

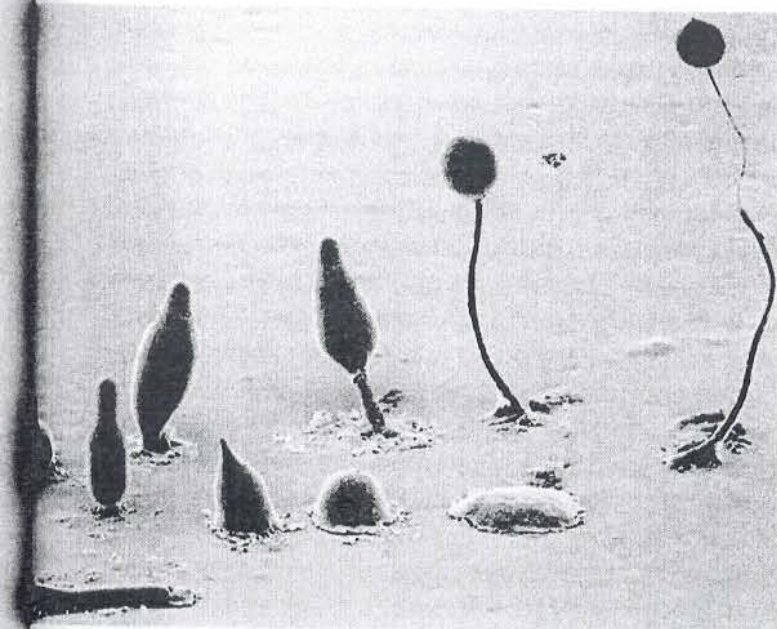
Perceiving is not a matter of passively allowing an organ—say of sight or hearing—to receive a ready-made impression from without, like a palette receiving a spot of paint. Recognizing and remembering are not matters of stirring up old images of past impressions. It is generally agreed that all our impressions are schematically determined from the start. As perceivers we select from all the stimuli falling on our senses only those which interest us, and our interests are governed by a pattern-making tendency, sometimes called *schema*. In a chaos of shifting impressions, each of us constructs a stable world in which objects have recognizable shapes, are located in depth and have permanence. In perceiving we are building, taking some cues and rejecting others. The most acceptable cues are those which fit most easily into the pattern that is being built up. Ambiguous ones tend to be treated as if they harmonized with the rest of the pattern. Discordant ones tend to be rejected. If they are accepted the structure of assumptions has to be modified. As learning proceeds objects are named. Their names then affect the way they are perceived next time: once labeled they are more speedily slotted into pigeon-holes in the future.

As time goes on and experiences pile up, we make a greater and greater investment in our system of labels.

— Mary Douglas, *Purity and Danger*

## SLIME MOLD

Evelyn Fox Keller



**I take an organism as** my object, the lowly amoeba-like protist, *Dictyostelium*. In times of plenty, it lives as an individual single-celled organism but, when food supplies are exhausted, it regroups. Then, this one-celled protist becomes part of a complex multicellular motile slug capable of producing fruiting bodies and spores, and of migrating in search of greener pastures where the new spores can germinate.<sup>1</sup> Over the years, it has attracted a great deal of scientific interest, partly because it so elegantly exemplifies a primitive form of biological development, and partly for the paradoxes it embodies. On the one hand, here is a single-celled organism, existing in a population of apparently identical organisms, and on the other hand, it is a part of a differentiated organism assuming a particular role and structure in the larger entity, the multicellular organism. Here is an object that traffics back and forth both between the one and the many and between sameness and difference. For me, this simple being, in its rich ambivalence, has served as an intellectual touchstone, a sustaining object throughout my academic career. Over and over, my work would confront me with a dilemma and this object would resurface to help, offering itself as a model for an entirely new way to think about it. For me, slime mold has unarguably served as an object-to-think-with.

My first encounter with *Dictyostelium* came in 1968. I was working at Cornell Medical College in New York City, looking for ways to fruitfully apply mathematical methods to biological problems. Lee Segel, an applied mathematician from Rensselaer Polytechnic Institute was visiting Cornell for the year, and we teamed up to tackle a couple of problems that looked as though they might be tractable, among these the problem of slime mold aggregation. The onset of aggregation is the

first visible step in the process that eventually leads to the cellular differentiation observed in the multicellular organism. Prior to aggregation, there is no apparent difference among cells. But once it occurs, aggregation creates a differential environment for the cells, and therefore it could presumably account for subsequent cell differentiation. The problem is, what sets off the aggregation? Is there some hidden-from-view prior difference, a difference that then serves as the trigger for the development of more elaborate, structured, and clearly visible heterogeneity? Does the onset of organization in fact require the existence of such a preexisting "cause"? Most biologists seemed to think so, and they hypothesized such prior structures under the name of "founder cells." Or was it possible that organization might emerge spontaneously, out of the dynamics of the population as a whole? The first possibility held little appeal for Segel and me: first, it only pushed the question of the origin of heterogeneity further back (where did the founder cells come from?); second, we could find no evidence for the existence of such specialized cells. We set out to demonstrate the feasibility of the second possibility—the notion that organization could emerge from the dynamics of the population as a whole.

The model we developed was a highly simplified—in fact, clearly oversimplified—representation of the actual biological case. Like other mathematical models traditionally employed in the natural sciences, it included just enough of the known biological factors to give rise to the essential phenomenon. At its heart, the model demonstrated that no designated initiator, founder cell, or organizer was required for understanding the advent of aggregation in a uniform distribution of previously undifferentiated cells. We showed that clusters of



amoeba would result from the collective dynamics of a population in which a change in external conditions (in this case, depletion of the bacteria that served as their food source) induced a change of state, and indeed, the same change in state, in each individual amoeba. Our account of the onset of differentiation in at least one kind of biological development offered a way to resolve the paradox (how does highly structured difference arise from similarity?) that so sharply divided genetics from embryology. We assumed that it would be of interest to biologists.

But we were wrong—not so much in our model as in our expectations. Biologists, for the most part, showed little interest in our ideas, and despite the absence of evidence, continued to adhere to the belief that founder cells (or pacemakers) were responsible for aggregation. At the time, Segel and I were disappointed and perplexed, but only after ten years had passed did I see how this fact was of interest in itself. What made my new recognition possible was a shift in intellectual mindset and the focus provided by a sharp question posed on the other end of a telephone.

By the early 1980s, I had found a new calling. I had moved my intellectual center of gravity from theoretical physics, molecular biology, and mathematical biology to issues of gender and science. To me, its questions were compelling: How to liberate science from its history of attachment to masculinist ideologies? How to understand the implications of the very different approach to science manifested in work such as that done by the geneticist Barbara McClintock? McClintock had not tried to separate herself from her objects of study—corn cells—to stay more “objective.” She imagined herself more “among them,” herself reduced to their size, perhaps as a way of becoming one of them.<sup>2</sup>

So, in 1981, slime mold aggregation was far from my mind when, one evening, I received a call from Alan

Garfinkel, a recent convert to mathematical biology. He had recently come across the paper Segel and I had written on the subject, but he had not been able to find any follow-up, either in the form of critique of the work or expansion on the ideas. He asked what was going on. He asked if there was a conspiracy. The call took me aback, not because I thought there had or had not been any conspiracy, but because it immediately brought into focus a problem I had been struggling with around the disparity between McClintock's perceptions and those of her colleagues. I had been wondering why accounts of biological processes that brought such explanatory satisfaction to her colleagues had failed to satisfy McClintock. And vice versa? The sharply worded telephone call reminded me that when I had tried to talk to biologists about our model of slime mold aggregation I had experienced that same wall blocking both interest and understanding.

Suddenly, I saw my own experience as an example of a general phenomenon—a widespread disposition to kinds of explanation that posit a single central causal locus (governor, founder cell, pacemaker)—and that this disposition was crucial in understanding the gap between conventional understandings of biological development as DNA driven and McClintock's own more dynamic proposals. Following David Nanney, I referred to such explanations as “master-molecule theories” and began to wonder why it should be that people tend to find such accounts more natural and conceptually simpler than global, interactive accounts in which causal force is distributed.<sup>3</sup> One possible hypothesis that seemed plausible to me was that we tend to project onto nature our first and earliest social experiences, ones in which we feel passive and acted upon. But in any case, I wrote,

As scientists, our mission is to understand and explain natural phenomena, but the words *understand* and *explain* have many different meanings.



In our zealous desire for familiar models of explanation, we risk not noticing the discrepancies between our own predispositions and the range of possibilities inherent in natural phenomena. In short, we risk imposing on nature the very stories we like to hear.<sup>4</sup>

Another twenty years have passed since my work on scientists and their preferred narratives of nature. My intellectual preoccupations have shifted again. For more than a decade I wrote about genetics and developmental biology, and today I find myself turning to developmental psychology. Insofar as my focus has remained on the nature of developmental processes *per se*, the shift has been but a small step, and a new community of intellectual allies was easy to identify. For example, in my work on developmental biology, I had mounted a strong critique of the concept of the "genetic program" (understood as a program for development encoded in the DNA), and I found an entire school of psychology engaging in similar arguments. Its scholars were making strong claims about the value of dynamical systems theory for understanding developmental processes. What drew these psychologists to dynamical systems theory was the language that theory provided for describing the emergence of novel patterns of organization in complex, nonlinear systems, patterns that could not have been predicted from studying the behavior of individual components in isolation.

To me, this had the ring of *déjà vu*, but much had changed over the years since my early foray into dynamical systems. In particular, familiarity with examples of self-organization—in physics, in computer science, and even in biology (where slime mold aggregation has become a canonical example)—now extends beyond these academic communities. One effect of a more common knowledge of examples of self-organization is that, over the last fifteen years, a series of proposals from different

disciplinary quarters urge the reframing of all psychological and biological arguments in terms of dynamical systems.<sup>5</sup>

The word *reframing* here is crucial. Almost all of these proposals suggest alternatives to conventional framings of development in terms of programs (either genetic or developmental), where the very term *program* is seen as implying the unfolding or elaboration of innate capacities. The authors who have taken up the cause of dynamical systems see in this approach an antidote to the prevailing innatism of so much of contemporary writing in biology and cognitive science.

All of these authors seek to redress what they see as the disproportionate emphasis currently placed on internal factors of development; I have enormous sympathy with their concerns. But in this particular intellectual dispute, I found myself jolted out of any comforting alliance by a sharp recollection of slime mold, a call from my past. Slime mold aggregation is illustrative of self-organization in dynamical systems, but does it not equally well illustrate the power of a developmental program embodied in an individual cell? And if it does, then how can it be said that dynamical systems offer an alternative to notions of program? Indeed, I am prompted to ask, can a viable distinction between the two even be made? For me, it cannot. The implications of this heresy are large; they include, for example, the possibility that the battle lines against innatism need, yet again, to be redrawn.

Let us return then to *Dictyostelium*. Our early model was deficient in many ways, yet our central point is still valid: the aggregation of a population of single-celled amoebae (and its subsequent development into multicellular organisms) proceeds spontaneously, without the need for distinctive founder cells; the population emerges as the product of decentralized and local interactions among molecules secreted by individual cells. In other words, despite the elucidation of its genetic



organization, *Dictyostelium* has survived as a simple and compelling model of a self-organizing dynamical system. Given the current state of controversy I return to my touchstone to ask: can we therefore say that there is no developmental program for this organism?

That depends, of course, on what we mean by a program. The most relevant definition given by the *Oxford English Dictionary* has two parts: first, "a definite plan or scheme of an intended proceedings," and second, "an outline or abstract of something to be done."<sup>6</sup> Both suffer, in this context, from an objectionable degree of anthropomorphism. There is no "intention" guiding the development of an organism, nor is there anywhere an agent "doing" the work. Nevertheless, developmental processes proceed along rather well-defined tracks and conclude with quite predictable outcomes. Remarkably little is left to chance in a developing organism—in fact, it might be said that one of the fundamental characteristics of biological development is the capacity to resist the effects of the myriad vicissitudes the growing embryo inevitably encounters.<sup>7</sup> Thus, to the extent that we can rid the notion of program of its anthropomorphic connotations and think of it simply as a plan or scheme of a proceedings with a definite outcome, a plan that need not be located in a particular structure or homunculus but that may instead be distributed throughout the system, I would argue that the very reliability of most forms of biological development demands the existence of a program.

The key idea of a plan or program for reliable development is that the organism (or machine) must be able to resist the disturbances that can throw it off course, either by suppressing or by adapting to them. In other words, such a program must have contingency built into it—instructions, if you will—for how to respond to a range of different kinds of input. In the case of slime mold, the single cell needs to have a change-of-state plan—a plan for changing certain key cell param-

eters when the food runs out. More sophisticated organisms are equipped with programs (or built-in strategies) for changing state in response to changes in a far larger set of parameters. A computer program may not be such a bad image after all, but think of it as a program for survival. Such a program (or strategy) no more requires anticipation than does any other function that has been evolved by the process of natural selection.

The main challenge for the notion of a program located inside the individual cell lies elsewhere: if we are to grant the existence of a program inside the individual (undifferentiated) slime mold cell, that program must not only allow for the change of state in that cell to be triggered by starvation but also for the reproduction of the cell. Without reproduction, there will be no population, and without a population of cells, there can be no aggregation. But after fifty years of work, most of which has been inspired by John von Neumann's early efforts, this problem too seems to have been resolved for programs. Today, programs for reproduction—in virtual even if not in physical space—have become ubiquitous. There are of course still problems, and these problems—primarily having to do with the programs' lack of robustness—are largely responsible for their confinement in virtual space. In this sense, real organisms remain far ahead.

As an object-to-think-with, slime mold has proven to be an immense resource. I am grateful for this opportunity to pay it homage. Some have resisted its message, but there is, too, the danger of overusing it. There are limits to what it has to teach us. The particular route to multicellularity it manifests is, after all, a rather primitive one; furthermore, it bears little resemblance to the developmental process by which most complex organisms come into being. Slime mold may be equipped with a program for adapting to the scarcity of food, but the developmental programs of higher organisms must deal with a far larger range of variability, and evolution has

equipped them with an extraordinary repertoire of ways of adapting to such variability. The world challenges them anew each and every day and in ways that could not possibly be met with a single tool, or even a few, or perhaps not even with a finite number of tools. Slime mold, in its capacity for self-organization, illustrates one strategy for survival, and it is undoubtedly a versatile and fertile object-to-think with. But ultimately more complex living beings find the need of a far larger repertoire of strategies than this little organism can possibly be expected to display.

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## WHAT MAKES AN OBJECT EVOCATIVE?

Sherry Turkle

What makes an object evocative?<sup>1</sup> As I write, *Bodies*, an exhibition of preserved humans from China, is on tour internationally. Its objects, poised between death and new animation, raise questions about the sanctity of what has lived, the nature of art, and the human beings who once were the objects on display. Thinking about the uncanny, about thresholds and boundaries helps us understand these objects with their universal powers of evocation.

And yet, the meaning of even such objects shifts with time, place, and differences among individuals.<sup>2</sup> Some find the preserved bodies the fearsome creatures of night terrors. For others, they seem almost reassuring, an opportunity to contemplate that although death leaves matter inert, a soul may be eternal.

To the question "What makes an object evocative?" this collection offers pointers to theory (presented as epigraphs) and the testimony of its object narratives, voices that speak in most cases about familiar objects—an apple, an instant camera, a rolling pin. One role of theory here is to defamiliarize them. Theory enables us, for example, to explore how everyday objects become part of our inner life: how we use them to extend the reach of our sympathies by bringing the world within.

As theory defamiliarizes objects, objects familiarize theory. The abstract becomes concrete, closer to lived experience. In this essay I highlight the theoretical themes of each of the six parts of this collection (with special emphasis on objects and the inner life) in the hope that theory itself will become an evocative object. That is, I encourage readers to create their own associations,