How Not to Identify Innate Behaviors

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Konrad Lorenz suggests that adequate grounds for classifying some behaviors as innate are to be found in the results of what he calls "the deprivation experiment": "... the experiment of withholding from the young organism information concerning certain well-defined givens of its natural environment." (Lorenz 1965, p. 83). Thus, a stickleback fish is deprived of the information that its rival has a red belly. The stickleback is then confronted, for the first time, with a red-bellied rival (or a red-bellied dummy). If that stickleback responds with species-typical rival-fighting behavior, then (according to Lorenz) the experiment has established that the stickleback possesses certain innate information about its natural environment. On the other hand, should the stickleback fail to respond in this way, Lorenz tells us that "...we should not be justified in asserting that this response is normally dependent on learning. There would still be the alternative explanation that, while trying to withhold from the animal information only, we have either inadvertently withheld 'building stones' indispensable to the full realization of the blueprint contained in the genome or else we are withholding in the experimental setups a stimulus situation necessary to release the behavior we are undertaking to investigate." (Lorenz 1965, p. 85).

A corresponding difficulty might seem to stand in the way of inferring innateness from the first-mentioned result of the deprivation experiment, since we might have failed to withhold information required for the stickleback to learn to recognize its rival. Lorenz may be aware of this difficulty, but he seems to regard it as surmountable: he warns us against the possibility of "extremely quick 'flashlike' conditioning." (Lorenz 1965, p. 96). Indeed, this possibility suggests to him a difficulty with the stickleback experimentation: "If on this first occasion of presentation, the dummy was red below, it is quite possible that the innate information is confined to 'red' alone and that the configurational property 'below' is learned in a flash." (Lorenz 1965, p. 96). But notice that this example only partly confounds the deprivation experiment, by suggesting that not all the information is legitimately inferred (from that experiment) to be innate. And note, too, that the stickleback is presumed to have been deprived, until the

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eventual presentation of the dummy, of the information which allows for "learning" (i.e., for rapid conditioning).

Setting aside any complications posed by viewing information as the innate component of instinctive behaviors, let's examine the methodological credentials of the deprivation experiment as a means of identifying native behaviors which are simply unconditioned responses. Let's begin by asking whether an embryonic chick might be conditioned to respond (by pecking) to an exterior stimulus, red seeds, by being exposed to a comparable though causally distinct stimulus, red spots on the inside of the eggshell. To this question, an affirmative answer seems quite reasonable: even without any causal link between stimuli inside the egg and those outside the egg, these two classes of stimuli could be functionally equivalent: conditioning the embryonic chick to peck at the red spots on the interior of its eggshell might effectively condition that chick to peck, subsequent to hatching, at tiny red seeds.

But if this answer inclines us to suppose that the contours of the concept of innateness have become hopelessly blurred, Lorenz is ready with the clarifying notion of what he once, facetiously, called the "innate schoolmarm," i.e., a "phylogenically programmed teaching machine". (Lorenz 1965, p. 104). The idea is that if the embryonic chick learns to peck at red seeds, then the chick must have been preprogrammed, genetically, to learn this from sources inside the egg. The notion of such a phylogenically evolved "teaching mechanism" gives rise to a new category of behaviors--a category not simply of innate _and_ conditioned behaviors (since that would also include any initially innate behaviors subsequently modified by experience), but a category of behaviors which are innate _by virtue of being_ conditioned.

Far from being troubled by such a category, Lorenz seems to view it as a valuable addition to his ethological theory, an addition which allows him to acknowledge the possibility of embryonic conditioning, without giving up on the notion of innateness. But let's suppose that red spots appear on the inside shell of the egg only when the outside temperature is between certain specific limits and that, when these limits are exceeded, the absence of red spots precludes the chick's learning to peck at red seeds or any other small red target-stimuli. Has our imaginary chick learned its characteristic pecking from an innate schoolmarm or from the outside environment? Is this pecking innate or acquired? Finally, is there any reason to think that the deprivation experiment could be used to tease out the innate elements of this situation?

Since learning or conditioning prior to hatching is at issue here, the deprivation experiment should presumably be conducted on the embryonic chick. Given the aforementioned details of the case, there is one obvious way to deprive the chick of the red spots on its shell's interior: raise or lower the temperature beyond the prescribed limits. The danger is that we might also thereby change other processes crucial to the case: who knows, say, what effects extreme cold might have on an innate schoolmarm? But perhaps there would be some less disruptive way of eliminating the spots: let's suppose that some very slight irradiation does this without otherwise affecting the chick or its present environment. By hypothesis, the chick, thus deprived, will not learn to peck at small red target-stimuli. So, the subsequent reintroduction of the shell spots will not immediately lead to their being pecked. Hence, the deprivation experiment fails here to establish the innateness of the (allegedly) innate-by-virtue-of-being-conditioned behavior.

Of course if the chick's learned pecking at red spots is just the sequel of some earlier innate behavior, some pecking unalloyed with conditioning, then the deprivation experiment might be thought to work to identify that behavior, by depriving the chick of the stimulus which elicits that behavior. But this assumes that the behavior is a response to some stimulus; whereas the pecking might just be a spontaneously emitted behavior, in which case there would be no way to carry out the experiment.

Conducting the deprivation experiment on the hatchling instead of the embryo would lead to the conclusion that the pecking behavior was innate, since it followed immediately after the stimuli were reintroduced into the environment. But all the (hypothetical) facts may not square with this conclusion: just what role did the temperature play in what transpired? The innate teaching mechanism used the red spots inside the shell to condition the embryo to peck at small red target stimuli. Yet the environment outside the egg produced the spots; so, the embryo learned how to do something which is well-adapted to, has survival value in, the external environment, and the embryo learned this behavior partly from that environment.

One defense of the deprivation experiment would be to distinguish between behavior learned in the egg or womb <u>directly from</u> the same <u>external stimuli</u> with which that behavior is coordinated (e.g., a chick's learning its mother's song in consequence of hearing it) and behavior learned <u>as a result of</u> external environmental <u>factors causally independent of those stimuli</u> (e.g., the chick's pecking behavior, resulting from temperature extremes in the external environment). Although the former sort of behavior does not deserve to be called innate, perhaps the latter sort does.

Of course this defense assumes that it will be possible in practice to determine in which of the two different ways a given piece of behavior has been learned. This might seem an easy feat, provided only that one can identify the stimulus to which the embryonic animal is exposed and from which it learns the behavior. Let's suppose that these stimuli internal to the egg have been identified in both hypothetical cases. Since the mother's song is the same stimulus within the egg as outside it, we may safely say that if the embryonic chick learns, within the egg, from that stimulus to recognize its mother's song, that learned behavior does not deserve to be called innate. But if the phrase "same stimulus" simply means "functionally equivalent stimulus", then the red spots within the egg are the same stimuli as the bugs or seeds with which the other chick's pecking behavior outside the egg is coordinated. And, in that case, this chick's pecking behavior seems no more deserving of the label "innate" than does the other chick's recognitory behavior.

But, of course, what we mean by "same stimulus" must have to do with causal connectedness, for it was the lack of such connectedness which was supposed to warrant our use of the term "innate" for behaviors otherwise learning-dependent upon the environment. Accordingly, the mother's song inside the egg is causally connected with the mother's song outside the egg; while the red spots inside the eggshell are not causally connected with (and, hence, not the same stimuli as) the red bugs or seeds outside the egg. Or are they? For all we know, the environmental factors which cause the spotted shell may also be factors essential to the presence within that environment, of red bugs or seeds (e.g., if the temperature strays beyond that range, it may be too hot or too cold for other organisms to produce their tiny red offspring, the chick's favored diet).

Admittedly, the lines of causal influence are not fully comparable; for, in the case of the mother's song, the exterior stimulus itself is the cause of the interior stimulus; whereas, in the case of the red spots, the exterior stimuli are caused by the same factor which also, independently, causes the interior stimuli. But why should these differing lines of influence matter to the issue of innateness? According to Lorenz, the most important consideration is whether or not the embryonic chick has been exposed to the environmental exigencies to which the behavior is well-adapted. And, notwithstanding the conspicuous lack of any direct line of causal influence from stimuli external to the egg to ones internal to it, Lorenz could insist that the chick-species whose shell is sometimes spotted, sometimes not, has been exposed to the relevant environmental exigencies, to the temperatures outside the egg which determine the availability of certain food stuffs.

Now one presently noteworthy feature of ordinary instruction is that the teacher need not expose his or her students (say) to lions or tigers, in order to teach them to respond appropriately to those beasts: for some instructional intents and purposes, a mere picture of these animals, combined with suitable commentary, will be sufficient. And while we have not gone into, have not imagined, the possible details of the instructional activity of an "innate schoolmarm", perhaps we ought to concede an analogous point for her: the innate schoolmarm need not expose the embryonic chick to bugs or seeds in order to teach it to respond appropriately to those target-stimuli--red spots inside the shell might suffice to introduce that chick to the right sort of stimulus-occasions for pecking after it hatches.

One may be tempted to dismiss out of hand any conclusion drawn from a rather far-fetched, purely hypothetical example. But despite its implausibility, the spotted-shell example is possible, and that's all that's necessary to warrant my conclusion: the deprivation experiment can, at least in principle, wrongly identify certain behaviors as innate.

A possible objection to this conclusion is that, for all we know, the sorts of behaviors which might be thus wrongly identified have never existed, do not and will never exist. But by the same token, we don't know that those behaviors haven't been, aren't, and won't be far more prevalent than we may imagine. Were there some independent way to establish the facts about the genesis of behavior exhibited after hatching or birth, then it would be possible to say just how rare or common cases problematic for the deprivation experiment really were; but since that very experiment is supposed to be our only indisputable means of ruling out the hypothesis that a given form of behavior was learned, of determining that that behavior is innate, there would seem to be no sure way to resolve either the smaller issue about the relative

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frequency of problematic cases or the larger issue of whether any behavior may be said, with any confidence, to qualify as innate.

Although it is difficult to make a precise diagnosis of the reasons why the deprivation experiment fails to identify innate behaviors, a somewhat fuller appreciation of what's wrong with that technique might be had by considering theories (or, theoretical outlooks) which call into question the whole idea of identifying innate behaviors. One such outlook finds expression in some analogical remarks by the Gestalt psychologist, K. Koffka:

> The intensity of an electrical current is proportional to the electromotive force, and to the conductivity of the system. ... But it would be unreasonable to ask how much of this intensity is attributable to the electromotive force and how much to the conductivity of the system. ... On account of his psycho-physical structure an individual possesses certain properties. These properties, together with his external social and physical situation, constitute the conditions of his behavior. ... What we inherit, then, is not a repertory of particular reactions, but a set of internal conditions for response, which together with external conditions, physical and social, co-determine our behavior. (Koffka 1928, p. 121).

The inherited response (or, instinct) is for Koffka a bit of misplaced concreteness: a would-be part-process presumed to correspond to the endogenous conditions for response. The only real process is for him some whole process of behavior, which is co-determined by internal and external, environmental conditions.

Koffka proposes an almost mentalistic interpretation of the innate internal conditions which co-determine various behaviors: "If we inherit anything specific, it is certain needs or stresses which pull us in certain directions. These needs or stresses result in responses which greatly vary with the external conditions under which the behavior occurs, yet remain constant in the direction of satisfying the needs which gave rise to them." (Koffka 1928, pp. 121-122). This account accords well with what James Drever gives as the original sense of the term "instinct", viz., "animal impulse". (Drever 1952, p. 137). And this conception of instincts allows for a generally Darwinian view of their origins; since the 'needs' must be satisfied if the organism is to survive, so there is selection pressure favoring a species whose members are moved in the direction of behavior which satisfies those needs.

Now even the staunchest defenders of the notion of innate behavior appear largely willing, at the present stage of biological knowledge, to concede D. Lehrman's point that environmental factors interpenetrate the processes of growth and development of the individual organism (Lehrman 1970, p. 36). So let's try to conceive of the hereditary determination of behavior in a way which seems maximally compatible with such a conception of ontogenic development: just as a family tree may, for all its complications, be used to trace the origins of a given person back to one among many suspected ancestors, so too it might be supposed that a particular theoretical account of innate (aspects of) behavior could be used to trace the origins of certain (aspects of) behavior back to a distinctively hereditary determination. Does Koffka's theory enable us

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to trace pre-conditions for behavior back along definite lines of hereditary descent? His image of an electric current would seem to suggest an affirmative answer; for however indirect and complicated electrical connections might be, a discharge of current from one point to another implies that some line does exist. Pursuing the analogy, let's consider the sort of connections which Koffka's theory (or some variant of it) might posit.

Need, by Koffka's reckoning, may be viewed as analogous to an electromotive force. Any of the external conditions which, in its capacity as stimulus, serves to release the behavioral response, may be construed as a ground-wire. The behavior itself may be the counterpart of the electrical current. And in order to complete this schematic diagram, some native endowment of the organism must be identified, by way of analogy, with the generating source of the electromotive force.

Perhaps 'need' is not really the best candidate for being the innate factor in Koffka's account. After all, need cannot adequately be defined without reference to the environmental factors with which it is associated--the need for food, for instance, makes obvious reference to (albeit the organism's deficiency of) nutrients. And besides, a far more plausible candidate would be the biological mechanism which generates need qua motive force--that force, presumably, does not even exist (innately or otherwise) in well-satiated organisms. Were we to designate such a generator as innate, the lines of hereditary descent might seem fairly obvious: the discharge of psychical-biological energy suggests--in the terms of what is, for Koffka, more than merely an analogy--a biological energy-generating source; and that mechanism may safely be presumed to be natively endowed (or, genetically predetermined).

As depicted so far, however, our electrical circuit of need does not work--the current is uselessly discharged into the environment. But further theoretical details may be added to achieve more utility: a motor may be attached between the generator and the environmental ground-wires (or, stimuli); and that motor might convert the biological need-energy into mechanical energy which, in the form of physical activity, might ultimately serve to shut off the need-impulse generator. This biological motor and the generator may both be said to be innate, assuming that their evolutionary genesis can be established.

This assumption is not a minor one, of course; and among the details to be worked out would be the identification of specific physiological mechanisms that correspond to elements of the schematic-theoretical design, to the need-impulse generator and the need-reducing activity motor. It is questionable, moreover, whether this jerry-built extension of Koffka's theory accords with his general theoretical presumptions: the presently postulated generator and motor would seem to involve what Koffka himself might regard as illegitimately inferred part-processes of the overall process of behavior. And while going too far in the one direction, this version of Koffka's theory might not go far enough in another. The critical point might be made by comparing Koffka's authorized version with the similar theoretical outlook that emerges in John Dewey's notable critique of the reflex-arc concept. Dewey suggests, among other things, that that concept fails to take account of the complex coordinations required between (what might better be called) "stimulus activities of the organism" (rather than, simply, "stimuli") and the "motor-response activities" appropriate to them: the former are said to "fix the problems" to which the latter are said to provide the solutions (Dewey 1896). Now it could be posited that the activity-producing motor is, by "evolutionary-design", already coordinated with environmental stimuli, that (say) the resulting activity of pecking is pre-coordinated with certain visual target-stimuli; but this views coordination as something achieved once and forever-after functioning properly, whereas Dewey and Koffka view it more plausibly as a process of continuous adjustment. Thus, as Koffka, puts it: "...the situation which presents itself to the sense-organs, after a movement has taken place, determines the continuation of the movement." (Koffka 1928, p. 104). One could, perhaps, devise some other means of accomplishing the requisite task of continual coordination--say, by adding to the schematic account some sort of feedback-mechanism, which readjusts the motor (and/or its mechanical attachments) to the changing environmental exigencies. But these additions, for better or worse, would lead the revised theory still further away from the holistic direction of Koffka's original theory.

Quite apart from its non-Gestaltist bent, such a revised theory might help to vindicate the suggestion that certain internal conditions for behavior are innate. Those conditions (including impulse generators, activity motors, coordinating mechanisms, and assorted interconnections) have been rendered so morphological by the prospective theory that there seems no more reason to question their phylogenic origins than those of any other bodily parts of the organism. And given neo-Darwinian theory's status as "the greatest unifying theory in biology" (Mayr 1963, p. 1), the conjecture that there has been an evolutionary descendence of such determining conditions for behavior seems, if nothing else, a very good bet.

Here we might be reminded, however obliquely, of Leibniz's famous quip about innate ideas: in response to the Lockean slogan that "there is nothing in the intellect but what comes from the senses", Leibniz adds, "except the intellect itself...." (Russell 1900, p. 162). So, too, however much we might wish to deny that nothing is innate in the organism, we must concede that the organism itself, its internal mechanisms and organs which make it the organism it is, is "innate". Mustn't we?

Of course we might harken back to the claim that environmental influences permeate the processes of ontogenic development, and we could use that claim to suggest that the organism is what it is as much because of the environment in which it develops (including the environment within the egg or womb) as because of its genotype. Though even this suggestion, which goes quite far toward effectively eliminating the occasions for using the term "innate" as mutually exclusive of "environmentally determined", could be said to preserve one final candidate for pure innateness: the DNA molecule (assuming, of course, that that molecule had not been subjected to any mutagenic environmental influences after fertilization). But there is something absurd about the idea that only the DNA molecule is truly innate, for that is all too much like saying that only of the letters of the alphabet can we be sure of the spelling: the DNA molecule is the ultimate (ontogenic) basis of our genetic traits and is not itself a genetic trait.

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But if our quest for innate elements has taken us too close to the core of hereditary determination, to its basis rather than to items determined on that basis, then the option remains of retracing our steps and reassessing the conception of innateness which has misled us in our prior search. The first step away from the center lands us back in an area of endogenous organic processes subject to environmental influences. Viewing our movement from center to periphery, in embryological terms, from the DNA in the fertilized ovum to the mature functioning organism, it might be said that even the blastula (the first grouping of embryonic cells) is what it is because of its environment as well as its genetic makeup. So, if we are to make sense of the suggestion that some internal mechanisms associated with the organism's behavior are innate, we shall have to conceive of innateness in some way which circumvents the issue of environmental determination. The deprivation technique, which addresses that issue in a simplistically experimental manner, is of dubious value in furthering this quest for instinct.

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