

This article was downloaded by: [Laurentian University]

On: 20 May 2015, At: 11:46

Publisher: Routledge

Informa Ltd Registered in England and Wales Registered Number: 1072954 Registered office: Mortimer House, 37-41 Mortimer Street, London W1T 3JH, UK



Angelaki: Journal of the Theoretical Humanities

Publication details, including instructions for authors and subscription information:

<http://www.tandfonline.com/loi/cang20>

THINKING LIKE A RAT

Vinciane Despret^a

^a Philosophie et Lettres, Université de Liège 4000 Liège, Belgium

E-mail:

Published online: 18 May 2015.

Routledge
Taylor & Francis Group



[Click for updates](#)

To cite this article: Vinciane Despret (2015) THINKING LIKE A RAT, Angelaki: Journal of the Theoretical Humanities, 20:2, 121-134, DOI: [10.1080/0969725X.2015.1039849](https://doi.org/10.1080/0969725X.2015.1039849)

To link to this article: <http://dx.doi.org/10.1080/0969725X.2015.1039849>

PLEASE SCROLL DOWN FOR ARTICLE

Taylor & Francis makes every effort to ensure the accuracy of all the information (the "Content") contained in the publications on our platform. However, Taylor & Francis, our agents, and our licensors make no representations or warranties whatsoever as to the accuracy, completeness, or suitability for any purpose of the Content. Any opinions and views expressed in this publication are the opinions and views of the authors, and are not the views of or endorsed by Taylor & Francis. The accuracy of the Content should not be relied upon and should be independently verified with primary sources of information. Taylor and Francis shall not be liable for any losses, actions, claims, proceedings, demands, costs, expenses, damages, and other liabilities whatsoever or howsoever caused arising directly or indirectly in connection with, in relation to or arising out of the use of the Content.

This article may be used for research, teaching, and private study purposes. Any substantial or systematic reproduction, redistribution, reselling, loan, sub-licensing, systematic supply, or distribution in any form to anyone is expressly forbidden. Terms &

Conditions of access and use can be found at <http://www.tandfonline.com/page/terms-and-conditions>

translator's foreword

Vinciane Despret's short 2009 book Thinking Like a Rat is a continuation of her concept of asking good questions in animal research and use of the tools of biosemiotics and cognitive ethology to address how animals perceive, interpret, and act upon research situations. She asks, prominently, what does the maze mean for the rat? In doing so she points out that behaviorist research using rats and mazes failed to inquire how the rats perceived the mazes and what the rats found interesting. It also overlooked the ways in which the maze itself was fashioned as a tool that intersected heavily with the everyday habits and navigation of rats living within the walls of a human built environment. Departing from that research and drawing on the critiques of the experimenter effect and the perception of expectation in research, she opens up the field of research involving animals and humans to include more understanding of how animals perceive contexts of research and form intersubjective ties with human scientists as well as other animals that affect the outcome of the research. She points out that animals, like human research subjects, are canny observers of the process who often have a good idea of what the research is designed to reveal, regardless of the lures, dissimulation, and other tools used to mask the true questions at hand. She proposes developing interesting questions that give animals a chance to demonstrate their interests and be interesting in lieu of reductive and standardizing set-ups that seek to hide the true questions at hand or treat all individuals as the same. Interesting research asks animals about their interests and ways of doing things rather than attempting to force

vinciane despret

translated by jeffrey bussolini

THINKING LIKE A RAT

an answer to a question that may or may not be of importance to them. The book is divided into six chapters and an interview discussion. The first chapter is devoted to "Lures and Artefacts" as important concepts and practices in research with animals. The second chapter asks what happens "if researchers are nice with their animals," opening up the considerations about how good questions and genuine interest in the life and mind of an animal can produce much more fascinating results than stultifying repetition. Chapter 3 asks "what a maze can mean" to a rat who experiences it, and how it is that rats recognize and navigate different parts of it. The fourth chapter looks at the perception that animals have of human expectations and how this can

be a major factor in results, as the animals ask themselves “what does it (the human) want of me?” Chapter 5 investigates the idea of response and says “the question of response is a question whose answer changes everything.” The final chapter says that “joy is demanding” and takes account of how recent research in judgment and emotions opens a fertile field for considering how animals judge the questions of research and what they feel about the activities and situations involved. The interview and discussion gives an elucidation of some of the themes and concepts Despret introduces in the book.

lures and artefacts

In the mid-1960s experimental psychology received severe critiques on the subject of the validity of its experiments: experimental subjects conform most often to the expectations of their experimenters. This is to say, as the American psychologists Martin Orne and Robert Rosenthal will each argue in their own way, that every experiment relies in large part on an artefact: the scientists think that the subjects respond to the question that is posed to them, but the subjects, in fact, respond to another question.

If I propose to take an interest in this and to make this detour it is because these critiques touch very close to the subject that I would like to explore. They interrogate the way in which the subjects of the experiments and their responses are affected by the way in which they live and actively take into account that which is expected of them. In sum, we will see, these two critiques pose the problem of the “point of view” of those whom the science investigates, the “point of view” on the question that is addressed to them, or on the protocol to which they are submitted, and how they respond to what they interpreted the question of the experimenter to be. Now, it is exactly this that seems to constitute, even if later and in forms surpassing that of critique, a remarkable slide in research on animals: beginning to take into consideration the point of view that animals have on the way in which

they can take a position in relation to what is proposed to them in scientific research.

The critiques of Orne and Rosenthal emerged at the same time, during the 1960s; both of them emanate from the very interior of psychology since they were both trained by practitioners of experimentation. Their critiques are founded on relatively close empirical premises and are presented in a very similar form: many of the convergences will nonetheless paradoxically lead to very different, even antagonistic, responses and propositions.

We should specify that their critique was not, in and of itself, an absolute novelty. Psychologists were well aware that their subjects could be influenced by what the scientist was looking for. This was incidentally the reason why, in the research, the experimenters tried most often to camouflage the real questions guiding their research, which would permit them to eradicate the hypothesis according to which the subjects would do what was demanded of them *because the researchers asked them to do so*. From the fact that they do not know what is expected of them, because it is hidden from them, the subjects do not do what they do because the experimenter asked them to do so, but for more abstract and more general reasons. This, according to the psychologists, would therefore guarantee the “ecological validity” of the experiment. This describes or demonstrates something that would apply outside the laboratory, which would not be the case if the subjects had done what they did because the scientist had asked them to do so: *that which they did by means of this strategy, they would do in other circumstances*.

When the psychologist Stanley Milgram, to take up a famous experiment dating from the same period, undertakes to study the capacity for obedience in humans, he does not ask his subjects: “are you capable of electrocuting someone because I ask you to do it?” He pretends, on the contrary, that they are taking part in an experiment on the effects of punishment in the learning setting, and that they must give electric shocks to a “student” when he does not respond correctly to questions that they must pose to him (the experimenter

convinces them that he is also a volunteer in the experiment). Since the subjects do not know that they are taking part in research on obedience, Milgram feels well justified in claiming that the true stakes of the experiment will not guide their responses. The problem of expectations, it was thought, had found its solution.

Orne and Rosenthal will, however, take up, each in his own way, this critique of the influence of the experimenter and take it further. On the one hand, this matter of the effect of the question was until then confined almost exclusively to experiments with humans, since it was believed that they were the only ones sensitive to expectations.¹ Rosenthal extends it to animals: they too would be affected by what is expected of them by the experimenters, and this would modify their performances.² On the other hand, if human psychology had thought to find a solution to this problem of expectations in hiding from its subject the real stakes of each experiment (as I just indicated in the case of Milgram), Orne shows that this solution raises still more difficulties than it resolves. The subjects, most of the time, not only predict what the experimenter expects of them but they conform to it with such good will that the care taken to hide these expectations cannot but underscore their extreme importance (Orne; Orne and Holland).

Starting with the work of Orne, we'll approach that of Rosenthal in the next chapter.³ In the beginning, this experimental psychologist, a specialist in hypnosis, did not have a critical dismantling of the way in which experiments were conducted in mind; he simply wanted to find the experimental dispositive that would allow him to discover a reliable marker of difference between the subjects who had been hypnotized and those who had not.⁴ In fact, nothing up to that point in the experimental procedure guaranteed that one was dealing with a subject really under hypnosis, and not with a subject that was simulating it. Every procedure indicating hypnosis was consequently always suspect, since one could never prove that the phenomenon the effects of which one was trying to elucidate was indeed what one claimed to have set up. Orne therefore

despret

considers a situation that can "make a difference": according to him, the capacity to tolerate an annoying task and to do it well over a long period of time and simply because the experimenter had asked for it would clearly create the contrast. Hypnotized people should, in principle, show a very different deference from that of normal subjects.

Orne starts with the test group, composed of non-hypnotized subjects. He asks them to conduct an absolutely absurd, repetitive, and tedious task. This was to resolve some two hundred additions on a sheet of paper and, at the end of this, to fish for a card that would invariably give a directive to tear the completed paper into thirty-two pieces, then to take another calculation paper, resolve the two hundred additions there, take another card that would invariably have a directive to ... It would be the experimenter who, after more than five hours of observation, blinked first. And when the subjects were asked why they did all this work without objecting and without posing other questions, they responded that they had thought that it was a test of endurance. And they obeyed because a scientist asked them to. That is to say that they did not respond to the question that the scientist thought he was asking but to the way in which they interpreted that which was expected of them, in the very particular context of the laboratory.

Now, Orne remarks, if I had asked my secretary to do a fortieth of this task, she would have refused. He continues, if you ask some people in your entourage whether they agree to do you a favor, and on their affirmative response you tell them to do five push-ups, they will respond "why?" If you ask a group of people if they want to take part in a scientific experiment and, after their agreement, you tell them that you expect them to do five push-ups, they will ask "where?" Deference, Orne concludes, evidently cannot constitute the acceptable criterion of difference between hypnotized subjects and "normal" experimental subjects.⁵

In light of what his subjects responded to him, Orne goes further in concluding that the lure utilized to mask the expectations, in

psychology, far from resolving the problem, only complicates it. A simple dispositive suffices to show this: Orne brings the subjects together and tells them what will be asked of them and what they will have to do in the course of the experiment. He carefully describes the protocol and the tasks to execute without telling them more about it than if they were really participating in the experiment, therefore hiding, as is done in these types of situations, the real stakes. He asks them at the beginning of these explanations what, in their view, the psychologist is really looking for: the subjects then formulate very precise and pertinent hypotheses.

This has since been nicely shown by an investigation of that famous experiment of Milgram's that I referenced above. The scientific journalist Ian Parker went to re-interview the subjects who had taken part in the experiment, forty years later. Most of them told him that, if they had played the game, it was precisely because they had understood that the experiment must necessarily have been rigged, since it is clear, according to them, that electrocuting people is not allowed in universities. Certainly, one can always suspect that persons retroactively revisit the story and seek to give themselves a clear conscience by always pretending to have known that it was, as the children say, "only make believe." The fact remains that the arguments make good sense: it would be difficult to imagine sending – with the blessing of a scientist, and under his responsibility – lethal charges to another human in a respected university – to an animal, we should note, it would be a different matter. The people interviewed, on the other hand, proposed explanations that seem convincing to me: some said, for example, that at the moment when the supposed victim screamed in pain they turned worried toward the desk of Milgram and his assistant watching the operations, behind glass, and saw them laughing – or undisturbed. They concluded from this what they should conclude. When Ian Parker asked them why they then continued, and why they said nothing, since they had taken account of the fact that all of this was nothing but a farce, they responded that it

was "*for the sake of science.*"⁶ Since they were asked to ...

The allure of a paradox in this type of research should not be ignored. When psychology inquires into this problematic deference on the part of the subjects, what it covers over or deliberately ignores is that this deference is not an inherent characteristic of humans, it is due to the organization of the research itself. Everything points to the necessity of this: the rigid and constraining protocol, the fact that the scientist distributes expertise in a very asymmetrical manner, a situation close to that of the examination, the supposed or induced ignorance of the subjects, etc. Now, psychology treats deference not as an effect of what it imposes but as an essential characteristic that it acts to counter. Which leads to a paradox: psychologists construct dispositives that give rise to deference and must do everything possible to neutralize it. And, as in every situation with a lure, they are then obliged to keep asking: "but did my subjects really believe me? Did they not nonetheless understand what I was looking for and respond to that very question, without my knowledge?" They also use post-test questionnaires to verify that the subjects have indeed been taken for a ride. Now, and it is Orne who emphasizes this, the subjects, in this case, knowing that the fact of having understood the hypothesis will invalidate their research, prefer to say nothing and to continue pretending to have responded in all naivety – it is what is called the pact of double ignorance, since neither of the two, neither the experimenter, nor the subject, really has the desire to say or to know what is really at play: on either side, this would ruin the experiment. It would thus be much better, concludes Orne, in the experiments, to count on the collaboration of the subjects rather than on their so-called gullibility.

what a maze can mean

In proposing to translate what happens to rats in terms of meanings, I draw here on the very important work of the naturalist Jakob von Uexküll, and his theory, especially that of the

Umwelt. And since we are with the rats here, it is with them that I propose to consider the ways in which this theory could open up for the animal, at least partially, the question of point of view.

Some biographical elements are in order. Jakob von Uexküll was an Estonian naturalist (1864–1944). After studies in biology, he took part in a comparative study of invertebrate physiology. This research led him, contrary to what the practices of the time encouraged doing, to want to enlarge his perspective and to consider the totality of the organism in relation to its environment (*milieu*), an environment that he will define as a concrete or lived one: the *Umwelt*.

The intuition from which this theory departs is to all appearances quite simple: the animal, endowed with sensory organs different from our own, cannot perceive the same world as we do. Bees don't have the same perception of color as we do, we do not perceive scents in the same way that butterflies do, and we are not at all able to sense, as a tick can, the odor of the butyric acid released by the sebaceous follicles of mammals. It is there that the theory will take a courageously original turn, for perception will be defined not as a form of "reception" but as an act of creation: the animal does not perceive passively, it "fills its environment with perceptual objects," it constructs its environment by peopling it with perceptual objects that, from then on, become perceived. In other words, perceptions are not passive, they are the object of an activity by which the animal will perceive them. The activity of perception is above all an activity that confers meaning. Only that which has a meaning is perceived, just as only that which can be perceived, and which is important for the organism, is accorded a meaning.

The *Umwelt*, or lived world of the animal, is above all a world where things are only perceived, on the one hand, because they are captured by particular sensory equipment – the butterfly lives in a world of luminous intensities and of odors, for example – and, on the other hand, to the degree in which they have taken on a meaning. And it is with these meanings

despret

that the animal constructs its perceptual universe. Time, space, place, path, way, house, odor, enemy, each event in the perceived world is an event that "signifies," which is not perceived except in that it signifies – and by that which it signifies – an event that makes of the animal a "lender" of meaning, that is to say a subject. For each perception of meaning, according to Uexküll, implies a subject, just as each subject is defined as that which accords meaning. How do things acquire a meaning? Quite simply, Uexküll responds, through action. The animal never enters into a relation with an object as such. The object is constituted in action; its meaning does not emerge except in relation to action that can be practiced.

Objects are not alone in having meanings accorded to them. Inspiring the work of Konrad Lorenz, in fact, Uexküll will hold that the *Umwelt* is at the same time an environment of relations, that is to say an environment in which beings will take on various meanings for one another. Consequently, if it is perceived, no animal can be neutral in the environment of another; that is to say if it can be accorded a meaning, or if it can be seen to be accorded one. What does a jackdaw mean in the life of a jackdaw, or rather, what does this jackdaw mean in the life of this other jackdaw? This is the question that Lorenz will pose to Tchock, the jackdaw he adopted. It is a strange lure that made this type of research possible: Lorenz himself became the alluring producer of sociality, an enticement for meanings (since lures are frequently required in order to convey the meanings of an animal). By adopting a young jackdaw, Lorenz shows that a human can take on the meaning of "*socius*" and subsequently learn what "*socius*" means in the life of a jackdaw. The jackdaws who live in society have the habit of associating, for their whole lives, with a companion (*socius*) with whom they carry out various activities together. Tchock, who was raised by Lorenz, thus took him for a maternal *socius*. He followed him everywhere and asked him to give him food. He later tried to teach him to fly; but after the failure of his repeated attempts the jackdaw finally gave up and considered Lorenz as an

activity companion, acceptable, certainly, but limited. This original adventure shows us that meanings are not fixed once and for all, flowing from elementary needs of the organism: they are flexible, can apply to other beings, extend to unforeseen situations, change, and even invent and create new relational uses.

It is time now to return to the rats, with the goal of asking them, in accordance with a tradition that I am, however, interrogating, to help me to test a hypothesis: when observing rats, what can produce the activity of translating their behaviors in terms of meanings?

“Thousands of experimental series have been made in the past decades by numerous American scientists,” Uexküll writes, “who tried to determine how soon an animal was able to learn a certain pathway, through requiring widely varied animals to orient themselves in a maze ... They have neither investigated the visual, tactile or scent cues, nor given thought to the application of the coordinate system by the animal – that right and left is a problem in itself has never struck them. Nor have they ever debated the question of the number of paces, because they did not see that in animals, too, the pace may serve as the measure of distance” (“Stroll” 51).

The critique is certainly merited, but its accuracy requires some clarification. The behaviorists, and John Watson in particular, did in fact very much consider the influence of optical, tactile, and olfactory perceptual characteristics. I would not, however, go so far as to say that they examined them. Unless one confuses the term “examine” with that of “neutralize.” For that is indeed what Watson did, in a procedure that, if one were to think of it as resembling an examination, would guide the patient toward a sadistic torturer rather than to their doctor: he removed the rat’s eyes, olfactory bulb, and whiskers, which are essential to the sense of touch in rats, before throwing it into the exploration of the maze. And since the rat no longer wished to run in the maze or go in search of the food payment, he starved him: “he began at once to learn the maze and finally became the usual automaton.”⁷ Of course. All that he proves is that, if we remove the conscience from a

psychologist, he continues to write.⁸ Who has become the automaton in this story?

This falls far short. And it is certainly very far from the universe of meanings. For that matter, this is even further from it since the being issuing from this systematic practice of destruction is no longer, for the psychologist, a rat. If the world had probably lost all meaning for this de-sensed rat, the rat itself had lost all meaning for its experimenter – that is, if it ever had one for him. It is a new organism, reduced to a minimum of its sensations, and who, from this fact, counts for all the others. This is the goal of the procedure: search out the lowest common denominator, the “left-over,” the automaton, the behavior that, from one species to another, will render all organisms commensurable (Burt). And this commensurability, it can be underlined in passing, bears on the criterion par excellence for a society haunted by the idea of production and efficiency (Haraway 43ff.): the time required to run a maze.

All of this, one can see, has nothing to do with the meanings that the maze can take on for the rat. We haven’t learned much of value; it is on this point that Uexküll will resume his critique, in his essay on the theory of meaning: “In this way, American researchers have attempted tirelessly, in thousands of experiments, beginning with white rats, to study the most different kinds of animals in their relations to a maze. The unsatisfying results of these labors, which were conducted with the most precise methods of measurement and the greatest skill in calculation, could have been predicted by anybody who had come to the realization that the tacit assumption that an animal could ever enter into a relationship with an object is false” (Uexküll, *Foray* 139; Despret cites Uexküll, *Mondes animaux* 94).

One can note in passing that this critique finds an echo today in the research on well-being. When we ask chickens about their preferences, Robert Dantzer explains, we generally ask them what effort they are ready to agree to for a particular environment. The bird has use of two keys that it can work with its beak to narrow (with one key) or augment (with the

other) the size of the cage. One can note, he continues, not without a point of irony, that the space the chickens grant themselves differs little from commercial conditions. One could conclude from this therefore that it is the optimum space for chickens. “But this would be a bit hasty and neglect a major epistemological problem, namely that it is not straightforward to enter into the subjective universe of an animal in interrogating it through an experimental dispositive thought up by a human. It is possible that the animal responds in the experimental dispositive on the basis of very different elements, for example the proximity of a congener, but not the representation of the space in itself as such” (Dantzer 99).⁹

To return to the rat: on the basis of what elements does it respond when it is submitted to the demand to traverse the maze? Posing this question goes back to asking what this particular experimental dispositive can mean for a rat. How can this traversing come to be, from the point of view of the rat, that which Uexküll calls a “familiar path”? How do the rats, in pretending to respond to the questions of the behaviorists (in this case, the question is: what is the abstract relation of a being, whatever it may be, that which the behaviorists call an organism, to a neutral object?), respond in fact to another question? For it is indeed this that it concerns: the artefact par excellence. The rats respond to another question than the one the experimenter poses to them. And the experimenter can never suspect this would be the case, simply because he never took into consideration the point of view that the rat could have of the situation.

The problem can be posed differently, on the basis of another supposition, that will allow us to affirm Uexküll’s hypothesis by adding some clarifications to it: why do rats always touch the walls as they go along them? It is this that all those who have been able to observe rats, notably when they invade our houses, have been able to affirm. Responding to this question will give us some indices as to what a “familiar path” can be for a rat. We must, however, reformulate the question, exiting the why of causes and entering into the regime of meanings:

despret

from this perspective, what does a wall (something to run along) mean for a rat? The American biologists who observed them invented a term to characterize rats: they are “haptophiles,” they like to touch. The wall therefore has the meaning of “thing to touch.” But a slightly more complicated hypothesis could make sense of this characteristic (Sullivan 12). Rats developed a particular kinesthetic memory, since the rat must resolve this problem in its everyday activities. And its haptophilia is a response to this problem. In its daily peregrinations that lead it from the nest to different places of exploration that will permit it to feed itself, how can it find the return path? How to memorize the indications, and all the more so since the majority of these are indications that have meaning only for humans – objects, name and number of the street, right, left, or indeed even maps or designs? The rat resolved this problem by mapping its route in another manner. It inscribes the course of its route in its body in the form of lines, curves, and turns, or even roughnesses, textures, sensations of cold or humidity – what do we know about what the body of a rat can sense?

The rat draws, marks, soaks up, in its muscles and on its skin, the map of a lateral landscape. And it is the agreement of this map with the sensations that it will check on the return route that will tell it that it is indeed going the right way, and that the nest will be there, at the precise place where all the sensations will have finished unfolding. The relation to the trace is inverted: it is no longer only a matter of “marking” the places one passes, like rats and many animals do, extending their bodies to the limits of their territory with many doses of odiferous substance, it is also a matter of letting itself be marked by the space, itself organized by the trajectory, and of incorporating the organization.

All this is to say: the maze was built by actively integrating a characteristic of the rat; one could say it is “rattish,” in slightly mimicking Uexküll. But it integrated this characteristic by retranslating it as an abstract characteristic – the dispositive will for that matter apply to a

considerable number of animals, inasmuch as it is the object on which is built an infinite series of comparisons between what will become organisms. Doing this, by effacing the link, the singular accord that can be woven between the rat and the structure that is presented to it, by rendering unthinkable the event of what a maze could constitute for the rat, the experimental dispositive pulls the rug out from any questioning on the subject of the rat, on all that which it could bear witness to regarding what is interesting for it. Since the rat does not respond to the question of learning, he responds to the question of an architecture that constitutes the world for him. Which is altogether different. And which cannot, from the manner in which the things are organized, be predicted.

Of course, we are here in the territory of meaning and points of view. I remain, however, with my question: how does the rat interpret this particular dimension of the experiment that sets her within a question from a human, how does she translate what is expected of her? How does she interpret what is wanted of her, when she is made to run, when she is rewarded, or when she is blinded and each of her sense organs is removed before she is starved? To this question, which is perhaps a contemporary question, at least in the domain of the sciences, Uexküll makes no response. These are the limits of his field of inquiry. For if Uexküll can attribute a subjectivity to the animal, and if the *Umwelt* is also a social environment, it seems to me that his interest is particularly focused on the physical environment and its objects. The “animal’s own world” can, with difficulty, include the human observer as an observer. The “own world” does not appear as a world subject to the double hybridization that requires the interspecific encounter and the crossing of an experimental universe with that of an experience of life. In other words still, even if the “own world” of Uexküll can aspire to “thinking as” another animal, only with difficulty can it envisage a “thinking with” this other animal.¹⁰

Besides, the animals that Uexküll, as a biologist, focused on (ticks, flies, sea urchins), are relatively simple organisms, of whom it could

not be said, to put it somewhat simply, that it is easy to interest them in our problems.

The fact remains that the contrast set up regarding the ways of thinking what a maze can represent maintains its full pertinence, and gives a measure of the cost, in terms of knowledge, of failing to take the animal’s point of view into account. The maze can authorize neither the question of the “familiar path” nor that of the meaning of the wall, still less its meaning as event in the world of a rat. It forbids doing so all the more surely since it is constructed in such a way that this question cannot be opened, since the rat cannot do other than follow along the walls. And when an animal cannot do other than what he is constrained to do, when he does so only because he does not have other possibilities or other choices, then there is a certitude: this has to do with an artefact. At the very least with one artefact.

what does it expect of me?

I closed the preceding section in a somewhat elliptical manner, by affirming that the situation in the maze presents at least one artefact, leaving one to think that there would then be others. One will recall that, regarding the experiments of human psychology, I defined as artefactual the situations where the being who is interrogated responds to a different question than the one the scientist poses to her. But there are many ways of responding to another question: there are therefore as many possibilities of artefacts.

If I broached this problem at the beginning of the book it is for a simple reason: when the question of the artefact is posed – I learned in the course of this research – there is frequently something interesting that opens up as possible. It appeared to me that, most of the time, the hypothesis of the existence of an artefact accompanies the possibility of taking into account the fact that the animal would have a point of view on the situation. Certainly, this possibility can be ignored, can lead to, as Rosenthal pointed out, the will to a larger epistemological sanitization, where the researcher does not take full

measure of what his anxiety prompts in the course of research. When this anxiety, in lieu of expanding the imagination, paralyzes it. I also equally learned to recognize, under the form of the injunction of “more control,” the little red light that announces this paralysis. And I admit to being a bit sad each time that a good opportunity was missed: what I thought to be a promise will not be kept. For there is something promissory each time the anxiety of the artefact is profiled in the sciences that mobilize the beings that respond to it. Consider, then, how the promises weaken and what favors the fact that they can receive, as a response, that which they take on.

In an article evaluating the research on relations between humans and livestock animals, the authors note that the animals can react to the observers. However, they continue, these are not the only elements taken into account by the animal: “Researchers must also consider the animals’ expectations during a test. For example, choice tests measuring animals’ preferences for or aversion to different handling procedures indicated that they could predict which procedure was likely [...] from environmental or human cues.”¹¹ That is to say, and I follow here the authors’ conclusion, that generalization becomes problematic. Each experiment indicates not only the manner in which the animals generally experience the procedures but the way in which each of the animals lives them as a function of the perception that it has of them, as a function of what it expects. One can see that the problem of expectations is here attributed to the subject of the experiment and that it conveys the way in which the animal actively integrates what is expected of it. Certainly, generalization is in that case compromised.

In fact I will go further in affirming that there is no artefact unless there is generalization. If one knows to what specific question this animal here, with perceptible or deducible expectations, in this particular context, responds, then there is no artefact. This clearly does not resolve the problem of generalization.¹² That which, one senses, can just as well open either onto a need for more control

despret

(even if somewhat vain), in virtue of which the researchers get it into their heads to neutralize everything that could permit the animal to interpret what is expected of it; or, in a more fecund manner, onto the fact of becoming interested in how the animal interprets the situation. In the first case, one does not eradicate the artefact – since animals always expect something, therefore always respond to another question; in the second, we subordinate the results to the question: “To what did it respond?”

The way in which the expectations of the animal affect the experiments was well noted by some animal professionals, and some scientists, in research designed to evaluate certain foods for farm animals.¹³ It would seem, when we observe the ways they behave, that animals interpret these dispositives for what they are: exceptional dispositives. But for some of them the term “exceptional” would seem to take on a double meaning: “this is not usual” and “this will not last.” Things become more complicated. In fact, everything about them indicates exceptionality: the time of the experimental dispositive is not the same since it is set within a provisional and short time (five days of testing, corresponding to the work week) while the time of the farm is a time of accumulated memories and experiences. That which is given to the animal as food also falls under the exceptional, since new types of feed will be tested.

Now, if animals eat these types of fodder with less appetite, it is for a very simple reason: because it is not the same thing that they are used to receiving. “From the point of view of the animal, the memory of the food eaten before plays a role, and thus it eats less than in usual circumstances: the results, then, say nothing about the situation but instead about the manner in which the animal interprets the transition. It expects something else, thus what one gives it is not the sole cause involved. This has to do with the effects of the transition because the animals, and it is the animal experts like farmers and shepherds who tell us this, know that this situation will not last. In the same way, when one tries a new dry fodder with a group of cows, and they see that the

group next to them receives fresh grass, they stop eating and think: ‘we too are going to have some of that’. And thus the results of the experiment are dependent on what takes place in the experiment next to it, but no one takes account of the fact that the researches are compartmentalized.”¹⁴ One could not give a better definition of the artefact: animals certainly respond to a question, but it is not the one we pose to them. The humor of the situation is too nice not to be underlined: the researchers compartmentalize the research; the animals do not stop prompting them to decompartmentalize it.¹⁵

At this point in my exposition you could respond to me that all these critiques go against my hypothesis, that things haven’t really changed since what they are directed at is the fact that researchers do not take into account the point of view of the animal. I am going to try to respond.

First of all, I approached the problem from the point of view of the artefact. I recall here that it was in this manner that it was possible to get bearings on the moments when the researchers pose the question from the point of view that the animal can have on the experimental situation. Now, the artefact always constitutes the object of a critique: a critical worry when the researcher ponders her own work; an accusation when another researcher says of the work of a colleague: “you didn’t think of” or, to take back up the terms that Dantzer used, regarding the cages and the chickens, “you have been a bit hasty.” In a certain manner, when Waiblinger affirms that the animal can predict what will be offered to it, or when Meuret suggests that that animals think “we are also going to have some of that,” we are very much within this perspective: animals do not judge an abstract situation, but a situation offered to them as it is offered to them.

Following this, I can confirm that some researchers have crossed the divide that consists in taking into consideration the animals’ points of view on the situations presented to them – but not all researchers. And the critiques are evidence of those, and for those, who have crossed the divide.

Finally, the critique can just as much take the form of anxiety. In this frame, it would mean that the researchers actively and explicitly started to take into account the fact that the animal poses, to her researcher, the question: what do they want of me? When, for example, Meuret describes his own research, his approach seems to me particularly exemplary of this possibility of considering, actively, the manner in which the animal itself actively takes the questions and the presence of the researcher into consideration. Meuret observes sheep and goats and a part of his research consists in evaluating what they eat when we put them in unfamiliar situations such as zones of underbrush clearing (to avoid forest fires). After a first stage of reciprocal habituation between the animals observed and their observers, each researcher on the team follows, each day, an animal and observes what they eat all day. Each detail is carefully noted, each species of plant inventoried, each bite recorded. The proximity is complete, the interest for the observed is unflagging.

The scientific method requires that the animals be chosen randomly, in order to constitute a random sample. But this random choice can turn out to be disastrous, for many reasons. The procedure therefore requires going through a series of steps. As such, the second step is designed “to identify animals within the group that can be monitored uninterruptedly from a very close distance. While alternating movement within the group and close monitoring, the observers look for individuals which seem indifferent to their permanent presence. The full-time presence of an observer automatically changes the social status of an individual. This is why, at the beginning of this step, the individuals to be sought should neither be a leader nor an aspirant leader. Here again, it is important to listen to the herder’s advice if he knows well the social hierarchy within his flock. At the end of this step, about 15–20% of the individuals are considered to meet the prerequisites for full-time close monitoring” (Agreil and Meuret 101–02). In this manner, for certain goats, Meuret explains, the fact of being the object of an intense interest

on the part of the human gives rise among them to forms of conduct such as that of wanting to supplant the others, to take their food, even of seeking fights. For others, being the object of a researcher's attention will provoke the aggression of their companions, as if the observer's interest conveyed a desire on the part of the goat to change its place in the hierarchy. And this introduces a famous disorder into the group. On the other hand, Meuret continues, we no longer really know what we are seeing: what a goat eats in natural conditions, or on the contrary, what a goat eats when it wants to show to others its superiority since, suddenly, it thinks that its status has changed.

Of course, one could always translate the preceding into the shoddiest and most conventional version of the artefact: we influence that which we observe! But if this version seems shoddy to me, and if I oppose this somewhat lazy conclusion with which the systematic theories have pestered our ears, it is only because it reduces the problem to its simplest expression. Because it supposes, once again, that there would be an active, influencing observer, and a passive observed, whose sole activity would be to be influenced. Now, there are many signs that say otherwise, that say that this has to do with beings who negotiate the conditions of research, who mutually affect one another, who exchange judgments and opinions, who reciprocally modify one another and who know that they do it.

Michel Meuret does not speculate on the fact that he influences the goats or the sheep that he observes, he actively asks them to take a position in relation to his proposals and he is in harmony with theirs.¹⁶ He expects, and he expects of the sheep and goats, that they respond to him, contest, and protest. And this implies something other than a simple reflexivity on the question of influence: it demands attention.¹⁷ The concern could be exclusively epistemological, and in fact it is epistemological, but not exclusively. Yes, it is a matter of not disturbing, of not creating an artefact, but there is also a quality of the relation, a concern for the comfort of the animal, a rightness of the relations that transpire as much in

despret

the writings as in what he relates to me. Meuret explains, for example, that at the third step (we had remained a bit with the second) the candidates observed are abandoned if their attitude testifies to "the interest, the anxiety of a discomfort due to the close and constant presence of the observer."¹⁸ Why is interest a bad motive, in this frame? Because the animal must be interested in other things besides the human being, it must continue to live its life as a goat or a sheep. The choice of the "good" animal is founded on a very simple conviction: the animal responds to her observer; and it is what her response indicates that will constitute the criteria permitting the continuation, or not, of the observation. A last remark from a researcher illustrates this in an even clearer manner: "... a good sign to start an observation is when an animal pushes you because you are in the way of what it covets: this says that it is capable of demonstrating that you are bothering it."¹⁹ You want to practice habituation and be certain not to disturb the animals? The solution, as simple as it is, took time to emerge: all you have to do is ask them.



disclosure statement

No potential conflict of interest was reported by the author.

notes

Translated from Vinciane Despret, *Penser comme un rat* © Editions Quae, Versailles, 2009, 8–15, 28–45.

1 Certainly, the investigation conducted in Berlin in 1904 about the case of the famous clever Hans, the horse who knew how to count, could be considered as the start of the critical elucidation of the effect of human expectations on an animal. However, the point of focus of the research, oriented toward the human factor and heavily laden with the mechanism typical of the emerging behaviorism, minimized the point of view that the horse could have on the situation. To put it in terms that will emphasize my process, the horse,

in the perspective adopted, did not “respond” to the expectations but “reacted” to them. On this subject see Despret, *Hans*.

2 One finds an account of his research in Rosenthal, *Experimenter Effects*.

3 The analysis of Rosenthal’s research is contained in the following chapter of Despret’s book, but is not included in this excerpt. [Translator’s note.]

4 The French term *dispositif* has an important specificity that has caused difficulties in prior translation and in capturing the range of meanings that it covers (including technical, military, legal, and ontological/arrangement dimensions). The term is at once an everyday, general term for referring to machines and devices of all kinds (such as cameras and pencil sharpeners but also airplanes) and it is a philosophical concept that has been drawn upon by Gilles Deleuze, Michel Foucault, Louis Althusser, Giorgio Agamben and many others. Owing to the technical connotations of the term, it has often been rendered as “apparatus” in English, but this presents a major problem since the French term *appareil*, much more closely related to “apparatus,” is used as distinct from *dispositif* by the thinkers mentioned. Owing to the specificity of the concepts, there is an increasing use of the English term “dispositive” to capture *dispositif* and the distinctions from *appareil*. Timothy Armstrong’s earlier translation of Deleuze’s famous essay on Foucault’s use of the concept uses “social apparatus” to distinguish it from “apparatus” and to emphasize the social and assembling dimensions. These social and assembling dimensions are particularly important to Despret’s use of the concept in the philosophy of science and ethology. See Gilles Deleuze, “Qu’est-ce qu’un dispositif?” in *Michel Foucault philosophe* (Paris: Seuil, 1989), Giorgio Agamben, *Che cos’è un dispositivo?* (Rome: Nottetempo, 2006), and Jeffrey Bussolini, “What is a Dispositive?,” *Foucault Studies* 10 (2010): 85–107. [Translator’s note.]

5 Since then, the possibility of discriminating between subjects really under hypnosis and subjects simulating it has been able to be staged experimentally. Thus, for example, a hypnotized subject can be convinced that he no longer knows how to read. “Not being able to read” seems impossible to simulate: when we know how to read, in normal conditions, the letters make sense in a fashion that cannot be ignored; we can no longer “not know how to read.” If one presents subjects with

an image representing the word “blue” written in yellow, the non-hypnotized subjects will show a latency time when one asks them the color of the letters, the meaning “blue” interfering with the answer “yellow”; the subjects under hypnosis, for their part, do not demonstrate this latent delay.

6 Parker, “Obedience.” For an analysis of the question of the authority of the scientist, and the way in which subjects actively take account of what is asked of them, to which the present text remains, across the years, profoundly indebted, I refer to Isabelle Stengers, *Invention of Modern Science*.

7 Watson, “Kinaesthetic and Organic Sensations” 2–3; cited in the wonderful little book by the English historian Jonathan Burt, *Rat* (103).

8 For a more extended analysis of what this type of experiment does to the experimenter, see Vinciane Despret, *Quand le loup habitera avec l’agneau*.

9 We find elsewhere, in an article by Isabelle Veisier and Bjorn Forkman, a very clear critique on the ties between different types of defining well-being (and therefore of testing it experimentally) and the different philosophical conceptions that preside over each of these definitions.

10 I would like to thank my philosophy colleagues at the University of Liège, and particularly Julien Piéron and Stéphane Galetic, whose attentive interest, commentaries, and discussions helped me greatly in analyzing the work of von Uexküll.

11 I have referred to this article before in terms of the illumination of the little red light (the models should permit more control): I try here to continue along the lines of interest opened up. Waiblinger 197 for what follows.

12 François Calatayud asks whether it really makes sense to present “to different individuals conditions that one imagines to be equivalent in order to test a hypothesis regarding an average individual.” The notion of meaning, he explains, is incompatible with an average individual, and this is the case, the author continues, even if one is able to bring forward two beings who have “the same usage of the world.” Text from the conference “From natural behavior to the discourse of ethology: Reflections on the place of subjectivity in ethology,” presented at the colloquium organized by Florence Burgat, *Comment penser le comportement animal* (*How to Think Animal Behavior*),

EHESS, Paris, 21–22 Jan. 2008. Burgat, *Comment penser le comportement animal*.

13 In this regard, see the work of Michel Meuret. In addition, Meuret agreed to take part in a long interview with me in June 2008 in which we were able to raise many of the questions that gave rise to this research.

14 In the same interview with Michel Meuret.

15 This humor of the situation appeared to me most clearly in following the work of the consultant clinicians already mentioned (Hellal). Far be it for me to compare animals and humans, but the institutional structures and the type of intelligence that they can give rise to are important. These clinicians based their work on the fact that the teams of social workers confronted with multiple cases of distress, and who frequently work with the same family, but in ignorance of what their colleagues are doing, must learn to follow the compartmentalizations that the families who call them present to them.

16 One recognizes, under this formulation in terms of “propositions,” the mark of the work of Bruno Latour, notably in *The Politics of Nature*.

17 The animal professionals of the Theix Center emphasize that the term “attention” largely overflows the dimension of “well-being.” “Paying attention” is to take care of, but it is also “to mind,” that is to say to pay attention to someone and not to ignore possible disagreement. For example, “paying attention, they say, is to put limits on what one does but also on what the animal does (‘a just environment’ they also say).” This dimension of attention insists on the fact that all research is intrinsically founded on collaboration, whether or not one ignores it, but they do not ignore it and say that they are not able to forget it.

18 Meuret interview with Despret, 2008.

19 Meuret interview.

bibliography

Agreil, Cyril, and Michel Meuret. “An Improved Method for Quantifying Intake Rate and Digestive Behaviour of Ruminants in Diverse and Variable Habitats Using Direct Observation.” *Small Ruminant Research* 54.1 (2004): 99–113. Print.

despret

Burgat, Florence, ed. *Comment penser le comportement animal. Contribution à une critique du réductionnisme*. Paris: EHESS/Quae, 2010. Print.

Burt, Jonathan. *Rat*. London: Reaktion, 2006. Print.

Dantzer, Robert. “Comment les recherches sur la biologie du bien-être animal sont-elles construites?” *Les Animaux d'élevage ont-ils droit au bien-être?* Ed. Florence Burgat and Robert Dantzer. Paris: INRA, 2006. 85–103. Print.

Despret, Vinciane. *Hans, le cheval qui savait compter*. Paris: Les Empêcheurs de penser en rond, 2004. Print.

Despret, Vinciane. *Quand le loup habitera avec l'agneau*. Paris: Les Empêcheurs de penser en rond, 2002. Print.

Haraway, Donna. *Simians, Cyborgs, and Women*. New York: Routledge, 1991. Print.

Hellal, Selma. *De proche en proche. Proximité et travail de réseau en Algérie*. Algiers: Barzakh, 2008. Print.

Latour, Bruno. *The Politics of Nature*. Trans. Catherine Porter. Cambridge, MA: Harvard UP, 2004. Print.

Latour, Bruno. *Politique de la nature*. Paris: La Découverte, 1999. Print.

Orne, M.T. “On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and their Implications.” *American Psychiatrist* 17.11 (1962): 776–83. Print.

Orne, M.T., and C.H. Holland. “On the Ecological Validity of Laboratory Deception.” *International Journal of Psychiatry* 6.4 (1968): 282–93. Print.

Parker, Ian. “Obedience.” *Granta* 71.70 (2000): 101–25. Print.

Rosenthal, Robert. *Experimenter Effects in Behavioral Research*. New York: Appleton, 1966. Print.

Stengers, Isabelle. *The Invention of Modern Science*. Trans. Daniel W. Smith. Minneapolis: U of Minnesota P, 2000. Print.

Stengers, Isabelle. *L'Invention des sciences modernes*. Paris: La Découverte, 2000. Print.

Sullivan, Robert. *Rats: A Year with New York's Most Unwanted Inhabitants*. London: Granta, 2005. Print.

thinking like a rat

Uexküll, Jakob von. *A Foray into the Worlds of Animals and Humans*. Trans. Joseph D. O'Neill. Minneapolis: U of Minnesota P, 2010. Print.

Uexküll, Jakob von. *Mondes animaux et monde humain*. Trans. P. Muller. Paris: Denoël, 1965. Print.

Uexküll, Jakob von. "A Stroll through the Worlds of Animals and Men: A Picture Book of Invisible Worlds." *Instinctive Behavior: The Development of a Modern Concept*. Ed. Claire H. Schiller. New York: International UP, 1957. 5–80. Print.

Veissier, Isabelle, and Bjorn Forkman. "The Nature of Animal Welfare Science." *Annual Review of Biomedical Sciences* 10 (2008): T15–26. Print.

Waiblinger, Susan, et al. "Assessing the Human–Animal Relationship in Farmed Species: A Critical Review." *Applied Animal Behaviour Science* 101.3–4 (2006): 185–242. Print.

Watson, John B. "Kinaesthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze." *Psychological Review: Monograph Supplements* 8.2 (1907): i–101. Web.

Vinciane Despret
Philosophie et Lettres
Université de Liège
4000 Liège
Belgium
E-mail: v.despret@ulg.ac.be

Jeffrey Bussolini
Sociology – Anthropology Department
City University of New York
2800 Victory Boulevard
Staten Island, NY 10314
USA
E-mail: jbussolini@mac.com