for payment are given on the last page. Allow four weeks for recording changes.

ANNOUNCEMENTS

Last Call for 2000 ISHE Directory

We are now finalizing our preparations of the new membership directory 2000. If you have not yet submitted your personal information, you should do so ASAP!

FIRST, your personal info should be sent via the internet. I will provide the form like the current email-form, which you can find at the old maillist-form-address:

http://evolution.anthro.univie.ac.at/maillist/form.html

Even if you have already used the email-form on the server, you need to use the new form on the same page again! If it is not possible for you to use the internet-form or email, please send your data via fax to Astrid Juette (+43-1-31336-788) (Full name (first, last name), title, postal address, tel, fax, working field (max 6 words), email, homepage)

SECOND, the directory will be permanently available via the internet. Contrary to the email list, which is now accessible for everyone on the internet and serves as a kind of advertisement for our society, the member directory will be accessible only for members via password which will be very simple and will be distributed with the autumn newsletter. The email list will probably be shortened to only the homepage and email, and the affiliation-info will be integrated into the new directory.

THIRD, you will have to check on the form (or indicate on your fax) if you want a printed version of the directory. This also saves money for printing and shipment and many of us will be faster with finding the list on the internet than finding the booklet anyhow. By the way I encourage you to send an email (fax) if your data changes during the following years (or is

printed incorrectly). This also means the internet-directory will be more recent than a printed one can be. Please send your info for this directory today! Take the time even if you think your data is still correct! In order to meet our publication date, deadline for submission of personal information is the 31th September 1999.

Also, Please note, that the name of our server will change with the end of this year to http://evolution.anthro.univie.ac.at/ishe.html

Astrid Juette, membership chair
(see address in officer box on following page)

The XVIII World Congress of the **International Political Science Association (IPSA)** will meet in Quebec City, August 1-6, 2000. This letter is a call for papers for the panels that will be allotted to Research Committee # 12, m"Biology and Politics." If you are interested in delivering a paper, please send an abstract of the proposed paper, with your name, address, institutional affiliation, e-mail (if you have e-mail) to either:

Dr. Albert Somit
Room 256, Lesar Law Building
Southern Illinois University
Carbondale, IL 62901

Dr. Steven A. Peterson
School of Public Affairs
Penn State Harrisburg
777. W. Harrisburg Pike
Middletown, PA 17037
Internet: sap12@psu.edu

In accordance with the IPSA deadline, could you please send your proposal to us by March 1st, 2000 at the latest (and preferably sooner). Alternatively, if you would like to serve as chair of a panel or discussant, please send that information to either Dr. Somit or Dr. Peterson at the above addresses.

Fondation Jean-Marie Delwart

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The Jean-Marie Delwart Foundation will award in 2000 a Prize for an original work or series of works, individual or collective, in the field of **Chemical Communication**, dealing with the specific action of certain substances on organisms and/or with the fundamental mechanisms governing the function of receptors.

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B-1348 Louvain-la-Neuve
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All applications should be accompanied by a cover letter, a curriculum vitae, and a complete list of publications, in triplicate, as well as by the works to be considered.

The Jury is composed of members of the scientific committee of the Fondation Jean-Marie Delwart and of members of the Académie Royale des Sciences de Belgique.

The Prize will be awarded in December 2000 at the occasion of the Public Session of the Académie Royale des Sciences de Belgique.

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Book Reviews

127 Years of Research on Expression of Emotions

The Expression of the Emotions in Man and Animals (3rd ed.), Charles Darwin. (The "Definitive Edition" with commentaries by Paul Ekman). New York & Oxford: Oxford Univ. Press, 1998, 473p., Hardback, $30.00. ISBN 0-19-511271-7.

What the Face Reveals: Basic and Applied Studies of Spontaneous Expression Using the Facial Action Coding System (FACS), Paul Ekman and Erika Rosenberg (eds.). New York & Oxford: Oxford Univ. Press, 1997, 511p., Paperback, $39.95. ISBN 0-19-510447-1. [Hdbk, $65.00, ISBN 0-19--510446-3]

Reviewed by Thomas R. Alley. Department of Psychology, Clemson University, Clemson, SC 29634-1511, USA.

The Expression of Emotions (a shortened title used throughout this review) presents Darwin's naturalistic approach to emotions as the combination of specific feeling states with bodily expressions. Using the key insight later rediscovered by the founders of ethology, that biological evolution affects behavior patterns just as it does physical characteristics, Darwin presents new methods, theories and data bearing on emotion and expression as evolutionary products that reveal the continuity of species and the common origination of human races. Within this book can be found numerous observations, insights and studies of infants, "idiots", animals, the blind, and the insane. Also within this amazing book is the first use of the method of presenting photographs of expressions to different people, inquiries on the universal nature of various human emotional expressions, suggestions of vestigial behavioral patterns in the displays of animals and humans, and analysis of the specific human facial configurations that may be used to express a wide variety of mental states.

It's hard to overstate the importance of the Expression of Emotions. This book is one of the seminal books for both ethology and psychology. Moreover, Darwin's legacy is readily apparent in contemporary research on facial expression, as is evident in two recent collections: Russell and Fernandez-Dols (1997) and What the Face Reveals (reviewed below). Konrad Lorenz wrote in the Preface to a previous edition of Expression of Emotions, Darwin foresaw "in a truly visionary manner the main problems which confront ethologists to this day and ...mapped out a strategy of research which they still use." Rather than summarize and critique this well-known book (see Ghiselin, 1969, Chap. 8), however, my main purpose herein is to highlight the value of this new edition.

This new edition of Darwin's classic should be of interest to almost every reader of this Bulletin, even those who already have a copy of an earlier edition on their bookshelf. Even if this were simply a well-produced hardbound version of this illustrated classic it would be a real bargain at the retail price of $30. This new edition is even more valuable than most people, including those familiar with previous editions, would guess. Some history is needed to understand this claim.

Darwin began work on what would become the Expression of Emotions in 1838. When originally published in 1872, it was a best seller. Darwin made revisions and additions for a 2nd edition but did not succeed in seeing a revised version published because the publisher still had copies of the 1st edition on hand. Thanks to his son Francis, a 2nd edition was eventually published in 1890, eight years after Darwin's death. Nonetheless, recent English language reprints have generally been facsimiles of the 1st edition (Barrett et al., 1986). Moreover, it is now clear that the rare 2nd edition is not exactly as Darwin himself would have liked, in part because his son took the liberty of making a few additions of his own. Extensive research by Ekman, relying heavily on the notes in Darwin's own 1st edition and on Darwin's correspondence with his publisher, was used to produce a text that comes as close as possible to Darwin's intended revision.

The value of this edition goes beyond the importance of providing a more accurate presentation of Darwin's views on the expression of emotions. In addition to giving readers the first definitive version of Expression of Emotions, this version provides several substantial contributions by Paul Ekman, one of the most important contributors to the scientific study of facial expressions. Ekman's contributions include a brief preface for this 3rd edition, a 16 page Introduction (which is obviously influenced by Ghiselin (1969)), plus a provocative and informative 31 page afterword on the history of debate about the universality of emotional expression. His most useful written contribution, however, may be the extensive commentaries scattered throughout this edition. These commentaries largely concern relevant research and theory from about 1960 onwards, but criticisms, passages from pertinent correspondence, reader tips (e.g., comments adding historical context or clarifying terminology), and alternative explanations also appear. There is no question that this masterful major work deserves some critical commentary, most notably for Darwin's use of inheritance of acquired characteristics as the mechanism used to explain the origin of a number of expressions. Ekman's commentaries ensure that such mistakes will not be passed on to contemporary readers while also extending to Darwin an sympathetic understanding of the scientific milieu in which Darwin worked. (In the case of belief in inheritance of acquired characteristics, Darwin was unaware of the genetic basis of inheritance.)

This new "definitive" edition is just that. It includes the "Preface to the Second Edition" by Darwin, an improved index covering both Darwin's text and Ekman's commentaries (differentiated by print type), and six Appendices. These Appendices include Darwin's obituary written by T. H. Huxley, a list of changes in the body of the text made for the 2nd edition, and three reports by English art historian Phillip Prodger concerning the book's photographs.

What the Face Reveals is a collection of 22 reproduced articles concerning or using the Facial Action Coding System (FACS)

developed by Ekman and Friesen (1978). [In brief, the FACS is a comprehensive means of coding all observable facial movements, including some that are probably not related to emotions, based on 44 "action units" such as a wink or lowering of the eyebrows.] This collection of papers did not attempt to sample from the whole realm of research in which FACS has been applied. In fact, papers on the widely researched topic of recognition of facial expression were excluded. Instead, as indicated by the book's subtitle, this volume brings together research for which facial expression is a dependent variable. This may strike many readers as rather restrictive but consider some of the diverse questions addressed:

- "Is the startle reaction an emotion?" (Ekman, Friesen & Simons, Chap. 1)
- What is the relationship between facial expression and affective experience? (Rosenberg & Ekman, Chap. 3; Ruch, Chap. 4)
- Do neonates produce different facial expressions in response to different basic taste qualities? (Rosenstein & Oster, Chap. 14)
- Does embarrassment have a specific facial expression? (Keltner, Chap. 6)
- Are there consistent differences between spontaneous ("genuine") and deliberate facial expressions? (Ekman, Friesen & Sullivan, Chap. 9; Frank, Ekman & Friesen, Chap. 10; Gosselin, Kirouac & Doré, Chap. 11; Hess & Kleck, Chap. 12)

In addition to these questions, issues relating facial behavior and psychopathology are addressed in most of the papers (Chap's 15-21) in Part 2 of this volume ("Applied Research"). The differences in facial behavior between different clinical populations is a common theme here. A paper by Heller and Haynal (Chap. 19) asks whether or not facial behavior can provide clues about suicidal behavior. Other chapters concern facial expression and pain (Craig, Hyde & Patrick, Chap. 7; Prkachin, Chap. 8), hemispheric specialization (Hager & Ekman, Chap. 2), "Extraversion, alcohol, and enjoyment" (Ruch, Chap. 5), Type A personality and facial behavior (Chesney et al., Chap. 22), and cross-cultural differences in infants' facial responses (Camras et al., Chap. 13). After decades of neglect, the papers here reflect the "affective revolution" in the behavioral sciences whereby emotion regained respectability as a research topic; respectability lost not so long after the publication of Darwin's 1872 book.

Clearly, the papers in this volume cover a lot of territory in terms of issues, populations and subject matter. This collection is not as strong on diversity of methodology or orientation, although even the limited number of papers in this volume reveals an impressive diversity of methods that can be employed in research that uses the FACS. Still, the FACS is a difficult method that is not optimal, or even suitable, for all studies of facial behavior. Even researchers addressing the same questions from the same basic perspective may use alternative methods of coding or recording facial behavior, the most notable alternatives being Izard's (1979) MAX system and EMG (electromyography) recordings. The pros and cons of these various methods are nicely presented in Rosenberg's Introduction and elsewhere with, naturally, a strong case made for the FACS in many situations. Not surprisingly, researchers are discovering that important information may be contained in the temporal dynamics of facial expressions that perceivers may not be able to recover from static displays such as a single photograph. For instance, Gosselin et al. (chap. 11) and Hess and Kleck (Chap. 12) both show that temporal dynamics provide useful information for distinguishing spontaneous and posed facial expressions. One benefit of the FACS (and some other techniques) is that it might encourage researchers to pay more attention to facial dynamics.

The 'problem' of too much uniformity in orientation or perspective is another matter. Fortunately, another collection of papers (Russell & Fernandez-Dols, 1997) on, loosely speaking, facial expression has been published recently that, when combined with the book assembled by Rosenberg and Ekman, provides an easy means of collecting a balanced and representative set of papers reflecting the

current diversity in the field. It is particularly fortuitous that the most important researchers on facial behavior not represented in What the Face Reveals – including Frijda, Fridlund, and Izard – have new papers in this other collection, as do some of Ekman's opponents (e.g., J. A. Russell).

All of the articles reproduced in What the Face Reveals are fairly recent, having originally appeared between 1982 and 1995, and most are from the 1990's. The volume certainly serves as a convenient collection of papers that originally appeared in 13 different journals representing a variety of specialties. Since many of these papers were published in prominent and readily available journals, one might question the value of this compendium, beyond the obvious convenience. This concern is quickly laid to rest since each reproduced paper is followed in this book by an afterword by one or more of the original authors. These afterwords usually discuss relevant studies that have appeared subsequent to the reprinted paper. Some also provide interesting background for the reprinted paper, make suggestions about future research, or note important unanswered questions. Moreover, the book concludes with a fine new chapter by Ekman summarizing "What we have learned by measuring facial behavior". Finally, the paper by Heller and Haynal (Chap. 19) originally appeared in French, and appears here for the first time in an updated English translation.

In short, this volume provides a convenient collection of classic papers with up-to-date commentary and discussion of numerous citations of follow-up research. What the Face Reveals includes a subject index but, unfortunately, no author index. Were there an author index, it would reflect numerous references to Darwin's Expression of Emotions, highlighting his long-lasting and widespread impact.

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Reclaiming a Scientific Anthropology

By Lawrence A. Kuznar. Altamira Press (Sage Publications), Walnut Creek, CA, USA, 1997, $22.95 (paper), 284 pp., ISBN 0-7619-9114-X.

Reviewed by W.C. McGrew. Anthropology & Zoology, Miami University, Oxford, OH 45056, USA.

What do the following groups have in common? Racialists, fundamentalist Christians, literati, and left-wing radicals? All, according to author Lawrence Kuznar, are opposed to scientific explanations of human behavior, though, obviously, for different reasons. Kuznar, a socio-cultural anthropologist whose empirical work has been on Andean herders, focuses on the impact of this opposition on anthropology, but he might just as well have done so on ethology. (For the record, ethology is mentioned just once in the book, in passing, but most of the topics addressed are familiar ones to human ethologists, see below).

Kuznar's thesis is both a defense of the scientific method and a rousing attack on antiscience, especially post-modernism. (Ethologists who do not work in anthropology departments may not realise the extent of such divisiveness in this discipline. A well publicised case in 1998 was the rancorous split-up of the anthropology department at Stanford University in California. At one of the most prestigious universities in the USA, the rifts were so deep between the scientists and humanists that they reconstituted themselves as two separate departments, much to the consternation of the university). His 49-page first chapter is a primer on the philosophy of science and its practice when applied to Homo Sapiens. All of the points he raises apply equally well to human ethology, e.g., the need for operational definitions, representativeness, reliability, validity, rigorous and falsifiable hypotheses, etc. We are reminded that science is the ultimate in revisionism, so that such self-correcting mechanisms should caution us to avoid absolutism, and the pontification that may follow on from it. This is not new, but it is usefully presented, with old (Piltdown hoax) and new (Neanderthal humanity) examples.

Kuznar is more contentious when he takes the offensive. He argues that science is not the only epistemology, and that typically scientists adopt a live-and-let-live attitude toward alternative views. The "Gaia hypothesis" may be more spiritual than scientific, but scientists do not condemn it, much less call for its eradication. However, the extreme anti-scientists have no place for science, since for them all knowledge is a social construction of reality. If this is so, then there are no data, and without data, there is no science. Thus, biblical creationists and postmodernists are allied in their opposition to the application of evolutionary theory applied to human behavior. Strange bedfellows, indeed! (Of course, this alliance does not hold up to logical scrutiny, for if the Bible is just another text, then it is just a mutable as any other....)

Kuznar's best example of scientific analysis is his extensive treatment of Man the Hunter, and how the subject has been revised repeatedly by syntheses from cultural ecology, evolutionary anthropology, behavioral primatology, etc. Similarly, for archaeology, he rehearses the history of analysis of the Hopewell mound culture of eastern North America, which turns out to be fully grounded in indigenous peoples, without need for recourse to alien cultures. Less satisfactory is Kuznar's treatment of issues of the nature and nurture of intelligence, in which the usual demons (e.g., Herrnstein and Murray; Rushton) are thrashed without mercy, as if the subdiscipline of psychometrics did not exist. (Kuznar falls into the trap of saying that unless a phenomenon is explicable by an adequate explanatory theory, that phenomenon is invalid and uninteresting. This may seem unnecessarily harsh to ethologists, who sometimes describe events before they have a way to explain them.)

All in all, Kuznar seems to have met his goals, of writing a treatise addressed to students, who otherwise may well be confused about where scientific method fits in among all the politically correct rhetoric of present-day academia. At its modest price, this is a book worth recommending to them.

Evolutionary psychology: The new science of mind

by David Buss. Allyn & Bacon, Boston, 1999, 456 pp.

Reviewed by Bill Charlesworth. P.O. Box 18, Stockholm, WI 54769 USA

Responding to evolutionary theory has been a good thing for the behavior sciences. The exciting new research reported by Buss shows how productive the theory can be when it is operationally formulated and tested by enthusiastic researchers.

Buss succinctly spells out the nature of the revolutionary advance the theory is making for psychology: "Evolutionary psychology cuts across these [interdisciplinary] boundaries and suggests that the field of psychology would be better organized around the adaptive problems that humans have faced over the long expanse of evolutionary history." (p. 371)

He then intensively and comprehensively treats aspects of human behavior that have either been frequently ignored (the "positive" function of aggressive behavior), misinterpreted (the grounds of sexual conflict), ridiculed into non-existence by many psychologists (sex differences in behavior and preferences), thought too obvious to be worth research effort (sexual attraction and harassment), or unexpected (sperm competition). This is revolutionary stuff and Buss does a good job in presenting it.

Chapters 2 and 3 introduce what is to come by framing them in terms of two important points--the logic of ideas and methods that structure the evolutionary approach and the organizing principle driving it. Chapter 2 contains an overview of most (not every--see below) of the concepts evolutionary theorists use to understand the "core of human nature" as well as the methods used to instantiate such concepts empirically. It also includes a refreshingly novel list of guidelines for evolutionary researchers. These guidelines include (along with evolutionary theory) "knowledge of universal structures", "traditional societies", "current mechanisms", "paleoarcheology", and "task analysis". How is that for being non-disciplinary as well as conceptually astute? Applying evolutionary theory to humans requires knowledge from everyone who knows anything about them.

Chapter 3 states the driving question--Survival: what adaptive mechanisms exist in humans to ensure it? Plagued by "food shortages, toxins, predators, parasites, diseases, and extremes of climates" humans have evolved numerous ways to survive--at least long enough to reproduce. Such ways are never foolproof, however, and may include pre-reproductive suicide. But that is the essence of evolution--novel variations and differential selection. Costs can be very high and the process never stops.

The domains of survival are covered in Chapters 4 through 12. They should be highlighted:

"Women's Long-term Mating Strategies" and "Men's",
"Short-term Sexual Strategies",
"Problems of Parenting",
"Problems of Kinship",
"Cooperative Alliances",
"Aggression and Warfare",
"Conflict Between the Sexes",
"Status, Prestige and Social Dominance".

Each chapter is lengthy and clearly worth taking time to read carefully. The volume, however, has two problems, one minor, and the other puzzling, if not close to fatal. The first problem is historical and appears in the Preface and Chapter 1, "The scientific movements leading to evolutionary psychology". In the former, Buss notes that "Charles Darwin must be considered the first evolutionary psychologist for this prophesy at the end of his classic treatise, On the origin of species (1859): 'In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation'." (p. xix)

The problem here is that in various editions of On the origin.. Darwin's quote reads differently. It says, "In the future I see open fields for far more important researches. Psychology will be securely based on the foundation already well laid by Mr. Herbert Spencer, that of the necessary acquirement of each mental power and capacity by gradation." (p. 373) But Spencer is not mentioned by Buss (not even critically) even though it was Spencer, not Darwin, who was enthusiastically received by scientists dealing with human behavior.

Buss's historical problem, however, does not end here. In commenting on ethology, he erroneously claims that Lorenz and Tinbergen "started a new movement called ethology" (p. 31). However, this was not the case. Lorenz and Tinbergen obviously made enormous contributions to ethology, but animal researchers such as Spalding, Heinroth, Whitman, von Uexkull, and Craig were doing ethology earlier.

Buss also notes that one of ethology's "problems" was that, because ethologists focus on observable behavior, they "did not look 'inside the heads' of animals to the underlying

mechanisms responsible for generating that behavior." (p. 12) This statement is patently false: ethology's research literature is inundated with studies on internal mechanisms, the vast majority albeit being neurophysiological and perceptual (Eibl-Eibesfeldt, 1975).

Many ethologists, however, did not ignore psychological mechanisms. Lorenz (1962) is well-recognized as an evolutionary epistemologist who drew heavily upon Kant: he viewed Kantian apriori thought forms as products of evolutionary processes, not mysterious givens of the human psyche. Also a convincing argument can be made that Lorenz was actually "The grandfather of evolutionary psychology" (Grammer, 1998). It can be added that evolutionary epistemology was given a lift by Campbell (1960) who is not mentioned by Buss.

Actually, not getting the history right runs the risk of giving the impression that Darwin's speculations and theory are the only progenitors of evolutionary psychology. In fact, Darwin "only" provided a general framework (his theory of natural selection) along with some interesting insight into the mind--see his "M Notebook" in Gruber (1974). It was those who followed Darwin--for example, most of those 13 scientists to whom Buss dedicates this book--who really put evolutionary into psychology.

The second major problem with the book is more serious. It bears directly upon the concept of evolution itself. Darwin's theory of evolution is primarily a theory of individual differences and selection forces that operate upon them. Without both elements, no evolution.

Buss, of course, realizes this, but waits until the last chapter to mention it. In my estimation it would have been more informative if he had wrestled with the rationale for focusing only upon species-specific "modules" for processing information and ignoring individual differences in intelligence. Historically, intelligence, as measured by g and other factors, has been a dominant concept in psychology (frequently within the psychometric tradition) for over a century. Also, it is a historical fact that the sources of intelligence were major concerns of early Darwinians such as Galton. Parenthetically, it is ironic that species-specific universals and brain modules have been major concerns of ethologists for decades.

In short, not to try to account for the concept of intelligence and individual differences seriously compromises this new discipline's claim to be evolutionary--at least according to Darwin's definition of evolution.

So what to do? Buy the book anyway. It is a clear and enthusiastic overview which does credit to this "revolutionary new science". Hopefully, a revised edition will be forthcoming.

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