I have the following critiques of points made last night (April 5, 2006) at the Panel Discussion on Intelligent Design and Evolution in which Prof. Cornelius Hunter of Biola University represented intelligent design and Profs. Richard Harrison, chair of the Ecology & Evolutionary Biology Department at Cornell, and Kern Reeve, professor in the Cornell department of Neurobiology & Behavior, represented evolution.

I don't think that Prof Hunter made adequate replies to some of the problems with points made by Profs. Harrison Reeve. The important additional responses, as I see them, are as follows

1. Prof. Harrison made an unusual and seemingly effective response to one of Prof. Hunter's main critique's of evolution. Prof. Hunter's critique was that the genome provides evidence that not all life descended from the same original cell. He claimed that there is evidence of several distinctly different sets of DNA in known existing life forms that could not possibly have all descended from the same ancestor. Prof. Harrison did not dispute this assertion. Based on his failure to dispute it, I will assume that Prof. Hunter's statement is an acknowledged fact. Prof. Harrison's response was to say that this is not a problem because there could have been 25 distinct ancestors from which all life descended (I can't recall whether he said 23 or 25, but it was some number on that scale).

I have to admit that this is an effective counter-argument to Prof. Hunter's critique of evolution, but it is an argument that raises many more problems for evolutionists than it solves. By using this argument to solve a biological evolution problem, he creates huge problems for pre-biological genesis of the first cell, for now one needs to independently generate not 1 cell but 25 (or was it 23?). If the likelihood that one cell arose by purely random coincidence is, say 10^(-100), which is essentially zero, then the likelihood that 25 independent first cells of 25 distinct genomes arose independently is 10^(-2500) which is much, much, much closer to zero. If there had been any Abiogenesis researchers in the audience, then I fear that they would have suffered cardiac arrest when they heard Prof. Harrison's response. The problem of ever explaining how even one cell arose is hard enough without having to explain how 25 different cells arose.

To make an analogy, suppose that Prof. Harrison were assigned to defend Seattle from an atom-bomb-equipped missile that had been fired from North Korea. If he were to defend Seattle the way he defended evolution in his comment about the 25 different original genomes, then he would decide to destroy the incoming atom bomb by detonating a hydrogen bomb just above the city at the moment then the North Korean missile was approaching. He would have very effectively destroyed the North Korean missile without the need to have very accurately tracked it or hit it. The North Korean missile would have done no damage whatsoever to Seattle. Unfortunately, Prof. Harrison's Hydrogen blast would have done far more damage to Seattle than the North Korean bomb ever could have done. In short, Prof. Harrison's cure is worse than the disease.

2. Prof. Kern Reeve made some important points about prediction in science and about his work on the relationships of certain mating behaviors, presumably genetically encoded, to population patterns. I don't recall all that he said about it, but I found it

fascinating, even inspiring, and I felt that Prof. Reeve expressed the better grasp of the importance of the ability to do prediction in the realm of validation of scientific theory. Unfortunately, there were also some flaws in Prof. Reeve's understanding of the interaction between prediction and theory.

A. The first flaw, if I understand what was said last night, concerns the relationship between the predictions that Prof. Reeve makes in his work and the validity of the theory of macro evolution. He implied that the two were intimately connected. In other words, he seemed to be saying that he could not have made the predictions that he made without the model that all of the complex forms of life, as we know them, arose from a single cell or set of single cells through much repeated application of the types of processes that he is studying. This is a non sequitur. It seems obvious that Prof. Reeve is studying and making predictions in the field of micro-evolution or even, one might say, in the field population dynamics. To say that, given a certain genome or given a certain predisposition to a certain type of behavior, we can predict a certain relative frequency of the sexes in a certain species is not to say that we can explain how that species came to be what it is, to have the genome that it has, to have the behavioral predispositions that it has.

Again, to make an analogy, one could call the science of apples falling "Micro-Newtonian mechanics" and the science of the planets orbiting the Sun "Macro-Newtonian mechanics." For Prof. Reeve to argue the way he argues about his research supporting macro-evolution would be the same as arguing for the acceptance of Newton's explanation of the motion of the planets by doing predictions and experiments about the falling of peaches, bananas, and cherries. Suppose Newton had proceeded in this fashion. Suppose that Newton had merely reasoned from the falling apples to other types of falling fruit. He would never have bothered to develop calculus and to use it to solve the difficult 1/r^2 nonlinear differential equation that was needed in order to prove that the apple falling and the planets orbiting were one and the same thing. Suppose that he claimed that his successes with the other fruit constituted sufficient reason to accept his proposition that he had explained the orbiting of the planets around the Sun. If that had happened, then Newton's name would not be a household word and I would not have the job that I have today.

Prof. Reeve may believe in macro-evolution, he may do good micro-evolutionary science or population dynamics science, and he may believe that his success in the lab or at the computer somehow support his macro-evolutionary beliefs, but his beliefs about this connection do not constitute an actual connection. Prof. Reeve correctly disputed Prof. Hunter's argument that Newton's success with F=m*a lent credence to Newton's belief that F=m*a is true because there was an intelligent designer behind who decided that F should equal m*a. Prof. Reeve's excellent point is that one can use F= m*a quite successfully without needing to believe that it arose because of intelligent design.

Isn't it true, however, that one can believe in intelligent design rather than macro-evolution and still do all of the same kind of laboratory and computer work that Prof. Reeve does with equal success? Based on what I heard from Prof. Reeve last night, I

believe that I could do his kind of work, should I learn the necessary background material, without ever having to accept his supposition that all of the different biological forms came into existence by purely naturalistic processes. On the other hand, Prof. Reeve could never do the type of rocket science that I do if he did not accept that F = m*a. Thus, my ability to make successful predictions in the lab and at my computer constitutes real proof that F= m*a (that is, in the low-speed, low-mass limit that does require Einstein's theory of relativity), but Prof. Reeve's success at prediction does not constitute proof of macro-evolution.

B. The second flaw in Prof. Reeve's presentation about prediction in science is that he demands too much of prediction. A theory has the right to define what it will predict. Its critics do not have to right to demand that it predict whatever they want to predict.

This touches on a very important subject in science, which I will call the humility of science. Good science has a sense of humility. It says that science can answer some important questions about the physical world in which we live. It does not say that it can necessarily answer any and every question that someone might like to ask about our physical world. Science has been very successful at answering some questions, and the number of questions that it can answer has grown steadily. Many people have mistakenly (sometimes arrogantly) misconstrued this progress to imply that science can answer any question than anyone might want to pose. The truth is that we do not know the limits of science, but it would be wisest to suspect that it may have definite limits. It appears that cosmology has certain limits when it looks at the early stages of the big bang, and certainly it can say nothing about anything that might have happened before the big bang. Behavioral sciences seem to have clear limits too. Why have we made huge progress on treating heart disease and minimal progress on treating mental disorders? One can't help but wonder whether we are coming up against a limit of science. Of course, the people with a vested interest in the research and clinical treatment dollars associated with mental disorders will never admit that there are fundamental limits to their science, but the rest of the public suspects that they are not to be totally trusted on this matter.

As an example of the limitation of science, consider quantum mechanics. One of its fundamental tenants, known as the Heisenberg uncertainty principle, is that certain questions of classical physics are unanswerable at the atomic and sub-atomic level: one cannot know exactly the position and velocity of a particle. This principle drove classical physicists, including Einstein, mad. They hated it. They were used to assuming that such questions could be answered, and the new quantum mechanics folks came along and told them that they had to stop asking the usual questions. This was purely a negative result, yet it was a key to making advances in this area.

The theory of intelligent design, or put better, the assertion that there exists irreducible complexity in certain biological mechanisms or biochemical processes, is similar. It makes few predictions. Its principle prediction is that there will never be found a naturalistic descent-with-modification (i.e., natural selection) explanation for how these irreducibly complex systems came to be. This is a negative prediction, and many

evolutionary biologists don't like its negativity. It is a prediction, nonetheless. It does not give power to predict about the sex ratios in certain populations, as Prof. Reeve would like it to, but that is not a problem, because it did not claim that it would make such predictions. Although it doesn't make the usual predictions that certain biologists might like, its prediction is an important one. If true, then the assertion of irreducible complexity is on par with the assertion in physics of the conservation of mass/energy and the assertion in chemistry of the immutability of the elements in chemical reactions. One cannot develop all of physics or all of chemistry from either of these principles, but woe to the physicist or chemist who does not understand and accept them. That physicist is liable to start trying to build a perpetual motion machine; that chemist is liable to start trying to find a chemical recipe for turning lead into gold. It is the same with the evolutionary biologists. The principle of irreducible complexity does not give one all of biology, but if true, it serves to divert the biologist from wasting time by trying to answer a question to which there is no scientific answer.