O P I N I O N — O P I N I O N

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Laws of nature and laws of ecology

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We address the question of whether there are laws in ecology. Although there has been a great deal of recent interest in this topic, much of the relevant debate has been conducted under some common misconceptions about what laws of nature are. Once these misconceptions are cleared up, the case for ecology having laws is much stronger. Indeed, we suggest that the case for laws in ecology is no better or worse than the case for laws in physics.

There has been a great deal of discussion lately on the question of whether biology and ecology have laws (Murray 1992, 1999, Quenette and Gerard 1993, Cooper 1998, Lawton 1999, Turchin 2001). There are a couple of reasons for this question attracting the attention of ecologists and biologists lately. The first is that in recent times a number of candidates have been put forward as laws of ecology, and questions concerning the status of the particular candidates under discussion is of central concern for contemporary ecological theory. Moreover, these questions very naturally invite the broader question of whether ecology is a law-governed disciple at all. Some of the particular candidate laws include the allometries (such as the Kleiber allometry) of macro-ecology (Lawton 1999) and various equations of population dynamics (such as the Malthusian growth equation; Ginzburg 1986, Turchin 2001).

Another reason the question of whether ecology and biology have laws is seen as important is that if, as some suggest, biology and ecology do not have laws, this would set them apart from other sciences like physics. Furthermore, it might be argued that physics is the most successful of all the sciences and this success is due in no small measure to the central role laws of nature play in this discipline. Thus, if biology and ecology do not have laws, it might be further argued that they cannot enjoy the success of physics. More radically, it might even be questioned whether ecology and biology are sciences at all (Murray 1999). Obviously, if there are no laws in biology and ecology this would be bad news for these disciplines and their practitioners.

Fortunately, no such pessimistic conclusions are warranted. The case against laws in ecology is based on some common misconceptions about what laws of nature are and about the role they play in the physical sciences. Of course, giving a complete account of laws of nature is no easy task (which, in part, explains why this important question has been mostly overlooked by those involved in the debate so far). In this note we say a little about what laws of nature are – or, rather, about what they are not – and the role they play in physics. Only then can we begin to answer the question of whether biology and ecology have laws.

Let us begin by clearing up a few misconceptions about laws and the role they play in science, especially physics. Our discussion here is especially influenced by Armstrong (1983), van Fraassen (1989) and Chalmers (1999).

The first misconception about laws is that they must be exceptionless. But this is far too strong; if we require laws to be exceptionless, there are no, or very few, laws – even in physics. Galileo's law that all massive bodies fall with constant acceleration irrespective of their mass has many exceptions: snowflakes fall quite differently from hailstones and with radically different accelerations. Or consider the law of conservation of kinetic energy: the kinetic energy of a closed system is constant. In particular, consider the collision of two billiard balls. The kinetic energy of the system, according to the law in question, will be the same after the collision as before. But this is not the case; the kinetic energy of the system after the collision is always slightly less than the kinetic energy before. Or consider Kepler's first law, which states that all planets travel in ellipses with the sun at one foci of the ellipse. Not only does this law have exceptions, *every* planet is an exception. The orbit of any planet is *approximately* an ellipse but because of all sorts of disturbing factors (such as gravitational influences from other planets and changes in mass of the planet and the sun) it is not *exactly* an ellipse. The point is that if laws are supposed to be exceptionless, it would seem that there are no laws. Indeed, some philosophers of science (van Fraassen 1980, 1989 and Cartwright 1983, 1999) have been somewhat deflationary about the role of laws in physics because of considerations such as these.

Now it's not too difficult to give an account of why the laws above fail: we've neglected to account for the effects of air resistance in the case of the snow flakes and hailstones; we've neglected to account for the fact that billiard-ball collisions are *not* perfectly elastic; and we've neglected to provide an account of disturbing factors in planetary motions. This suggests that the view of laws of nature as exceptionless can be salvaged if we simply limit the scope of the laws in question. So instead of the standard statement of the law of conservation of kinetic energy, we limit it to cases of perfectly elastic collisions. Now the law has no exceptions but it also fails to be of any use. It is of no use for the simple reason that *there are no perfectly elastic collisions*. A law, thus construed, tells us nothing about the kinetic energy of billiard balls and the like. In particular, it fails to account for why billiard balls *almost* conserve kinetic energy in their collisions.

The appeal to idealised setups such as frictionless free fall, perfectly elastic collisions, and two-body problems seems to be on the right track, though. How such idealisations are to be used in articulating laws of nature is a contentious issue, but it is clear that something like them is needed. Perhaps, as some suggest, laws of nature describe the dispositions physical systems have to behave in certain ways in these idealised setups; in real setups the physical systems have the same tendencies but the behaviour is slightly different because of the interaction of several different tendencies. What is clear, however, is that idealisations are important for our articulation and understanding of laws of nature. In any case, laws of nature (if there are any) are not exceptionless; that's all we're claiming here.

The next misconception is that laws should make precise predictions. Or as Popperians are fond of putting it: laws should be *falsifiable*. The idea is that the law in question L should make some very specific prediction P about what will happen in some set up S. If, in circumstances S, we observe P, then L is (provisionally) confirmed (or at least it lives to be falsified another day); if, in circumstances S we do not observe P, then L has been falsified and should be rejected. According to this simple falsificationist line, what distinguishes science from non- (or pseudo-) science, like astrology, is that the former but not the latter is falsifiable.

It would take us too far afield to rehearse the many (and in our view, decisive) objections to the simple falsificationist account of science. Suffice to say that this model fails to account for the holistic nature of confirmation (and disconfirmation), and it finds few supporters among modern philosophers of science. As Quine puts it "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine 1980, p. 41). This point was made long ago by Duhem (1954), and more recently by Quine (1980, 1995) and Lakatos (1970). Once we appreciate this basic point about the logic of scientific methodology, it turns out that no hypothesis (or law) is strictly falsifiable in the simple falsificationist sense, because we can always make adjustments elsewhere in the theory (in what Lakatos called 'the auxiliary hypotheses') to accommodate recalcitrant data.

Think of the way in which Newton's law of gravitation was saved from falsification in light of the aberrant behaviour of the orbit of Uranus. The auxiliary hypothesis adjusted was the one concerning the number of planets (at the time, thought to be seven). Once an eighth planet (Neptune) with suitable mass and orbit was posited, Newton's law of gravitation was saved from falsification. Not only was the theory saved from falsification, the discovery of Neptune was taken by most commentators as one of the great achievements of Newton's theory. But the simple falsificationist view has a hard time accounting for such episodes. For, according to one reading of the simple falsificationists view, Newton's law was falsified by the orbit of Uranus and that should have been that. The law should have been rejected. On another reading, Newton's law was not falsified because it could be protected from impending falsification by making suitable adjustments elsewhere. But such adjusting is an option for protecting any law, so it's hard to see how any law could be falsified.

The point we're making here is simply that a single law typically does not make specific predictions on its own; a great deal of extra theory and facts about initial conditions are required to make any predictions at all, let alone precise predictions. So, for example, while Newtonian gravitational theory makes some rather precise predictions about Halley's comet, say, it makes much poorer predictions about the trajectories of the smaller asteroids in the asteroid belt (because the latter involves knowing a solution to the intractable N-body problem). While there's no denying that predictive power in a theory is a virtue, it should not be seen as the sole responsibility of the laws to provide this. Indeed, the unreasonable attention given to predictive power by some scientists and philosophers seems to be a hangover from more naive empiricist philosophies of science. Modern commentators of science have paid due attention to the role of other theoretical virtues like simplicity and elegance (Quine 1976, Fagerström 1987).

The final misconception is that laws cannot be mere regularities. There a number of ways to try to distinguish laws from regularities. The first is to expect laws to distinguish cause and effect. For example, consider the regularity between wearing a seat-belt and surviving serious motor vehicle accidents. We take it that it's obvious that it's the seat-belt restraining the occupant of the vehicle that *causes* the survival (or, alternatively, the lack of a seat belt that causes the fatalities). But simply stating a regularity between seat-belt wearing and survival does not make it a law. According to this line of thought, laws must distinguish cause and effect, or determine whether both events are the result of a common cause.

But this is mistaken. First, it's not clear that there is any role for causation in our most fundamental physics (e.g. quantum mechanics). But in any case, there seem to be many laws in physics that simply state correlations without distinguishing cause from effect, or even talking about causation. Kepler's laws are perfect examples of such laws. Or consider the various conservation laws, such as conservation of mass/energy, in physics. Such laws are central to physics and yet there is no mention of what causes the quantity in question to be conserved.

Another way of distinguishing laws from mere regularities might appeal to explanatory power. The suggestion is that laws, but not mere regularities, are explanatory. That is, we assume that appeal to a law will explain the regularity of the events in question. So, for example, Newton's law of gravitation does not merely *predict* the gravitational pull of the Earth on the Moon, it *explains* it. This line of thought is hard to sustain, though. We all know that explanation must end somewhere, and typically it ends with the laws of nature. In a very important sense then, such laws do not explain anything—they merely state the fundamental assumptions of the theory.

Reconsider our earlier billiard-ball example. If two billiard balls of the same mass collided such that before the collision one is moving and the other is stationary and after the collision the first is stationary and the second is moving, why is it that the velocity of the second ball after the collision is the same as the velocity of the first before the collision? Because of the conservation of kinetic energy. The relevant law does seem to explain. But this appearance is only superficial. The law of conservation of kinetic energy really just describes the situation; we are none the wiser as to why the two velocities are the same after hearing the story about the conservation of kinetic energy. To see this point from a slightly different perspective, consider the question: why is kinetic energy conserved? We really don't have an explanation of the billiard ball velocities until we have an adequate explanation of the conservation of kinetic energy. It seems, then, that fundamental laws need not be explanatory – indeed, it seems that fundamental laws of nature are an appropriate place for explanation to stop and so *cannot* be explanatory (at least, if explanation is thought of in this foundational way).

We take the above discussion to show that whatever laws of nature are, we should not expect them to be exceptionless, we should not expect them (in general) to be explanatory or distinguish cause and effect, and we should not expect them to always be predictive. This is not to say that they never have any of these features. Indeed, we might even prefer laws that do have some or all of these features. Our point is simply that these cannot be necessary conditions for being a law. Although we stress that it does not follow from this discussion that *any* regularity counts as a law of nature.

Our account, thus far, has been entirely negative – we've said what you should not expect of laws of nature – but we have not said what laws of nature are. To provide a positive account, however, is a substantial undertaking, and one that we cannot hope to do justice to here. Moreover, we are not really in a position to offer a positive account of laws of nature. There are, after all, many accounts in the literature (we've discussed some of these above), but the definitive account remains elusive. But not having a positive account of laws of nature is not important for present purposes. The negative characterisation we've given suffices. Our strategy, after all, is to argue that the standard arguments for the lawlessness of ecology are defective because they presuppose certain unrealistic accounts of laws. What the correct account of laws should be is not important. It is more important to appreciate what laws are not. The interested reader is referred to Armstrong (1983), Chalmers (1999) and van Fraassen (1989), for the various accounts of laws of nature (and their respective shortcomings).

Now that we have a clearer picture of laws of nature in general, let's return to the question of whether there are laws in biology and ecology. We will focus on ecology, because the case for laws in ecology is generally thought to be weaker, since ecology lacks a grand, widely-accepted, explanatory theory such as Darwinian evolution.

It seems that a great deal of the dissatisfaction with the candidate laws in ecology is that they are not exceptionless. Most laws in ecology are fairly inaccurate in the sense that they have many exceptions or they only hold approximately. Consider, for example, the Kleiber allometry: basal metabolism rate is proportional to a 3/4 power of body weight (Calder 1996). The relationship claimed here, although the most accurate of all the known allometries, is only approximate (most organisms do not *strictly* obey this law). But why should such inaccuracies rule this out as a candidate for a law of ecology? After all, we've already seen that most laws fail to be exceptionless and it is also very common for laws to hold only in idealised situations. But this is precisely the case with the Kleiber allometry.

Now we're not claiming that the Kleiber allometry *is* a law of ecology – just that it is a good candidate. At least, the fact that it only approximately holds should not exclude it as a candidate for a law.

It might also be objected that the Klieber allometry just states a regularity between metabolism and body weight and until the reason for this relationship is known, the allometry cannot count as a law. But this is to insist on a law having explanatory power and we've already argued that this is expecting too much. Consider, for example, Kepler's third law that the square of the period of a planet's orbit is proportional to the cube of the length of half the major axis of its orbit. Although the reason for this relationship was eventually given by Newtonian gravitational theory (Kepler's laws can be derived from Newton's theory), at the time of Kepler, there was no reason given for why Kepler's third law held. Moreover, even taking account of the explanation for this law given by Newton, it might be argued that until there's an explanation of the inverse-square relationship in Newton's law of gravitation, the relationship articulated in Kepler's third law has not been explained. So even in physics, laws do not need to explain the relationships they describe, so we should not insist on this in ecology. (Although there is a sense, in which other sciences, including ecology, should be held to a higher standard than physics in this regard. The idea is that it might be appropriate for explanation to stop in a fundamental science like physics, but ecology is not a fundamental science and so explanation should be more highly valued here. This issue is discussed in more detail in Colyvan and Ginzburg 2003.) Indeed, the Kleiber allometry seems very much like one of Kepler's laws; it holds (approximately) but there's no account given as to why it holds. Of course an explanation of why the Kleiber allometry holds is highly desirable (and there's a great deal of work devoted to this topic), but the absence of such an explanation does not rule the allometry out as a candidate for a law.

Finally, we note that it is often objected that ecological laws like the law of Malthusian growth do not count as genuine laws because they are not predictive. The future abundance of a population is notoriously difficult to determine, in part because of the many complicating factors that impact on population growth. But we've already seen that many of the laws of physics are not predictive and they are often not predictive for similar reasons. In short, lack of predictive power is not a good reason to deny that the law of Malthusian growth is a genuine law. Indeed, it has been noted that Malthuss law acts in an analogous manner to Newton's first law: the law of inertia (Ginzburg 1986, Ginzburg and Colyvan 2003, Turchin 2001). Both these laws describe what happens in the absence of disturbing factors (i.e. when there are no mechanical forces or "biological forces" respectively).

So to sum up this discussion, we believe that there are good candidates for laws in ecology. On this issue we find ourselves in broad agreement with Turchin (2001, 2003) and Mikkelson (2003), who too suggest that the case against ecological laws is based on some questionable philosophical assumptions. (Of course, there is still disagreement over the details of what the laws are – see Berryman 1999, Ginzburg 1986 and Turchin 2003 for different accounts of the laws of population ecology.) We have argued that those who deny there are laws in ecology have a somewhat unrealistic account of what laws of nature are and how they operate in the rest of science. Once we rectify these misconceptions, there are no good reasons to deny that ecology has laws. At the very least, ecology and physics seem to be in the same boat in this regard. They both have laws that typically have exceptions, are not necessarily explanatory, may not be predictive, and often invoke idealised situations.

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