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SCIENTIFIC LAWS AND CAUSALITY IN THE PHILOSOPHY
OF SECOND LANGUAGE ACQUISITION: A NEO-ARISTOTELIAN APPROACH

BY

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THESIS

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ABSTRACT

The field of second language acquisition (SLA), since its inception, has been strongly influenced by a cognitivist tradition that stretches back to Descartes. This cognitivist influence is particularly visible in the application of Chomsky's Universal Grammar to SLA (e.g., White, 1989, 2003, 2007) and in Doughty and Long's (2003) call for SLA to abide under the same disciplinary roof as cognitive science (see also Gregg, 2003). But cognitivism in SLA has not gone unchallenged. Block (1996), for example, argued for a poorly defined version of relativism in SLA and was convincingly refuted by Gregg, Long, Jordan, and Beretta (1997). Others offering alternatives to cognitivism, however, have been more successful. Among them, Firth & Wagner (1997) set off a rousing debate concerning the scope, key concepts, and *explananda* of SLA and were promptly misunderstood by prominent cognitivist SLA researchers (e.g., Long, 1997, and Gass, 1998). This failure to communicate, on both sides, significantly resembles the notion of incommensurability developed by Kuhn (1962/1996): researchers working from different paradigms, or research traditions, inevitably talk past each other because they have no rational basis for evaluating the claims of the other side.

The present work suggests a potential remedy for the divide between cognitivist and social SLA. First of all, SLA research is often pursued with little regard for its philosophical heritage. The only major effort to situate SLA within the history of modern philosophy (Jordan, 2004) begins in the seventeenth century with Descartes and accepts the early modern denunciation of Aristotelianism without reservation. The recent neo-Aristotelian revival in philosophy, however, indicates that such an approach is problematic. Particularly in metaphysics and philosophy of science, neo-Aristotelian researchers have shown that key Aristotelian concepts – in particular natures, capacities, and final causes – ought to be reconsidered (e.g.,

volumes edited by Feser, 2013, Groff & Greco, 2013, Marmodoro, 2010, and Tahko, 2012). Drawing on Cartwright (1999), the present work suggests that neo-Aristotelian philosophy of science provides a way to account for the different emphases of cognitivist and social SLA, particularly conversation analysis (CA) as used in SLA research. In Cartwright's neo-Aristotelian terms, cognitivist SLA searches for scientific laws *ceteris paribus*, in order to uncover humans' *capacity* for language acquisition. CA, as an example of social SLA, investigates the phenomena of language learning *ceteris non paribus*. This approach, motivated by neo-Aristotelian notions of causality and scientific laws, provides an alternate way to conceptualize the discipline and account for the differences between cognitivist and social SLA.

To my wife

Non nobis, Domine, non nobis, sed nomini tuo da gloriam.

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Undertaking such a project would have never occurred to me were it not for a class on philosophy of science that I took with Stephen Barnett at Bryan College (spring 2009). Though I was an English major with no background in science beyond high school physics, he made the subject not only accessible but fascinating as we moved rapidly from Aristotle's physics to the Copernican revolution to Thomas Kuhn to quantum mechanics. Ever since, I have considered him a model of intellectual rigor and personal integrity, both inside and outside the classroom.

Another event directly responsible for this project is the class on second language acquisition that I took with Numa Markee in spring 2012. When I recognized familiar territory in the philosophy of science as I became acquainted with the SLA theory construction wars of the mid-1990s, I began to consider writing a thesis in the area. Prof. Markee not only encouraged me to pursue this project but has also provided invaluable guidance and feedback as it took shape, particularly over the past year.

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Finally, I have greatly benefited from the insightful comments of my committee: Numa Markee (as already mentioned), Peter Lasersohn, and Kirk Sanders. Needless to say, any errors great or small that remain are entirely my own.

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CHAPTER 1

INTRODUCTION

Since its inception, research in second language acquisition (SLA) has been influenced by a variety of other fields and research methodologies: generative linguistics, sociolinguistics, ethnomethodology, sociocultural theory, etc. The variety of approaches used by various researchers in SLA has led to consternation about the unity of the field, most notably in the theory wars of the early to mid-1990s (e.g., Beretta, 1991; Block, 1996; Gregg, 1993; Gregg, Long, Beretta, & Jordan, 1997; Long, 1990, 1993) and in the so-called “social turn” (Markee, 1994; Firth & Wagner, 1997, and their critics, in particular Long, 1997, and Gass, 1998). Those on the cognitivist side of the aisle have argued emphatically that the unifying principle of SLA is its status as a cognitivist discipline and have even gone so far as to claim that SLA research, for maximum efficiency, should be carried out under the aegis of cognitive science (Doughty & Long, 2003; Gregg, 2003). Others, however, critique cognitivist SLA in various ways and prefer different ways of thinking about the nature of cognition, learning, and other key concepts in SLA (see Chapter 3).

A significant part of the disconnect between cognitivist SLA and its alternatives, I argue, stems from the relative infrequency with which SLA researchers take into account the history of modern science and their own metaphysical presuppositions. In Chapter 2, I follow the concepts in the philosophy of science relevant to SLA, beginning with Aristotle, proceeding to the modern thinkers Bacon, Descartes, and Hume, and ending with the postmodernism of Kuhn. In Chapter 3, I sketch how these concepts appeared in the history of SLA as it developed out of Chomsky’s Universal Grammar into a field in its own right through the social turn of the late 1990s.

In Chapter 4, I turn to neo-Aristotelian philosophy of science, which bears some explanation here. As the name suggests, neo-Aristotelianism is a recent revival of certain Aristotelian concepts that – its partisans maintain – ought to be reconsidered after they were jettisoned by the early modern contempt for all things Scholastic. Neo-Aristotelian work ranges from epistemology, metaphysics, ethics, and the philosophy of science (for an introduction to this literature, see the volume edited by Groff and Greco, 2013; for metaphysics, see Molnar, 2003, and Oderberg, 2007, as well as volumes edited by Feser, 2013, Marmodoro, 2010, and Tahko, 2012; for the philosophy of science, see Cartwright, 1989, 1992, 1999, 2007, 2009, Cartwright and Pemberton, 2013, Chakravartty, 2013, B. Ellis, 2002, and Pemberton, 2011). As yet, however, neo-Aristotelian work in the philosophy of science has been confined primarily to physics and economics and has not addressed SLA at all. In the present work, in addition to calling for increased metaphysical awareness on the part of SLA researchers, I argue that a particular neo-Aristotelian approach to scientific laws (that of Cartwright, 1999) may offer a way to conceptually unify cognitivist SLA and social approaches to SLA, particularly conversation analysis (CA).

CHAPTER 2

METAPHYSICS AND PHILOSOPHY OF SCIENCE: ARISTOTLE TO KUHN

ARISTOTELIAN METAPHYSICS

In the following account of Aristotelian metaphysics, I follow Feser's (2009) lucid overview (for a general introduction to Aristotle, see Shields, 2007; for Aristotle on teleology, see Gotthelf, 2012, Johnson, 2005, Leunissen, 2010, and Quarantotto, 2005; the standard English translation is Barnes, 1984). The most basic concept is Aristotle's distinction between act and potency. In response to the ancient Greek philosopher Parmenides, who had argued for the impossibility of change, Aristotle attacked Parmenides' assumption that "the only possible candidate for a source of change in a being is non-being or nothing, which (of course) is no source at all" (Feser, 2009, p. 10). A block of wood, for example, is rectangular and solid and of a certain weight. On the other hand, it is not soft, or blue, or as light as a feather. Aristotle observed that in addition to what it is (being) and what it is not (non-being), there is a third category: potentiality. The block of wood is "in act" in the ways mentioned above, but it also has the potential to, say, be burned into a pile of ashes, or to be cut up into smaller pieces. As Feser notes, it is important to keep in mind that these potentialities are circumscribed by the nature of the object in question; it would be facetious to object that it is logically plausible, in some possible world, for the block of wood to become almost anything at all. In other words, "the potentialities Aristotle and Aquinas have in mind are ones rooted in a thing's nature as it actually exists, and do not include just anything it might 'possibly' do in some expanded sense involving our powers of conception" (p. 11). These concepts are easily applied to language acquisition: a block of wood, for example, does not have the potential to learn a language even under conditions that would otherwise be considered optimal. A one-year-old human, on the

other hand, does have the potential, and will acquire language given the appropriate environment. Finally, according to Aristotle, potency can be actualized only by something already in act. The wood cannot become ash by itself; it requires heat. In terms of language acquisition, the human child's potential for language acquisition can be actualized only by another human's already actualized capacity for language acquisition.

Another key component of Aristotelian metaphysics is the doctrine of the four causes: formal, material, efficient, and final. More precisely, the four causes are the “four modes into which cause falls”: τὴν ὕλην, τὸ εἶδος, τὸ κινῆσαν, τὸ οὐ ἕνεκα (tēn hūlēn, to eidos, to kīnēsan, to hou eneka), or “the matter, the form, the mover, the for the sake of which” (Johnson, 2005, p. 42; see Aristotle, *Physics* 2.7, 198^a21-8). When discussing them, it is essential to leave behind our ideas about causation derived from post-Aristotelian, particularly modern, philosophical discourse (e.g., that causes are events) (Johnson, 2005). Rather than a block of wood, take, for example, a wooden chair. The material cause is what the chair is made of: wood. The formal cause of the chair is the form, or shape, it has: four legs, a seat, and a back. The efficient cause is what brought it into being: a carpenter. The chair's final cause, or its teleology, is its purpose: to provide a place for someone to sit. But as Feser (2009) notes, final cause is not the same as function, as we might say a metal part has a certain function in a machine. Final causality has to do with goal-directedness, or the tendency of some object “to produce some particular effect or range of effects” (Feser, 2009, p. 17). Just like potency is circumscribed by an object's nature – the wood has the potential to become ash, but not to become a bird – so an object's final causality is intrinsically connected with its nature, with what it *is*. My car's final cause, its purpose, is to transport me and any passengers I might have to a chosen destination. If my car hits a deer, that does not mean my car's final causality has anything to do with reducing the deer

population. Not everything, however, is explicable in terms of final cause (Johnson, 2005; see Aristotle, *Generation of Animals* 5.1, 778^A30-^B1). In contrast to the Humean approach to causality (see below, p. 8), the Aristotelian notion of efficient cause is part of a taxonomy of explanation that includes the notion of final cause. Aristotelian metaphysics precludes the Humean view that causation is mere regularity, because for the Aristotelian, causes are *things* rather than merely successive events.

DESCARTES AND BACON

Jordan (2004) begins his historical overview of the philosophy of science at the early modern period, with René Descartes and Francis Bacon. Given that Jordan uncritically accepts the early modern denunciation of Aristotelianism, such an approach is perfectly reasonable, though regrettably myopic. Since modern science, including SLA, owes so much to Bacon and Descartes that it is appropriate to follow Jordan in addressing what is relatively familiar (Cartesian induction and Baconian induction) in detail before proceeding to what is relatively unfamiliar (neo-Aristotelianism).

In the second chapter of the *Meditations*, Descartes (1641/1990) uses the example of a piece of wax to establish the mind's centrality to human experience. When just removed from a beehive, the wax has certain attributes: hardness, coldness, a slight smell of flowers, and so on. Near the fire, however, the wax loses those attributes as it melts due to the heat: it becomes soft, too hot to touch, and odorless. From this, Descartes argues that it is his mind alone which is able to perceive both its forms as wax: "the perception I have of it is a case not of vision or touch or imagination – nor has it ever been, despite previous appearances – but of purely mental scrutiny" (Descartes 1641/2013, p. 43).¹ In the *Discourse on Method* (1637/1968), he attempts to doubt everything in order to obtain certainty. But even the attempt to doubt everything left him with

one thing which he thought could not be doubted: himself, the one whose mind was engaged in doubt. In other words, *cogito ergo sum* (I think, therefore I am).

Descartes here emphasizes the use of reason and deduction from first principles to arrive at knowledge. His glorification of the mind and his conception of it as completely divided from the material world eventually became one of the key doctrines of cognitivism as identified by Atkinson (2011): substance dualism. Of course, if mind and matter are completely distinct *kinds* of things, then in what way do they interact? This “mind-body problem,” though philosophers today generally accept neither Descartes’ way of arriving at it or his way of solving it, has not gone away. Another of Atkinson’s cognitivist doctrines, or subject-object dualism – though he does not explicitly make the connection – may also be traced to Descartes’ substance dualism. Just as Descartes thought he could arrive at a certain premise – the existence of his own mind – by the means of his own mind, so modern science views the mind – the object of study – as having “nothing directly to do with the researching subject” (Atkinson, p. 5). (See below, Chapter 3, for the Cartesian tendencies of Chomsky’s Universal Grammar.)

But Descartes also has something to say about causation and scientific explanation. Though he is a theist and considers God the source and end of rational inquiry (Clatterbaugh, 1999, p. 48), the *cogito* exhibits his departure from his medieval predecessors: his philosophical point of departure is not proving the existence of God, as Aquinas does in the *Summa*, but instead establishing the individual’s epistemological and rational capacity. Descartes’ views on causation are nuanced and not always open to straightforward interpretation. But since our primary interest is in their relevance for modern science, Clatterbaugh’s summary of them will be sufficient. Descartes affirms that “causal explanation is revealed by explanatory deductions which proceed from (efficient) causes to their effects” and that “there is a pure deductive order

which begins with clear and distinct ideas and terminates in observable effects, but that order to the extent to which it can be known is known after the effects are known and not prior” (p. 58). Clatterbaugh argues persuasively, contrary to some interpretations, that God is significant for Descartes’ theory of causation. On the other hand, as mentioned above, the fact that God is included by Descartes does not mean that God’s role is as central in Descartes’ system as God’s role is, for example, in Thomism. In fact, Descartes’ emphasis on efficient causes, combined with Bacon’s and Hume’s (see the following section), laid the groundwork for the mechanical philosophy and the so-called Scientific Revolution.

Bacon’s empiricism is another crucial piece of the puzzle. Unlike Descartes, whose rationalism begins with deduction, he developed a different approach: empiricism, which shuns deduction and relies instead on knowledge derived through the senses (and laws formulated through induction). According to Jordan (2004), “Bacon attacked the triviality of medieval theologians and philosophers and, in common with Descartes after him, Bacon insisted that the crucial issue was that of reliable knowledge. Bacon criticised medieval philosophers for their preoccupation with petty scholastic squabbles (famously, how many angels could dance on the head of a pin) and blamed their silly thinking on the blind tradition . . . of giving pre-eminence to logical deduction” (pp. 20-21). While accurate in the main, this account exhibits a common anti-Aristotelian bias. Medieval philosophers, heavily influenced by Aristotle, were also concerned about reliable knowledge, though they approached the problem differently than the early modern thinkers did. Also, it is not clear whether any medieval writer asked how many angels could dance on the head of a pin; the first references seem to be in anti-Scholastic works.

But anti-Aristotelian polemics aside, Bacon did in fact emphasize induction from empirical observation as a methodology. Jordan (2004) summarizes Bacon’s contribution in this

way: “Bacon proposed that empirical observation and formal experiments should take the place of scholastic conundrums, and become recognised as the real business of science” (p. 21). But this too needs qualification, as does the facile attribution of empiricism to Bacon at the beginning of this paragraph. In fact, Bacon owed much more to the Scholastics (and therefore to Aristotle) than many of his later interpreters, and even he himself, realized (Dijksterhuis, 1961). He aimed at a synthesis of reason and sense experience rather than simply relying on the latter and rejecting the former. In any case, however, he is rightly known for being an early and influential advocate of the experimental method, which is based firmly on evidence derived from the senses and is opposed to the Aristotelian preference for “unaided observation and everyday experience” (Arabatzis, 2008, p. 159).

HUME AND CAUSATION

The extent to which David Hume (1748/1999) has influenced the practice of modern science is indicated by this remark by Jordan (2004): “In fact, if one actually stuck to such a strictly empirical programme [as Bacon’s], it would be impossible to arrive at any general theory (there is no logical way to derive generalisations from facts)” (p. 21). The claim in parentheses derives from Hume’s view of causation, which he argues for in the following way. “All reasonings concerning matter of fact,” he says, “seem to be founded on the relation of Cause and Effect. By means of that relation alone we can go beyond the evidence of our memory and senses,” because there is a “connection” between the fact and the inference from it (p. 109). But according to Hume, this connection derives from experience alone rather than from any *a priori* reasoning, since “the effect is totally different from the cause, and consequently can never be discovered in it” (p. 111). Moreover, we can always conceive of the usual effect *not* following from (what we take to be) its cause. He offers a thought experiment involving a billiard ball:

since many different outcomes are logically possible, *a priori* reasoning cannot help us adjudicate between them. Only experience – whether one has previously witnessed similar scenarios or, like Adam in the Garden of Eden (an example used elsewhere by Hume), must wait for the outcome – is a reliable indicator. In other words, contrary to Aristotelianism, there are no innate natures, no powers, no final causes.

But then Hume turns to examine experience in greater detail. If I know that eating bread yesterday gave my body nourishment, why should I suppose that eating bread today will do the same? Hume claims that our implicit reasoning is as follows: “I have found that such an object has always been attended with such an effect, and I foresee, that other objects, which are, in appearance, similar, will be attended with similar effects” (p. 114). But, according to Hume, it is impossible to argue from the first to the second. Neither “demonstrative” reasoning – “that concerning relations of ideas” – or “moral” (or probable) reasoning – “that concerning matter of fact and existence” (115) will suffice. The former is insufficient because it is not logically contradictory to suppose, for example, that if I ate bread yesterday and derived nourishment from it, I could eat a similar object today and not derive nourishment from it. The latter is insufficient because invoking facts necessarily involves relying upon past experience, and thus begs the question.

Having demolished – or so he thinks – our common-sense notion of causation, Hume provides an alternative. I do not learn to expect pain, for example, after stubbing my toe because stubbing my toe *causes* pain; rather, I associate the two events from long experience. Reasoning, according to Hume’s arguments above, cannot justify the assignation of a causal relationship (Clatterbaugh, 1999, p. 198). Yet it is permissible and even defensible, from Hume’s perspective, to go on expecting the pain after stubbing my toe because of “custom or habit”:

“Perhaps,” Hume says, “we . . . must rest contented with it as the ultimate principle, which we can assign, of all our conclusions from experience” (p. 121). Eventually, Hume offers two definitions of “cause.” The first relates to the fact that “similar objects are always conjoined with similar”; since we have experience of this, it is possible to define cause as “an object, followed by another, and where all the objects, similar to the first, are followed by objects similar to the second” (p. 146). The second relates to the fact that “the appearance of a cause always conveys the mind, by a customary transition, to an idea of its effect”; based on our experience of this, cause may be defined as “an object followed by another, and whose appearance always conveys the thought to that other” (p. 146).

These two definitions, phrased differently, appear in Hume’s *Treatise of Human Nature* (1739/1975, p. 170), where we also find a statement of lasting consequence for the philosophy and practice of science: “For as our idea of efficiency is derived from the constant conjunction of two objects, wherever this is observed, the cause is efficient; and where it is not, there can never be a cause of any kind” (p. 171). Of the four Aristotelian causes (formal, material, efficient, and final), discussed above, only efficient cause is admitted by Hume. And even that is considered to be mere regularity. For an Aristotelian, the efficient cause of a chair is the carpenter not because our experience unfailingly associates the production of chairs with the observation of carpenters building them, but because the carpenter in a real way *caused* the chair to exist by making it. According to Feser (2009), “Humean analyses of causation, along with the philosophical puzzles they notoriously give rise to, are only possible if one rejects the Aristotelian notion of final causality, and thus the Aristotelian notion of efficient causality along with it” (p. 20). But it was Hume’s notion of efficient cause as regularity that modern science adopted as causality *in toto*.

CAUSATION AND THE MECHANICAL WORLDVIEW

The attack on formal causation by Hume and others is closely connected with the rise of mechanism – roughly, the attempt to treat the natural world like a machine. Henry (2002) offers a useful summary of the so-called “mechanical philosophy.” First of all, its adherents wished to exclude many of the explanatory principles used by Aristotelianism. They limited themselves to mathematical concepts such as “shape, size, quantity, and motion” (Henry, p. 69), rejecting explanations that could not be framed in such terms – including, of course, the Aristotelian notion of final cause. Democritean and Epicurean atomism also formed a plank in the mechanist platform, though not all of its early modern adherents held, as their ancient atomist predecessors did, that atoms are indivisible. (In this context, “atom” refers to a minimal particle, not to the models of current atomic theory.) Henry identifies two major systems of mechanism in the early seventeenth century: the Epicurean one of Pierre Gassendi and a more original one developed by Descartes. Descartes based his system, which would exert significant influence on science, on deduction from philosophical first principles: a procedure which is anathema, at least in principle, for scientists today. It is also an important reminder of the theoretical and practical heterogeneity during this period: Descartes, starting from metaphysics, and Bacon, starting from experience, are only two examples of thinkers who used differing approaches to influence what is now considered modern science.

In addition to Gassendi and Descartes in France, Newton in England was also developing the mechanical philosophy. According to Dijksterhuis (1961), Newton was in some sense its culmination, unifying its previously disparate strands into a coherent and systematic whole. Newtonian physics provided a mathematically respectable account of the mechanistic worldview, in which “the push and pull of external forces are central to understanding the

behavior of objects in the universe” (DeWitt, 2004, p. 182). The old Aristotelian terminology of act and potency, with all the teleological baggage it implied, was cast off. On the mechanistic view, the universe was like a watch: once wound up, perhaps by God, perhaps not, it had no need of a Prime Mover’s sustaining power in order to keep ticking (DeWitt).

THE TWENTIETH CENTURY: POSITIVISM TO KUHN

Since “positivism” is often bandied about like a dirty word, particularly in the SLA theory debates of the 1990s (see Chapter 3), it is worth the trouble to establish what the logical positivists attempted to do and how their failure led to further developments in the philosophy of science that bear on SLA theory (cf. Jordan, 2004, pp. 27-29). (For the distinction, if there is a meaningful one, between “logical positivism” and “logical empiricism,” see Uebel, 2013; for an introduction to the movement’s history and views, see Richardson & Uebel, 2007.) The nineteenth-century French philosopher Auguste Comte named the movement and was one of its first exponents. He explained the development of humanity in terms of three stages: the theological stage, the metaphysical stage, and finally the scientific stage. For Comte, a theological worldview, whether “animistic,” “polytheistic,” or “monotheistic,” is “inevitably more dependent on feeling and imagination than on reason” and thus it cannot be “intellectually satisfying” (Scharff, 2009, p. 81). The next stage, the metaphysical, involves the rejection of superstition and the embrace of reason and abstraction. Finally, in the scientific (or positive) stage, empirical observation is recognized as the only source of knowledge, but even that knowledge is relative to the observations we make. The quest for the “absolute” is in vain. Comte praised Bacon, Descartes, and Galileo in particular for paving the way to positivism (Scharff).

Comte's successors in the late nineteenth and early twentieth centuries – most prominently at the University of Vienna, in what became known as the Vienna Circle – established the Verifiability Criterion of Meaning (VCM), according to which a meaningful proposition is one that is (a) logically true or false by definition (i.e., propositions in mathematics or logic) or (b) one that is empirically verifiable. More technically, the logical positivists affirmed that “either propositions were of a synthetic nature and their assertion justifiable only *a posteriori*, or they were analytic in nature and justifiable by *a priori* reasoning” (Uebel, 2008, p. 79). Needless to say, this approach relegated many domains of inquiry to the non-meaningful – e.g., metaphysics, since metaphysical reasoning is synthetic *a priori*. In other words, the logical positivists allowed propositions such as “all bachelors are unmarried” (analytic *a priori*, true by definition) and “the sun rose this morning” (synthetic *a posteriori*, a statement about the external world verifiable by experience). They did not allow a proposition such as “God exists,” which is presumably not verifiable by experience nor true by definition, although it purports to make a claim about the external world.

There is an obvious problem with the VCM: it is neither verifiable through observation nor demonstrable through logic, and thus it fails its own test. As Uebel (2008) notes, however, it may be rescued by treating it as a “metalinguistic proposal” rather than as something intended to be either empirical or analytical (p. 79). In any case, however, the strongest form of the logical positivist project was eventually discarded: “In the twenty-first century, no one is a logical empiricist” (Richardson, 2007, p. 346). Richardson presents the so-called “received view” of logical positivism's decline as follows: Karl Popper's *The Logic of Scientific Discovery* (1935/2002) was one of several major challenges (see also Sellars, 1963, and Polyani, 1958) to logical positivism beginning in the late 1950s. These newcomers critiqued and problematized

logical positivism's commitment to the VCM. Thomas Kuhn, however, in his *The Structure of Scientific Revolution* (1962/1996), struck the death blow. This is, again, the "received view," as Richardson characterizes it, and he offers reasons worth considering for thinking it might be too simplistic. But this is not the place to evaluate Kuhn's effect on the decline of logical positivism or even whether Kuhn attacked it fairly. Instead, we need only outline Kuhn's sociological approach to science and its epistemological implications, since it helped pave the way for SLA's social turn.

Kuhn (1962/1996) first presents scientific progress as composed of four stages: pre-paradigm, paradigm (normal science), crisis, and revolution. In the pre-paradigm stage, there is little or no consensus among researchers on such fundamental items as how terms should be used, what facts count as significant, and what explanations or types of explanations are acceptable. If one particular theory emerges that is "sufficiently unprecedented to attract an enduring group of adherents" and "sufficiently open-ended to leave all sorts of problems" (p. 10), then a paradigm coalesces around that theory, leading to the halcyon state of normal science. Here, researchers' productivity increases since many of the pre-paradigm problems have been taken away: now, the victorious theory has given them a common vocabulary, shared assumptions, and consensus regarding what facts are significant and what explanations are acceptable. Within normal science, researchers engage in a form of puzzle solving. The paradigm provides strict boundaries for investigation. Their aim is not to innovate so much as to solve the puzzles which are important according to the theory.

But the bliss of normal science does not necessarily last forever. Crisis, Kuhn's third stage, occurs when one or more anomalies lead researchers to question the paradigm and to regard that anomaly as "something more than just another puzzle of normal science" (1962/1996,

p. 82). According to Kuhn, crises are resolved in one of three ways: either the anomaly is eventually explained within the paradigm; the anomaly remains unexplained but normal science continues; or finally the anomaly persists and ultimately causes the rejection of the current paradigm in favor of something new. The third possibility – revolution, the fourth stage – may occur only when a suitable alternative to the current paradigm is available. But Kuhn’s most debated contribution to the philosophy of science is perhaps the notion of incommensurability (see Hulstijn et al., in press, pp. 29-37, for the contribution of Ludwik Fleck to Kuhn’s thought). Since “the differences between successive paradigms are both necessary and irreconcilable . . . the normal scientific tradition that emerges from a scientific revolution,” Kuhn argues, “is not only incompatible but often actually incommensurable with that which has gone before” (p. 103). This claim is not only sociological but also epistemological (see Kuhn, pp. 103-108 for historical examples). Any particular scientific “advance” is explained in the context of a particular paradigm, not necessarily in terms of its correspondence to putatively objective reality. The new paradigm moves the goalposts, as it were, so that it is impossible to objectively compare it with the old. Moreover, a paradigm involves using terms in a certain way and in looking at the data in a certain way (i.e., the theory-ladenness of observation), so that researchers from rival paradigms “inevitably talk through each other when debating the relative merits” of their positions (p. 109). By the end of the book, Kuhn seems to embrace outright relativism: “we may have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth” (p. 171) (see the next section for the interpretation of Kuhn).

Having travelled far afield from Bacon and Descartes, we should take a moment to retrace our steps. As we have seen, Bacon’s emphasis on empiricism and Descartes’ emphasis

on rationalism, though not in complete harmony, nonetheless formed two prongs of a modernist approach to scientific research. The logical positivists took this modernist approach to the extreme by denying the meaningfulness of all propositions that are not verifiable either (a) by logic or mathematics or (b) by observation. Before long, it was deemed that positivism led to a cul-de-sac. Popper (1935/2002) challenged positivism with his falsificationism, which expanded the positivists' narrow conception of science; for Popper, a scientific claim was one which could be falsified. In contrast to a claim such as "Milton is the greatest English poet," which one could argue for or against with no resolution in sight, a scientific claim is testable and, at least potentially, falsifiable. But falsificationism is ultimately inadequate because theories may be easily modified in order to escape from a presumed falsification. If the hypothesis generated by my theory appears to be falsified by the empirical evidence, I need only make an *ad hoc* modification to the theory in order to save it. Popper attempted to avoid this difficulty by proposing that modifications be admitted only if they "increase the degree of falsifiability of the theory" (Losee, 2001, p. 153). Kuhn, however, bypassed falsificationism by abandoning the (modern) quest for objective scientific progress altogether and positing (postmodern) relativism instead: if paradigms are incommensurable, the falsification of any given claim will only be persuasive to those working in that particular paradigm; gestalt switches, not arguments and evidence, are responsible for conversions (the word's religious overtones are appropriate here) from one paradigm to another.

RELATIVISM: KUHN AND FEYERABEND

In the 1990s, SLA researchers spilled a great deal of ink over the notion of relativism. Block (1996), though claiming to eschew "unprincipled relativism," concluded that "we are condemned to accepting the existence of multiple theories, and above all, multiple perspectives

on research” (p. 78). His arguments were excoriated by Gregg, Long, Beretta, and Jordan (1997), who stand firm in support of “rational inquiry” (p. 538). (For discussion, see Chapter 3). Because the relativism supported by Block and rejected by Gregg, Long, Beretta, and Jordan owes a great deal to Kuhn and the famous “anything goes” relativist Feyerabend, it is important to establish Feyerabend’s oft-misunderstood position and how it developed from Kuhn’s.

First of all, Kuhn’s own position must be distinguished from the reception of his work by philosophers of science and by practitioners of various other disciplines. As mentioned above, the last edition of *The Structure of Scientific Revolutions* seems to advocate relativism, at least insofar as it decouples scientific “progress” (i.e., paradigm shifts) from the gradual accumulation of true beliefs about the natural world. Shapere (1964), in an early review of the book, argues that Kuhn’s relativism is merely the result of using “paradigm” too broadly and does not stand up to closer scrutiny: more historical examples of putative revolutions are less important than fine-tuning Kuhn’s “conceptual apparatus” (p. 393). Toulmin (1970), another important critic of Kuhn, cast doubt on the distinction between normal science and the state of revolution. Against Shapere (1964) and Scheffler (1967, 1972), Doppelt (1978) attempts to rescue Kuhn by distinguishing between “short-run” and “long-run” relativism – the former meaning relativism about “every major *stage* of scientific development” and the latter meaning relativism about “scientific development *as a whole*” (p. 71, italics original). Ultimately, however, Kuhn’s relativism – however interpreted, maligned, or defended – was designed to rescue science from the positivists (crucially, by denying the positivist commitment to the neutrality of observation) (Bird, 2011).

Although invoking the name of Feyerabend is fraught with peril in applied linguistics, it is necessary to do so here, precisely because Feyerabend’s famous catchphrase “anything goes”

was used as a club with which to beat purported “relativism” in the SLA theory construction debates of the 1990s (see below, Chapter 3). Block (1996), for example, accuses Long (1993) and Beretta (1991) of “Feyerabend bashing” (p. 68). As Block observes, Long mischaracterizes “anything goes” (Feyerabend, 1975/1993) as a “conclusion” (Long, 1993, p. 231), when in fact Feyerabend seems to have intended it more as historical description than methodological prescription (see Feyerabend, 1975/1993, pp. 9-19). In practice, “anything goes” encapsulates several of Feyerabend’s observations about the actual practice of science: for example, that using hypotheses “that contradict well-confirmed theories and/or well-established experimental results” may be effective, or that the “proliferation of theories is beneficial for science, while uniformity impairs its critical power” (Feyerabend, 1975/1993, p. 24). According to Feyerabend himself, there is in fact much agreement between him and Kuhn. In a “Postscript on Relativism” added to the third edition of *Against Method* (1975/1993), Feyerabend distinguishes between relativism and absolutism, rejects them both, and then proceeds to compare his own views with Kuhn’s. Both “oppose the strong programme in the sociology of science” (p. 271), which Bloor (2011), one of its proponents, characterizes as “sociologists go beyond the study of facilitation and distortion and claim to detect social influences at work on, and present in, the ‘internal,’ cognitive content of science” (p. 436) (the strong program was originally set forth in Bloor, 1976/1991). It seems that this position, rejected by Kuhn and Feyerabend, would be a more appropriate target for Long and Beretta’s ire than the convenient slogan of “anything goes.”

Ultimately, the arguments of Kuhn and Feyerabend, however they are best interpreted, do not entail complete relativism of any kind, epistemic or ontological. On the other hand, they do require the recognition that no observation is entirely neutral and that any truth claim is inevitably embedded in a sociological context. As Feyerabend says, “a *general* appeal to

scientific authority” – on a modernist conception of science’s purported objectivity and inexorable progression from less truth to more truth – “runs into contradictions,” but this is separate from the question of what scientists should do with their results (1975/1993, p. 271).

CHAPTER 3

SECOND LANGUAGE ACQUISITION

EMPIRICISM AND RATIONALISM: BEHAVIORISM VS. CHOMSKY

We now turn to an episode in the history of linguistics which exhibits a debate internal to the modernist tradition, between the empiricism of Bacon and the rationalism of Descartes: Chomsky's (1959) demolition of behaviorism as represented by Skinner (1957). Skinner's behaviorism derived its impetus from Bacon's empiricist research tradition, while Chomsky's approach, though some have called it Platonic (cf. Atkinson, 2011), owed the most to Descartes' rationalism.

Skinner (1957) attempted to apply the insights of behaviorism, derived largely from work with animals, to language acquisition. He confidently asserted that experimental work done on "other species . . . can be extrapolated to human behavior without serious modification" (1957, p. 3), a claim which Chomsky undermines in a footnote (1959, p. 26). For Skinner, understanding the *causes* of verbal behavior is nothing more or less than being able to predict individual events and, ultimately, being able to "produce or control such behavior by altering the conditions under which it occurs" (p. 3). In other words, according to the empirical method derived from Bacon, the scientist first uses observational data to make predictions and then attempts, by manipulating the environment, to validate those predictions by further observation. Skinner believed that psychology was the only discipline which could attempt this, and that even psychology had been hampered by the persistent notion that "to explain behavior, or any aspect of it, one must attribute it to events taking place inside the organism" (p. 5) – the rejection of which was a hallmark of behaviorism. At least for the purposes of psychological research, Skinner rejected not only the relevance of mental events, whether they exist or not, but also the independent

existence of both speech and meaning. In exchange for such philosophical conundrums, Skinner proposed a purely external analysis of verbal behavior.

As Chomsky (1959) noted, Skinner attempted to account for the variables of verbal behavior “completely in terms of such notions as stimulus, reinforcement, [and] deprivation” (p. 26) (or reinforcement and punishment). For example, a rat in a cage might be given food (a stimulus) after pressing a lever (a response). In this case, the stimulus of food is positive reinforcement, since it rewards the behavior. Or the rat might hear a loud noise after pressing the lever. Here, the stimulus is punishment, the opposite of reinforcement, since it reduces the frequency of the behavior.

Chomsky first attacks Skinner concerning definitions: in a laboratory, the terms may be used without difficulty; in “real life,” however, things are not so straightforward. He presents Skinner with a dilemma: if the psychologist defines behavior very broadly, so as to maximize the explanatory power of the theory, “he must conclude that behavior has not been demonstrated to be lawful,” owing to the interference of factors such as “attention, set, volition, and caprice” (p. 30). If, on the other hand, behavior is defined more narrowly so as to keep it within the bounds of the laboratory, it is “lawful by definition . . . but this fact is of limited significance, since most of what the animal does will simply not be considered behavior” (p. 30). According to Chomsky, Skinner is inconsistent and avoids both horns of the dilemma, preferring to keep his theory’s scope as broad as possible while eschewing the difficulties that come with the narrower definition. In these terms, Chomsky states his aim, which is to show “that with a literal reading (where the terms of the descriptive system have something like the technical meanings given in Skinner's definitions) the book covers almost no aspect of linguistic behavior, and that with a

metaphoric reading, it is no more scientific than the traditional approaches to this subject matter, and rarely as clear and careful” (p. 31).

At length Chomsky turns to language acquisition. In first language acquisition, children of immigrants, to use Chomsky’s example, learn language easily without the more advanced cognitive abilities of their parents, who struggle with the linguistic features absorbed naturally by their children. This argues against Skinner’s claim that reinforcement is necessary for each linguistic feature to be learned. In a similar vein, Chomsky advances the poverty of the stimulus – how can children and adults alike produce and understand sentences which they have never seen before? The stimuli are completely new. According to Chomsky, “These abilities indicate that there must be fundamental processes at work quite independently of ‘feedback’ from the environment” (p. 42). This is yet another illustration of the difference between the research traditions of Bacon and Descartes. Chomsky rejects Skinner’s Baconian empiricism in favor of a Cartesian view. Mental processes *must* be posited, he claims, to explain the phenomena of language acquisition; the environment alone is not sufficient.

UNIVERSAL GRAMMAR (UG) AND SLA

Of course, as Kuhn (1962/1969) has argued, scientific revolutions do not occur merely because one research program has been shown to be unsatisfactory; one must also fill the gap with an alternative. In Chomsky’s case, with behaviorism devastated, the alternative was Universal Grammar (UG) (see Kibbee, 2010, for a collection of essays on Chomsky’s role in “(r)evolutions”). Indeed, two decades or so after much of Chomsky’s most important work had seen the light, the analysis of Universal Grammar in Kuhnian terms had sufficiently advanced that Koerner (1983) was already able to write on the historiography of the “Chomskyan ‘Revolution.’” The quotation marks indicate Koerner’s view that “the term ‘revolution’ does not

properly apply to [generative grammar],” characterizing it as “basically post-Saussurean structuralism” that valorizes *langue* at the expense of *parole* (p. 152). Its revolutionary aspects were more social than theoretical. Newmeyer (1995) takes the opposite view. Chomsky’s success in showing “the practical possibility of a non-empiricist theory of linguistic structure,” which in turn made it possible to “derive” the *langue/parole* distinction (i.e., competence/performance) rather than merely asserting it as Saussure did (p. 25). For Newmeyer, this constitutes an honest-to-goodness Kuhnian revolution. In any case, what matters most for our purposes is that Chomsky’s work has undeniably exerted a tremendous influence on linguistics, whether it is properly termed “revolutionary” or not, and that it has also affected the beginning and continuing development of SLA research. I will here provide a brief account of UG, following Jordan (2004), before turning to the use which has been made of UG for SLA.

Chomsky’s approach to the human faculty of language begins with child language acquisition and the so-called “poverty of the stimulus” or the “logical problem of language acquisition.” These terms refer to the same *explanandum*, or thing to be explained: namely, the L1 input children receive cannot adequately account for the grammatical utterances that they eventually learn to produce. These utterances “are far more complex and rule-based than could be expected” (Jordan, 2004, p. 125), given the poverty or degeneracy of the input they receive. In a similar vein, the conundrum is called the *logical* problem of language acquisition since adequate input would seem required to explain the complexity of children’s utterances. In other words, children’s linguistic competence is underdetermined by the input (White, 2007). This position contradicts the behaviorist claim that the environment is all that is necessary to explain language acquisition. (Whether the logical problem of language acquisition obtains is a separate

question, open for debate, and does not concern us here; see Larsen-Freeman and Long, 1991, chapter 5, for evidence against one way that input might be degenerate.)

According to Chomsky, then, the consistency and complexity of children's utterances had to be explained in the face of apparently inadequate input. Similarly, the ability of native speakers, children or adults, to analyze the grammaticality of an apparently infinite number of sentences was also a problem. Chomsky illustrated this with the famous sentence "colorless green ideas sleep furiously," which is meaningless but still grammatical. "Colorless sleep ideas green furiously," on the other hand, is still meaningless but no longer grammatical (Jordan, 2004, p. 126). So UG posited that humans are born possessing a "language acquisition device" or "black box" which enables us to learn the rules of our L1. In the "principles and parameters" version of UG, some of these grammatical rules are universal (the "principles") and do not have to be learned, while some are specific to individual languages (the "parameters"). The task of linguists was to discover and codify the rules for a given language in order to accurately predict any possible grammatical sentence. It was also necessary, since it privileged innate rules, for UG to distinguish between "competence" and "performance." Competence refers to the internal mental representations speakers have of their L1's rules. Performance refers to what they actually produce. The latter, of course, is often marred by a variety of neural, physiological, affective, and environmental factors, meaning that UG as a research program is restricted to *inferring* from performance – production – a speaker's underlying grammatical competence.

What does SLA have to do with UG? First of all, UG had established itself as the dominant research tradition of linguistics by the time SLA was emerging as a unique endeavor in the sixties and seventies. Initially, SLA took its point of departure from Chomsky's theory (e.g., Corder, 1967), though Selinker (1972) is an example of early SLA research that was open to

other approaches such as ethnomethodology. White (2007), a major researcher in this framework, characterizes the goals of UG-for-SLA as parallel to those of UG itself: while UG attempts to “provide a characterization of the linguistic competence of native speakers of a language and to explain how it is possible for child [L1] acquirers to achieve that competence,” UG-for-SLA attempts to “account for the nature and acquisition of interlanguage competence” (p. 37). As we have seen, UG grounds “language use (comprehension and production)” in an “abstract linguistic system, a mental representation of grammar (syntax, phonology, morphology, and semantics)” (p. 37). Perhaps significantly, White characterizes the leap from UG to UG-for-SLA as an “assumption” that the linguistic competence of non-native speakers can be treated the same way as that of native speakers, though she emphasizes that the poverty of the stimulus obtains in both cases (p. 39). As in UG, researchers can investigate speakers’ linguistic competence only indirectly, through performance. (For a more detailed account, see White, 2003; for an argument against applying UG in child language acquisition to second language acquisition, see Bley-Vroman, 1989.)

Before proceeding to critiques of UG-for-SLA, it is worth addressing the “common misunderstandings” noted by White. First, the theory has a narrow scope and is not intended to account for all SLA phenomena. It does not address the question of *how* parameters change in L2 acquisition, although *that* they do is crucial to the theory. Second, UG does not necessarily predict native-like acquisition for non-native speakers. It does not predict that a language learner *will* acquire a feature such as *wh*-movement, for example, but that if the learner does acquire it, the principles of UG will apply. Third, UG does not entail that the L1 grammar is insignificant for L2 acquisition; some UG-for-SLA researchers, including Schwartz and Sprouse (1996) and White (1989) “claim that the initial state of L2 acquisition consists of the steady state grammar

of L1 acquisition” (p. 46) (i.e., the full access / full transfer model). Finally, White takes on claims that UG researchers think grammaticality judgment tasks “provide a direct reflection of linguistic competence” (p. 47). On the contrary, these tasks, like other sources of data used by UG researchers, allow only indirect access to UG.

Both Larsen-Freeman and Long (1991) and Jordan (2004) acknowledge the strengths of UG-for-SLA while identifying important weaknesses. The former critique the theory in terms of three assumptions it makes. First, it assumes that L1 acquisition is more or less complete by the age of five, although certain aspects of syntax are learned as late as age sixteen (Larsen-Freeman & Long). Second, it assumes that “certain syntactic principles are unlearnable, and therefore innate” (p. 236). Some data which UG researchers consider evidence for this claim, however, may be analyzed in ways that do not require positing innate linguistic knowledge. And third, UG assumes that “input available to learners is inadequate, specifically that it lacks essential negative evidence with which to remedy excessive complexity and overgeneralization,” when instead we may explain the evidence by taking learning to be “a process involving initial adoption of conservative hypotheses . . . followed by movement along a developmental continuum (of gradually increasing complexity)” (p. 238). Ultimately, Larsen-Freeman and Long lump UG-for-SLA together with “other approaches to [interlanguage] development based upon theories of language rather than theories of language learning” (p. 240).

Jordan (2004) critiques UG-for-SLA in terms of theory construction. For Jordan, UG’s primary deficiency as a theory of SLA is its limited scope. Out of the five communicative competencies proposed by Celce-Murcia, Dörnyei, and Thurrell (1995) – linguistic, strategic, sociocultural, actional, and discourse – UG can address only the first. More importantly, UG “is essentially a description of core grammar: it is not really a theory of learning at all” (Jordan, p.

162). In UG, “the process of acquisition is not interesting”; instead, the researcher’s “task is to describe the components of the core grammar” (p. 163). In other words, the theoretical apparatus of UG has already provided the mechanism *by which* acquisition takes place; the researcher’s only responsibility is to describe *what* is acquired. This is not satisfactory for a field which is at least ostensibly about how second languages are acquired, not merely about the state of language learners’ internal grammars.

THEORY CONSTRUCTION IN SLA: THE 1990S

Krashen’s Input Hypothesis was a precursor for the SLA theory construction debates of the 1990s. Because it unfortunately proved indefensible *as a theory*, it prompted a fierce discussion about the nature of SLA and what a defensible theory of SLA might be (Gregg, 1984, is a representative dissenter, discussed below). Krashen (1985) summarized his key claim in this way: “people acquire second languages only if they obtain comprehensible input and if their affective filters are low enough to allow the input in” (p. 4). This impressionistic language is already a clue to the problems with the Input Hypothesis, which largely consist in imprecise terminology.

The Input Hypothesis includes five subordinate hypotheses. The first posits a distinction between “acquisition” and “learning”: the former is unconscious, roughly equivalent to child language acquisition, while the second is a conscious process “that results in ‘knowing about’ language” (p. 1). The second, based on SLA research to date, claims that “we acquire the rules of language in a predictable order” (p. 1). The third develops the famous “Monitor”: utilizing Chomsky’s competence/performance distinction, Krashen argued that “learning” (in his sense) can affect only what is generated by our already acquired competence, as a kind of editor. The fourth hypothesis addresses input. We “acquire” (not “learn”) languages “in only one way – by

understanding messages, or by receiving ‘comprehensible input’” (p. 2). Krashen defines comprehensible input as $i + 1$, where i is the student’s current level and $+ 1$ indicates the next step in the “natural order” (hypothesis 2). This is explicitly grounded in Chomsky’s Language Acquisition Device (LAD) (Chomsky, 1975). Finally, the fifth hypothesis posits an “affective filter.” With Krashen’s penchant for quotation marks, he claims that the “acquirer needs to be ‘open’ to the input,” i.e., the filter needs to be “down” (p. 3); learners experiencing negative emotions, for whatever reason, will be unable to use the comprehensible input they receive.

Gregg (1984), mentioned above, is a standard attack on the Input Hypothesis. Though Krashen published the standard account of his theory in 1985, enough of it was already present by 1984 for Gregg to produce a withering critique. Purportedly applying “Occam’s Razor” to “Krashen’s Monitor,” Gregg addresses each hypothesis in turn and excoriates Krashen for inconsistency, for overextending Chomsky’s LAD, for terminological imprecision, for misunderstanding the morpheme studies that supposedly ground the Natural Order Hypothesis, and for making grandiose claims without sufficient evidence. Consensus quickly grew around the critiques of Gregg and other opponents of Monitor Theory (see Long, 2007, for a more recent but still negative evaluation of Krashen), and by the early 1990s, it was acknowledged by most SLA researchers that the field was once again in search of a unifying theory.

In 1993, the problem was addressed by a special issue of *Applied Linguistics*. Long (1990) had already broached the subject, proposing that “accepted findings” in SLA be identified and used to ground future efforts in theory construction. Long (1993) went further and, in the face of a wide variety of purported theories in SLA, advocated “theory culling.” To that end, after defending the experimental method against the so-called relativists, he follows Darden (1991) in setting forth the following criteria for assessing theories: “internal consistency” and

“non-tautologousness”; systematicity, modularity, clarity, “explanatory adequacy,” “predictive adequacy,” scope, and generality; extendibility and fruitfulness; “consistency with accepted theories in other fields”; “experimental testability” and the “ability to make quantitative predictions” (pp. 235-37). With Laudan (referenced again in Long, 2007), he suggests that consensus tends to arrive when one theory becomes “dominant” by satisfying the various sets of assessment criteria that already exist (p. 241). Even if perfect agreement on assessment criteria is unlikely, a single set of criteria may in principle still be defended.

Gregg (1993), in the same issue, attempts to define “explanation.” He distinguishes between explanation, description, and prediction: for a given phenomenon, one might be able to explain it but not describe or predict it, and so on. In SLA, he takes the *explanandum* to be “the acquisition (or non-acquisition) of L2 competence, in the Chomskyan sense of the term” (p. 278). Borrowing from Cummins (1983), he claims that any adequate SLA theory must have answers both to “why?” questions and to “in virtue of what?” questions; that is, “transition theories” and “property theories” (p. 278). Generative grammar is an example of the latter, while the explanation of a process such as language acquisition is an example of the former. Gregg affirms that causality must play a role in a theory of SLA, and in a passage that is intriguingly similar to neo-Aristotelian philosophy of science, argues against “restricting ourselves to the observable” (where “observable” does not mean the same as “empirical”) (p. 283). This perspective is clearly based on Universal Grammar, as indicated by Gregg’s choice of illustration – grammaticality judgments. Gregg even mentions Molière’s famous joke about opium – “Why does opium make one sleepy?” “Because it has a dormitive virtue.” This, however, is a typical early modern caricature of Aristotelian metaphysics rather than a fair representation (Feser, 2009).

Block (1996) disagreed with this cognitivist manifesto on the following counts: the undesirability of theory proliferation; the alternative to theory culling is Feyerabendian relativism in which “anything goes”; the existence of relatively uncontroversial “accepted findings” in SLA; and the importance of attempting to falsify these accepted findings (p. 64). Block accuses Beretta (1991) and Long (1990, 1993) of “science envy” due to their arguments that SLA should, in Kuhn’s (1962/1969) terms, move from a pre-paradigm stage to normal science, in which a guiding paradigm informs research and ensures productivity. Long and Beretta have oversimplified Kuhn, according to Block, by reinterpreting a “paradigm” as something identifiable and concrete, rather than the more nebulous Hegelian metaphor that Kuhn intended. Block was also concerned, Long’s assurances to the contrary notwithstanding, about the imposition of a dominant theory on SLA by the “gatekeepers” (p. 66). These gatekeepers would marginalize research from other paradigms and (somehow) contribute to what Block saw as an Anglo-American hegemony over published SLA research. Block invoked Feyerabend in his defense of relativism, making the appropriate observation that Feyerabend’s famous slogan – “anything goes” – must be contextualized and cannot properly be used as an instrument to malign relativists (see below, Chapter 2).

Gregg, Long, Beretta, and Jordan (1997) issued a scathing rebuttal of Block (1996). The quotation appearing on its title page neatly summarizes the rationalist agenda for SLA and indicates SLA’s characteristic neglect of metaphysics: “I take science philosophically seriously; good science is as close as we ever get to the literal truth about how things are” (Fodor, 1996, p. 20). Further down, they admit to recognizing “that reality cannot be fully apprehended, that it is at times extremely difficult to interpret empirical data, that any particular instance of falsification can be challenged . . .” (p. 549). Gregg et al. characterize their own position as “critical

realism,” according to which “reality exists but cannot be fully apprehended” and “objectivity remains a regulatory ideal” (p. 550). After a one-paragraph description of constructivism (Block’s favored alternative) taken from only one source, they dismiss it as incoherent.

THE SOCIAL TURN IN SLA

Before the theory construction debates had fairly died down, the next chapter in the history of SLA was already beginning: the so-called “social turn” (cf. Block, 2003). The aggravating factors were not unique to SLA. On the contrary, they have their antecedents in the field of education, where researchers such as Vygotsky (1978) had ignited a debate between researchers working from a cognitivist orientation and those working from a situative orientation (Lafford, 2007). The former, of course, is in the modernist tradition of Descartes, Chomsky, and Long, with its dualistic approach to the mind. The latter is in the postmodern tradition, which now begins to exert emphasis on SLA, previously dominated by modernism. Lafford aptly observes that this difference manifested itself in SLA in the realm of ontology: what is the nature of acquisition and where does it occur? Cognitivists asserted that acquisition occurs in the individual’s mind; other researchers questioned our ability to peer inside the subject, as it were, and access such a place, preferring to locate acquisition in the co-construction of interaction between interlocutors (Lafford, 2007).

One of the earlier challenges to the prevailing cognitivist orientation of SLA, along with others such as Breen (1985) and Block (1996), was Markee (1994). Markee introduced a valuable distinction from Ochsner (1979) between nomothetic science and hermeneutic science: the former, from the Greek verb νομοθετέω (*nomotheteō*, “to frame or ordain laws”), “assumes the existence of a single discoverable reality that causally obeys the laws of nature”; the latter, from the Greek verb ἐρμηνεύω (*hermēneuō*, “to interpret”), “assumes that multiple realities exist

and that human events in particular can be interpreted only according to their outcomes” (Markee, p. 90). It should be clear that nomothetic science as characterized by Markee is squarely in the modernist tradition of Descartes as well as Bacon, while the positing of multiple realities (and truths) is a more postmodern move. Markee critiqued cognitivist SLA’s commitment to nomothetic science at the expense of hermeneutic science and called for a “respecification” of SLA via ethnomethodology in order to “make the world investigable in the participants’ own terms” (p. 97).

But it was the appearance of Firth and Wagner (1997) (hereafter F&W), as Larsen-Freeman (2007) observed, that garnered the most attention for the nascent social turn and became the lightning rod for the cognitivist/social debate. Debate in the social turn, as I will argue, was hindered by its contributors’ occasional failure to articulate and address their own and others’ philosophical or meta-disciplinary assumptions. For this reason, it is particularly important to elucidate the philosophical assumptions of F&W, which they did not always make clear. First of all, F&W did not argue that cognitivism in all its manifestations should be thrown out (a point seemingly lost on some of their detractors). What they did argue is that “SLA research requires a significantly enhanced awareness of the contextual and interactional dimensions of language use, an increased ‘emic’ (i.e., participant-relevant) sensitivity toward fundamental concepts, and the broadening of the traditional SLA data base” (p. 285). The first point has to do with the distinction between acquisition and use. Cognitivist SLA researchers, as we have seen, located acquisition in the mind of the individual learner. As an example, they mention Gregg (1993, cited above) who took the explanandum of SLA to be Chomskyan linguistic competence, and the research question of SLA to be “How is L2 knowledge instantiated in the mind/brain?” (p. 279). F&W argued that an excessive preoccupation with

language *acquisition* – as it purportedly occurs in the individual learner’s mind – had led SLA researchers to neglect rich insights available in the study of language use.

The acquisition/use dichotomy also relates to F&W’s second point, regarding the notion of participant-relevancy. Even here, F&W are careful to emphasize that they are not calling for the wholesale rejection of etic (analyst-relevant) concerns in favor of emic (participant-relevant) ones. Instead, they are claiming that SLA as a discipline is biased in favor of the former.

F&W’s view is that it is not sufficient to treat learner language solely as data useful for explaining the instantiation of L2 knowledge in the mind (to use Gregg’s words). Since learners are agents in the interactions they take part in, it stands to reason that SLA can benefit from an *emic* perspective, one which asks what is relevant from the participant’s point of view and not necessarily from the researcher’s. Even using the term “learner” as I have done is problematic from an emic perspective, since the individuals considered “learners” by researchers may or may not consider themselves to be such. In addition to “learner,” F&W call into question the time-honored SLA constructs of “native speaker,” “non-native speaker,” and “interlanguage.”

Finally, as for “broadening the SLA data base,” their third point, F&W seem to mean that SLA should incorporate data garnered by means other than standard Chomskyan grammaticality judgment tests.

F&W’s paper evoked a storm of protests from cognitivist SLA researchers. Kasper (1997), Long (1997), and Gass (1998) were the initial respondents (the former two in the same volume in which F&W appeared, Gass in the following one). Unfortunately these papers exhibit a striking failure to engage directly with F&W’s points, suggesting that a social orientation to SLA may be actually be *incommensurable* with a cognitivist orientation. As Larsen-Freeman (2007) observed, “one view cannot readily accommodate the other” (p. 779). Kasper (1997)

(who later defected from the cognitivists to the social orientation) asserts that “issues of knowledge representation, processing, and recall have to be central to any discipline that is concerned with learning . . . a non-cognitivist discipline that has learning as its central object is a contradiction in terms” (p. 310). This illustrates the importance of getting one’s assumptions out in the open. The claim in question rests on the assumption that the mind – defined as cognitivists would define it – is the locus of acquisition. But presumably this is an assumption that F&W, or at least other non-cognitivist SLA researchers such as Markee (2000, 2008, 2011; Markee & Kunitz, 2013), as well as Kasper herself (today), would deny. What Kasper seems to mean is that “a non-cognitivist discipline that has learning as its central object is a contradiction in *cognitivist terms*” – this is certainly defensible, but if one rejects cognitivism, it is hardly final. In the same vein, the “issues of knowledge representation, processing, and recall” are based on a computational model of the mind that is also based on cognitivism; remove certain cognitivist presuppositions and suddenly much more is open for debate. On the other hand, Kasper acknowledges that “there is a whole range of issues about SLA that cognitive theory does not tell us anything about” (p. 310). As we have seen, this is one of F&W’s points. They did not call for the wholesale rejection of cognitivism, but instead for the recognition that it cannot address everything that can reasonably fall under the auspices of SLA.

Long (1997) and Gass (1998) both attack F&W with similar concerns about the relevance of language *use* for language *acquisition* – again, “acquisition” as defined in cognitivist terms. Long concedes that F&W are “probably right” to argue that SLA would benefit from additional attention to participant-relevant concerns and a more “generally sociolinguistic orientation” (p. 322), but excoriates them for what he considers an indefensible and ultimately unnecessary endorsement toward the relativism of Block (1996). Long’s article seems to beg the question

against F&W several times, of which I will limit myself to the first. He accuses F&W of “ignoring their own admonition” by considering “‘learner,’ ‘NS,’ and ‘NNS’ relevant ‘identities’ and legitimate terms in this discussion” and goes on to assert that “‘NNS’ was the relevant label in Long (1981) given the study being presented there” (p. 320). This illustrates that Long, like Kasper, has missed at least part of F&W’s point. First of all, in order for F&W to have contradicted themselves merely by using the terms “learner,” “NS,” and “NNS,” their argument would have had to be that no one may even discuss those identities under any circumstances unless they have been shown to be relevant in a given interaction. But they did not make such a sweeping and impractical claim. It is perfectly legitimate to discuss, in the abstract, possible identities that participants may orient to. This falls far short of F&W’s real concern, which was arbitrarily *assigning* those identities to participants when it has not been shown that any participant has in fact oriented to one of them. Long, however, does just that by claiming that “NNS” is the “relevant label” for a particular study. As Long himself is fond of saying, this is an empirical matter (not to be settled by fiat). (Long, 2007, persists in arguing against F&W by accusing them of failing to address “acquisition” in the cognitivist sense that they of course reject.)

Gass (1998), for her part, approaches the issue of identifying participants as “learners” in a similar way. F&W, when referencing Varonis & Gass (1985), said that “NNSs tend to be cast in the same light as learners” (p. 292). In response, Gass argued the following:

It was not a matter of casting them in the same light as learners, but rather they *were* learners inasmuch as these individuals were students at the English Language Institute of the University of Michigan where they were paying rather large sums of money in order to learn. (p. 85)

Here again we find different research traditions talking past each other. Surely F&W would not deny that people who have student identification cards from a university, for example, are students in one sense of the word. But the money one spends, or the card one carries, or the class one goes to are not criteria that F&W consider relevant for establishing the identity to which participants orient in a given interaction. Yet Gass asserts that people who happen to pay money for language classes are learners for the purposes of SLA research – a claim which is not participant-relevant at all.

Ultimately, then, the social turn made possible “alternative approaches” to SLA (as in the eponymous volume edited by Atkinson, 2011). The seeds were sown in part by the theory construction debates of the early to mid-1990s, by Markee (1994), and others, but it was F&W’s article that somehow became the standard-bearer for those critiquing the cognitivist hegemony. Many of the symptoms described by Kuhn in his account of crisis and revolution are present: disagreement about what counts as evidence, about what key terms are and what they mean, and about who may even appropriately claim to be an SLA researcher. It should also be noted, however, that social SLA researchers did not necessarily aim to provoke a revolution; even F&W’s language in their 1997 paper reflects their emphasis on *expanding* SLA’s repertoire of concepts rather than any desire to overthrow cognitivist research altogether (see Chapter 4 on cognitivist SLA, social SLA, and neo-Aristotelianism for how the two research traditions may coexist).

EMERGENTISM IN SLA

The “emergentist turn” in SLA (Ortega, 2009, p. 102) was inaugurated most notably by N. Ellis (e.g., 1993, 1996) and by Larsen-Freeman (e.g., N. Ellis and Larsen-Freeman, 2006; Larsen-Freeman, 1997, 2011). According to Hopper (1998), emergentism rejects the notion of

“discretely bounded ‘modules’ of syntax, semantics, morphology, phonology, and pragmatics” and instead claims that there are only “contextually conditioned ways of saying things” (p. 172). Apparent order is *laid over* the data rather than actually existing *in* the data. Connectionism, a key notion for emergentism, applies this approach to linguistic “rules”: whatever rules we might seem to find are only convenient regularities produced by learning mechanisms. Language is “rule-like” rather than “rule-governed” (N. Ellis, 1998, p. 638). In general, as Larsen-Freeman explains it, emergentism views language “as a complex adaptive system, which emerges bottom-up from interactions of multiple agents in speech communities . . . rather than a static system composed of top-down grammatical rules or principles” (2011, p. 49). Further, according to Ellis, “a fundamental tenet of SLA is that we learn language in much the same way as we learn everything else” (2007, p. 77). Although both emergentism and cognitive SLA owe much to information processing theories in cognitive science (Ortega, 2011), emergentism’s insistence on language learning as a garden-variety cognitive activity rather than unique in some way indicates the extent of its difference from cognitive SLA. In any case, Ellis’s “Associative-Cognitive CREED” uses an acronym to provide a useful introduction to the his version of emergentism for SLA: “SLA is Construction-based, Rational, Exemplar-driven, Emergent, and Dialectic” (2007, p. 77).

The first attribute of SLA refers to the linguistic *constructions* that learners acquire. Crucially, for Ellis, these constructions are acquired based on their frequency in the input: “high frequency constructions are more readily processed than low-frequency ones” (p. 78). As learners receive input, they come to associate various forms with meaning, with higher frequency constructions, of course, being learned more quickly. As they receive and process more and more input, these statistically derived regularities reinforce the form-meaning connections they

make. Second, language learning is *rational*: learners' "understanding of the way language works is the best mental model possible, given their linguistic experience to date" (p. 80). Third, SLA is *exemplar-based* because it derives generalizations from "frequency-biased abstraction of regularities from similar constructions" (p. 80). We use prototypes, or the most representative exemplars of their category, as a basis for classifying other like or non-like things. In language, to use Ellis's example, this helps explain why irregular plurals in English are problematic: they are outliers, dissimilar from the prototypical plural (e.g., "child," but also "moose" compared with "goose").

Fourth, SLA is *emergent* because regularities arise from a mass of complex phenomena and processes. Ellis is careful to disavow any causal role for these regularities, comparing them to geological descriptions of the Earth's crust: observing and cataloguing a phenomenon is not the same as causing it. On emergentism's own testimony, then, the causal mechanisms desired by many in SLA must be sought elsewhere. As Larsen-Freeman (1997) explains, emergentism in SLA is based on "the similarities among complex nonlinear systems occurring in nature and language and language acquisition" (p. 142). These complex nonlinear systems have several characteristics: they are dynamic, chaotic and unpredictable, and adaptive. Finally, SLA is *dialectic* because language learning results in a conflict between learners' interlanguages and the feedback they receive, which "allows socially scaffolded development" (N. Ellis, 2007, p. 84).

Emergentism encompasses a variety of approaches to SLA, rather than a single theory, that attempt to find a place for all the factors seeming to play a role in second language acquisition. Ellis and Larsen-Freeman (2006) see emergentism as a potential way of reconciling the binaries and oppositions which so often appear in systems made by humans. They identify a large variety of these binaries in applied linguistics: langue/parole, mind/brain, implicit/explicit,

acquisition/learning, learning/use, form/meaning, etc. These purportedly oppositional pairs, however, according to Ellis and Larsen-Freeman, “drive change” between the two extremes – in other words, the dialectic mentioned by Ellis, discussed above. To their credit, they acknowledge this dialectic’s rich philosophical history – from Plato to Hegel – but they stop short of sketching the epistemological and metaphysical common ground necessary to truly reconcile research traditions whose practitioners hold different presuppositions.

SLA THEORY TODAY: THE *STATUS QUAESTIONIS*

Where then does SLA stand in the second decade of the twenty-first century? Shortly after the turn of the millennium, a volume edited by Doughty and Long (2003) appeared in which cognitivist researchers Doughty and Long (2003) and Gregg (2003) reaffirmed positions articulated in the 1990s (see above, Theory construction in SLA: the 1990s). Long and Doughty’s paper, titled “SLA and Cognitive Science,” reads like a manifesto. It is a call for SLA to identify itself, once and for all, as a cognitivist discipline and find its “intellectual and institutional home” in the field of cognitive science (p. 869). Only by doing this, according to Long and Doughty, will it “achieve the stability, stimulation, and research funding to survive as a viable field of inquiry” (p. 869). Lest their argument seem merely pragmatic, however, it should be noted that they identify similarities between SLA and cognitive science and argue from these that the two fields should unite, or perhaps that the former should be subsumed under the latter, to the benefit of each. It is ironic, however, that one of the similarities mentioned between the two fields is “theoretical and methodological diversity,” when Long and Doughty are calling for the elimination of a great part of that diversity in the name of “substantive coherence” derived from their “common focus of inquiry, the mind and cognition” (p. 869).² For we should make no mistake: if SLA is similar enough to cognitive science that both may be pursued under the same

roof, many SLA researchers would need to continue their investigations under a different name. But presumably this would not trouble Doughty and Long, since Long (1997), in response to Firth and Wagner, argued that researchers focusing on language use, not acquisition (as defined by cognitivists), are not actually doing SLA at all (see above, *The Social Turn in SLA*, p. 31).

Gregg (2003), in the same volume, unsurprisingly reaches the same conclusion as Doughty and Long (2003): SLA is not a social science but a cognitive science. I expand Gregg's argument here: (1) "SLA theory is the theory of the acquisition of a second language" (p. 835). (2) The term "acquisition" means the same as the term "learning." (3) Learning occurs in the mind. (4) Therefore, "SLA is . . . first and foremost an *internalist* discipline rather than an externalist discipline" and "falls within the scope of cognitive science" (p. 835, italics original). Like Doughty and Long, Gregg takes to task the benighted SLA researchers who refuse to accept such a characterization of the field. Charitably, however, he notes that learner behavior is still valid and valuable data for SLA, and restates his position in slightly weaker terms: "The point is simply that we must distinguish between evidence for an SLA theory (learner behavior) and the object of that theory (learner mental states)" (p. 836).

But a growing number of SLA researchers, especially since Firth and Wagner (1997), do not embrace the cognitivist approach to SLA that seems utterly unavoidable to Doughty, Long, and Gregg (an approach which is by no means universal even in cognitive science: for the reconceptualization of cognition as socially distributed, see Clark, 1997, and Gallagher, 2005; Kunitz & Markee, 2014, discuss this research in the context of CA). Ortega (2011) identifies several differences between the social approach to SLA developed since Firth and Wagner and the cognitivist approach championed by Long and others. First of all, cognitivism favors "*psychological*" explanations while other SLA approaches favor "*socially oriented*" ones (p.

168, italics original). Second, cognitivist SLA values *abstractness* and universality based on a modernist view that knowledge is independent, universal, and transferable. Social approaches to SLA, on the other hand, emphasize the *situatedness* of knowledge, a concept that takes a variety of incarnations depending on which social approach is using it, but which in essence has to do with the impossibility of extracting knowledge from its context and removing it from its possessors. Third, cognitivist SLA tends to view phenomena in terms of “*entities and objects*,” while social approaches to SLA tend to prefer “*actions and processes*” (p. 168, italics original). A hallmark of SLA after the social turn, according to Ortega, is a tendency to reject the dichotomous thinking that we have inherited from the Enlightenment: rather than compartmentalizing the world with such dichotomies as mind/body, langue/parole, competence/performance, and acquisition/use, many SLA researchers are asking what benefits there might be from rejecting these rigid categories (cf. Ellis and Larsen-Freeman, 2006, discussed under Emergentism in SLA above. As we have seen, the competence/performance dichotomy has come to us from Chomsky via Descartes’ mind/body dichotomy – a classic problem of modern philosophy that, as Feser (2009) has argued, Aristotelian metaphysics avoids completely. In this way, social SLA is similar to Aristotelianism in their united rejection of modernist dichotomies.

But to return to the *status quaestionis*: Larsen-Freeman (2007), discussing cognitivism after the social turn, observed that “one view cannot readily accommodate the other” (p. 779) – language reminiscent of Kuhn’s notion of incommensurability. It remains a fact that cognitivist SLA and social approaches to SLA are divided by contradictory epistemological and metaphysical commitments, which directly affects each side’s philosophy of SLA and leads one of them to claim that the other is not even in the same field. For evidence, we need only look to the often unproductive debate following the publication of Firth and Wagner (1997), which was

discussed above. In sum, there can be no *rapprochement* without some willingness on each side, but particularly on the cognitivist side, to consider a philosophy of science grounded in different epistemological and metaphysical commitments. Perhaps a different set of commitments would allow SLA to navigate between the “Scylla of modernism and the Charybdis of postmodernism” (Dempster, 2003, p. 16) in their extreme forms, or, better yet, to reject the dichotomy altogether.

CHAPTER 4

SLA AND NEO-ARISTOTELIANISM

NEO-ARISTOTELIANISM

Such a *rapprochement*, however, must begin with metaphysics, a subfield of philosophy that is not commonly mentioned in SLA. As Tahko (2012) writes, “we could not get very far in our inquiry into the nature of reality with just one of these disciplines” (i.e., metaphysics or science; p. 33); and, I would add, whether or not we take “reality” to be accessible. The comparative rarity with which SLA researchers articulate the metaphysical positions underlying their research programs is at least partly responsible for the incommensurability, real or apparent, between cognitivist and social SLA (Doughty & Long, 2003, Long, 1993, 1997, 2007, Jordan, 2004, and Markee, 1994, are to be commended for tackling this problem head-on). It is my contention that neo-Aristotelianism, since metaphysically it differs from both cognitivist SLA and socially oriented SLA, may offer a unifying principle for the various strands of SLA research as they stand after the social turn. Some justification for this optimism may be found in the growing body of neo-Aristotelian work in the philosophy of science, particularly in physics and economics (in general, see Cartwright & Pemberton, 2013, Chakravartty, 2013, Harré, 2013, and Oderberg, 2007, chs. 8-9; for physics, see Cartwright, 1989, 1999; for economics, see Cartwright, 2007, and Lawson, 2013).

The central notion to be explicated in neo-Aristotelian metaphysics is that of *powers* (sometimes called *dispositions* or *capacities*; the difference will be noted when significant). The idea that things have inherent powers has been ridiculed since the early modern period (Molière’s joke about opium having a dormitive virtue, mentioned above, is representative), when Scholasticism fell out of favor. But as C. S. Lewis (1955/1965) once wrote, we must not

assume “that whatever has gone out of date is on that account discredited” (p. 207). To reject Aristotelian powers with typically pejorative terms such as “ancient” or “medieval” will not do. Lewis continues: “Was it ever refuted (and if so by whom, where, and how conclusively), or did it merely die away as fashions do? If the latter, this tells us nothing about its truth or falsehood” (p. 207). It is not at all clear that Aristotelian metaphysics was conclusively refuted by the early moderns, since much of their attacks on it consisted more of ridicule than of attempts to argue against Aristotle (or Aristotle as interpreted by the Scholastics) on his own terms, without oversimplification or misrepresentation (Cartwright, 1999; Feser, 2009).

In any case, neo-Aristotelians are reviving the notion of powers. Cartwright and Pemberton (2013) explain it with a minimum of metaphysical terminology: “Our powers are Aristotelian in that we suppose that what a power does when exercised is in the *nature* of that power” (p. 93, italics original). But what does “power” mean? B. Ellis (2002), using the term “disposition” rather than “power,” notes that “essentialists believe that there are genuine dispositional properties in the world, which are inherent in the things that have them” (p. 60). It is important to keep in mind that though the power inherent to a given entity is invariable, its effect may be realized in different ways depending on external factors. Cartwright and Pemberton (2013) call this the difference between a power’s “canonical effect” and “the outcome that actually results” (p. 94). For example, human beings have the power or capacity, among others, to learn language (at the moment it is not necessary to locate this power more precisely). The canonical effect of this power in children is, eventually, the native-like acquisition of the L1. But that canonical effect is not guaranteed; it requires the presence of certain external factors. The tragic story of “Genie,” a girl who had very little contact with anyone for ten years starting at age 3, is a case in point (Krashen et al., 1973, Fromkin et al., 1974, and Curtiss, 1977).

Because Genie was deprived of input when it was necessary, her power of language acquisition had no opportunity to manifest itself. She had the capacity, but the canonical effect was not produced due to external circumstances.

Cartwright (1999) advanced a way to account for this difference between a power's canonical effect and its actual outcome: the nomological machine (further developed in Cartwright & Pemberton, 2013). A nomological machine "is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws" (Cartwright, 1999, p. 50). In the case of L1 acquisition, we have already sketched the beginnings of a nomological machine: there is at least one necessary factor for a child's power of language acquisition to have its canonical effect: adequate input. In terms of a nomological machine, Genie's failure to attain native-like competence in an L1 resulted from the absence of that necessary factor. In Genie's case, the nomological machine in which the human capacity for language acquisition has its canonical effect was broken.

But nomological machines are also important for Cartwright's account of scientific laws. She rejects the Humean account of laws as mere regularity (see above) and instead defines them, somewhat idiosyncratically, as "a necessary regular association between properties antecedently regarded as OK" (p. 49). Crucially, these laws hold *ceteris paribus*, given a nomological machine. In this way, she takes laws as comparable to fables, observing that the morals of fables – such as "never trust your enemy" – are not necessarily true in all circumstances. Instead, they hold *ceteris paribus*. Other things being equal, the admonition to never trust one's enemy is a good one, but of course in many situations it might be advisable to do so. The same is true for scientific laws: "Newton's law, for instance, can be true of exactly those systems that it treats

successfully . . . That does not mean that we have to assume that Newton has discovered a fundamental structure that governs all of nature” (p. 47).

Cartwright originally developed the notion of a nomological machine in order to ground the isolationist experimental techniques used in physics and other sciences (such as some areas of SLA, to be addressed below). The crux of what Cartwright’s neo-Aristotelianism has to offer the sciences is found in the role of nomological machines in experiments. Experiments aim to uncover regularities, which, for Cartwright, indicate the “successful operation of nomological machines” (1999, p. 77). According to a Humean view of causality, behind those regularities are only more regularities, and so on. For Cartwright, however, the basic stuff of our scientific knowledge is not laws (i.e., Humean laws of regularities), but capacities. It is important to note that Cartwright is not advocating that science study capacities rather than laws; instead, she is arguing that, without realizing it, science already does. Modern science distinguishes between a thing’s nature and what it does, unlike the Aristotelians, who took natures and essences to be the same; furthermore, unlike the Aristotelians, modern science does not expect these natures to reveal themselves in everyday behavior, but instead uses the experimental method to find them out (Cartwright, 1999). Nonetheless Galileo, Bacon, and Descartes handed down to us the tendency to “associate a particular principle of change with a given structure or characteristic” (p. 81), and it is this continuity with the Aristotelian tradition that leads Cartwright to make the above claim.

Like Cartwright, I accept, at least for the present purpose, the modern distinction between natures and essences. If neo-Aristotelianism is to be made amenable to SLA researchers, it is probably best done with a minimum of metaphysical baggage. Cartwright’s philosophy of

science, combined with Aristotle's four causes (addressed above), should be sufficient for a first step toward unifying SLA along neo-Aristotelian lines.

SLA AND NEO-ARISTOTELIANISM: LAW AND CAUSALITY IN COGNITIVIST SLA

In this section and the following one, I intend to draw out representative accounts of scientific law and causality in SLA and compare them to Cartwright's neo-Aristotelian account. My central claim, to be developed in this section and the following ones, is that cognitivist accounts of SLA are interested in what Cartwright calls *ceteris paribus* laws, and that social orientations to SLA are interested in the phenomena of SLA *ceteris non paribus*. This is a principled difference that requires neither camp to relinquish its claim to be "doing SLA." On the contrary, it means adopting a particular, neo-Aristotelian approach to scientific laws and recognizing that the search for *ceteris paribus* laws is not the only enterprise worthy of the adjective "scientific" or the only enterprise that addresses "learning."

Since Long (1990, 1993, 2007; Larsen-Freeman & Long, 1991) has perhaps written the most directly addressing laws in SLA, his work is the logical starting point, along with that of his allies Gregg (1993, 2003) and Jordan (2004) (see also Gregg, Long, Jordan, & Beretta, 1997). To what extent does it align with Cartwright's neo-Aristotelianism? Recall that Cartwright's crucial distinction is between laws as accounts of what things do (in the Humean tradition) and laws as accounts of what is in the nature of things to do (Cartwright's neo-Aristotelianism). We are immediately confronted by a difficulty, namely the tendency of many SLA researchers to dance around the explicit articulation of their metaphysical positions. Even those such as Long, who do articulate some of their philosophical assumptions, do not talk in terms of Humean causation versus neo-Aristotelian causation, nor do they concern themselves with what a scientific law *is*. The absence of "metaphysics" and "law" from the index of a recent handbook

of *Cognitive Linguistics and Second Language Acquisition*, co-edited by Nick Ellis (Robinson & Ellis, 2008), is symptomatic (“cause” appears, although the only reference leads to a chapter in which the notion of causality is employed uncritically, without investigation into its meaning). Even Long’s (2007) account of the components of a theory – source, domain, content, type, and form – addresses the concept of “law” only briefly, under “form.” Here we are treated to a distinction made by Larsen-Freeman & Long (1991) between set-of-laws theories and causal-process theories. In the first case, researchers make generalizations about their findings, which, if they generate consensus and remain unfalsified long enough, eventually become laws. The more laws heaped up in this way, the broader the theory. But even this takes for granted, rather than specifies, what a law *is* or what it is about. The second variety of theory consists of constructs and concepts together with ways to operationalize them. This leads to interrelated statements of which some, at least, are testable, even if not every construct is. In this way, one may indirectly get at the construct.

None of this seems to directly conflict with Cartwright’s neo-Aristotelianism. It might help to look at some actual candidates for laws, or at least generalizations, in SLA. Long (2007) provides some examples from Spolsky (1989), such as “The younger one starts to learn a second language, the better chance one has to develop a native-like pronunciation” (Spolsky, 1989, p. 96). On Cartwright’s neo-Aristotelian analysis, this generalization holds true in a specific nomological machine. Given an American eight-year-old and an American twenty-year-old in the same environment – e.g., a year-long stay in France – one would expect the eight-year-old to have more native-like pronunciation of the L2 at the end of the year. We could specify certain factors for the machine: adequate comprehensible input, adequate opportunities for output, proper neural functioning, etc. If *these things are equal*, the law holds true. On the same

analysis, of course, we could break the nomological machine. Assume the eight-year-old lives with an American family and attends an English-speaking international school while the twenty-year-old lives with a French family and attends the University of Paris. In this situation, the twenty-year-old might very well finish the year with better French pronunciation (and likely conversational skills as well) than the eight-year-old. It is only when other things are equal that the nomological machine is able to function based on the *capacity* of the eight-year-old for superior phonological acquisition. On this admittedly superficial analysis, therefore, it seems that Long's account of laws could easily incorporate Cartwright's neo-Aristotelian apparatus.

But it is worth examining an actual study from a cognitivist perspective. I have chosen to focus on interactionism since this approach to SLA has the backing of prominent cognitivists such as Long, Gass, and Mackey (e.g., Long, 1996, Gass, 1997, and Mackey & Abbuhl, 2005). Although its proponents do not claim for it the status of a theory (Gass & Mackey, 2007), it is still an approach to SLA that addresses the phenomena from an unabashedly cognitivist perspective. Because, again, the relevant philosophical presuppositions are often not fully articulated by SLA researchers, I will extract what seem to be the metaphysically significant statements. In Gass, Mackey, and McDonough (2000) – which Gass and Mackey (2007) take as an archetypical interactionist study – we find unsurprising statements that indicate a Cartesian view of the mind: for example, Gass, Mackey, and McDonough refer to making “forms not yet in the learners' current repertoire” (p. 473). Reading between the lines, we naturally understand this to be a *mental* repertoire. They also mention that noticing “is a step toward change” (p. 473). Change in what? In the learners' mental interlanguages, representations of L2 grammar that exist in the mind. Though Gass, Mackey, and McDonough are not working in UG-for-SLA, they nevertheless have recourse to the Chomskyan distinction between competence and

performance, claiming that understanding competence is a necessary condition for understanding L2 acquisition. Again, the locus of this acquisition is the mind. The isolationist experimental method used in the study itself is very similar to the examples of physics research discussed by Cartwright (1999).³

Of course, the difference between acquisition and the behavior of physical objects means that the direct application of Cartwright's examples to SLA is impossible. Nevertheless, using Cartwright's example of an excited atom will illustrate the similarity. Cartwright says that "an atom in an excited state, when agitated, emits photons and produces light. It is, I say, in the nature of an excited atom to produce light. Here the explanatory feature – an atom's being in the excited state – is a structural feature of the atom, which is defined and experimentally identified independently of the particular nature that is attributed to it" (p. 81). Gass, Mackey, & McDonough say that, at least in this particular study, learners had "relatively accurate" perceptions about "lexical, semantic, and phonological feedback" when engaged in "task-based dyadic interaction" (p. 472). If these results are generalizable, according to a neo-Aristotelian approach to SLA à la Cartwright, we might say that it is in the nature of language learners (or rather human beings engaged in language learning), during task-based dyadic interaction, to have relatively accurate perceptions about lexical, semantic, and phonological feedback. Thus the generalization would hold only in reference to the particular nomological machine (i.e., the task-based dyadic interaction), set up by the researchers for the study. That nomological machine is what enables the researchers to examine interaction *ceteris paribus*, i.e., without the multifarious factors that influence SLA in the wild, as it were. Turning to social approaches to SLA illustrates what happens when nomological machines created by controlled experiments are discarded.

SLA AND NEO-ARISTOTELIANISM: SOCIAL SLA

With social approaches to SLA, it becomes harder to find a representative account of laws, probably because social approaches are not as interested in generalizability (although see Markee, 1994, for an account of ethnomethodology's ability to make generalizations in spite of its methodological difference from cognitivist SLA; see also Markee, in press, on the possibility of replication studies in qualitative research). As Ortega (2011) observes (see above, p. 41), cognitivist SLA emphasizes abstractness while social approaches emphasize situatedness – i.e., that which is not intended to be generalized. On the other hand, one notable cognitivist alternative makes many generalizations: conversation analysis (CA) as applied to SLA (see Markee & Kunitz, in press, for arguments against using terms such as CA-SLA or CA-for-SLA). As an example of CA's underlying disagreement with cognitivist SLA, observe the position taken by Kunitz and Markee (2014) on noticing: "In cognitive SLA, noticing is thought to facilitate learning. We are agnostic about this assumption and instead describe *how* participants orient to doing learning behavior, regardless of the accuracy of the final outcome" (p. 2). In other words, Kunitz and Markee do not deny but rather remain agnostic about cognitivist SLA's position that the locus of noticing is the mind. This claim is irrelevant for their interest in what participants (observably) do as they go about learning (whatever neurological processes are involved). The generalizations made by CA are of a very different sort from even the one by Spolsky discussed above. They have to do with *practices* used in talk. These generalizations are not laws, and Cartwright's nomological machines that function based on capacities do not apply: capacities are located in the producers of conversation rather than in conversation itself. In sum, then, CA makes generalizations but has no interest in laws, so at least on this score there is no cause for disagreement between CA and cognitivist SLA.

But we should examine the metaphysical underpinnings of the social turn more closely. Since CA embodies many characteristic social turn emphases, I will continue to use it as an example. Kasper and Wagner (2011) clearly lay out the guiding principles of CA, though they are less than perfectly clear regarding its metaphysics. The most dramatic break that CA makes with cognitivism is that it “reconceptualizes cognition as socially shared and grounded in interaction,” rather than in minds of individual learners (p. 120). If cognition is grounded in and revealed by interaction, this makes it unnecessary to “construe hidden internal processes behind observable behavior” – such as Chomsky’s language acquisition device or mental grammars (p. 121). Note that Kasper and Wagner do not categorically reject such processes; instead they leave them aside, preferring to investigate only the data that is already available without recourse to hypothesizing, intuition, or introspection. This leads to another hallmark of CA: it “breaks with the standard practice of locating studies in a theoretical framework that supplies perspectives and concepts that make some aspects of the data analytically salient and render others less conspicuous” (p. 122). In other words, it refuses to do the very thing that characterizes the modern isolationist experimental method, used by the cognitivist tradition in SLA. Cognitivism seeks to make generalizations and eventually laws *ceteris paribus*. CA seeks to understand the phenomena in all their messiness, *ceteris non paribus*. I submit that this is the crucial difference between a social approach to SLA such as CA and cognitivist SLA from Chomsky to Long, Gass, and Mackey.

One might object that on a neo-Aristotelian view, science is about the analysis of capacities and their canonical effects. If scientific knowledge is fundamentally about capacities and if we want SLA to count as a science, it would seem that the methodology of cognitivist SLA is more appropriate than that of CA, which is not interested in isolating the canonical

effects of capacities (i.e., the human capacity for second language learning). In other words, neo-Aristotelian SLA could be grist for the cognitivist mill, resembling the arguments made by Long (1997) and Gass (1998) against Firth & Wagner (1997). In response, I would suggest the following. First of all, much of neo-Aristotelian philosophy of science, particularly Cartwright's, has addressed the hard sciences, physics in particular. Physics, insofar as it studies the interactions of inanimate objects rather than those of rational agents, is very different from a discipline like SLA. Further, we have seen that capacities have both canonical effects and actual outcomes. It is well and good to focus on canonical effects to the exclusion of actual outcomes when it is possible to manipulate nomological machines to control or predict what will happen in the world (Cartwright & Pemberton, 2013).

In SLA, however, the majority of language learning does not happen in closely controlled laboratory environments. It happens in the wild, where researchers cannot manipulate nomological conditions. Due to the nature of language learning as a social activity performed by independent agents, it stands to reason that the *actual outcome* of the human capacity for language learning is at least as important as its *canonical effect*. This is contrast to a science where findings may be applied so as to create a particular nomological machine which will then function in a particular desired way. (Classrooms may be manipulated as nomological machines to some extent, but SLA encompasses far more than classroom instruction.) Cognitivist SLA, understood in neo-Aristotelian terms, is interested in the same type of isolationist experimental methodology that gives us knowledge about the canonical effects of capacities. CA, as a social variety of SLA, gives us information about the actual outcomes of capacities – information which is essential, unlike sciences such as physics, because language learning inevitably happens in uncontrolled environments. In sum, then, a neo-Aristotelian view of SLA is justified in

including the actual outcomes of human beings' underlying capacities for language learning because the phenomena of language learning cannot be understood in all their richness and complexity otherwise.

CHAPTER 5

CONCLUSION

Rares sont les mécanistes qui admettent qu'il y ait de la finalité dans la nature, rarissimes, s'ils ont jamais existé, sont les finalistes qui nient le mécanisme et sa fonction nécessaire dans les êtres naturels. . . . Normalement, le mécanisme exclut le finalisme, mais le finalisme n'exclut pas le mécanisme, au contraire il l'implique nécessairement.
—Étienne Gilson (1971, p. 171)

I have had two related aims in this work. The first is to call for a greater metaphysical awareness, as it were, in SLA. Part of this project involves demonstrating from the history of modern science that SLA owes a debt to previous thinkers rarely mentioned by researchers in the field (Jordan, 2004, being the notable exception). Bacon, for example, helped develop empiricism in scientific research, while Descartes's rationalism directly influenced the philosophy of mind underlying Chomsky's Universal Grammar and the cognitivist tradition in SLA more broadly. Hume's view of scientific laws as mere regularities, with its attendant rejection of the Aristotelian notion of final cause, became orthodoxy for most scientists and was unsurprisingly accepted by SLA researchers as well. Finally, positivism – modernism gone too far – was followed by Kuhn's postmodern, sociological, and relativist approach to science. This succession of ideas, stretching from the seventeenth to the twentieth centuries, is represented in miniature in the history of SLA, which in less than fifty years has run the gamut from Cartesian rationalism (Chomsky) to relativism (Block, 1996). I submit that the notorious exchange between Firth and Wagner (1997) and their critics (particularly Long, 1997, and Gass, 1998) is an illustration of Kuhn's incommensurability and resulted in large part from the participants' failure to examine the metatheoretical and metaphysical principles behind their disagreements (see above, Chapter 3). It is imperative to articulate not only one's position on the standard theoretical issues in the field – the language faculty, cognition, interaction, learning, identity

(Ortega, 2011) – but also on the underlying metaphysical commitments that drive those positions.

My second aim has been to argue that the fundamental disagreement between cognitivist SLA and social SLA – particularly between cognitivism as represented by Long, Gass, and other interactionists, and social SLA as represented by conversation analysts such as Markee, Kasper, and others – may be explained in terms of Cartwright’s (1999) account of scientific laws as *ceteris paribus*, along with their nomological machines. A neo-Aristotelian approach to SLA along Cartwright’s lines must begin by rejecting the Humean account of scientific laws as mere regularity. It must affirm our common-sense notion of causality as something real, and it must locate that reality in a thing’s *capacity* or nature. Successful scientific laws, far from being universal, are actually quite narrow, because their applicability is circumscribed by a particular nomological machine. If any factor in a successfully operating nomological machine is removed, the regularity identified by a given scientific law ceases. Crucially, Cartwright argues that the reason we consider “certain circumstances ideal and others not” is that in putatively ideal circumstances, we think we are able to attain knowledge about a thing’s *capacity*. The case of Genie (see above, Chapter 4), for example, tells us very little about the human capacity to acquire language due to the absence of a necessary factor for the successful operation of the nomological machine. The linguistic input Genie received was wholly inadequate, preventing her capacity for language acquisition to have its canonical effect.

Cognitivist SLA, then, is interested in the canonical effect of the human capacity for (second) language acquisition. Under ideal conditions identified by a particular theoretical approach – i.e., *ceteris paribus* – what is this capacity’s canonical effect? Social approaches to SLA, particularly CA, are interested in the messiness of the observable world *sans* exogenous

theorizing: what is second language learning *ceteris non paribus*? What are the actual outcomes of the human capacity for (second) language acquisition? This different approach to the data underlies much of the disagreement between the two research traditions. But when viewed from Cartwright's neo-Aristotelian approach to laws, that disagreement may be accounted for *without recourse to incommensurability*. This solution is at bottom metaphysical, hence my urging for greater metaphysical awareness on the part of SLA researchers. It is also realistic for both cognitivism and CA. The former is already open to positing unobservable entities (Gregg, 2003), which means that the notion of capacities is not ruled out. The latter is quite content to remain agnostic about any claims which are irrelevant to getting on with the analysis of raw data (Kunitz & Markee, 2014), which means that conversation analysts need not deny capacities even if they prefer not to affirm them. In sum, whether or not SLA makes any significant use of neo-Aristotelian philosophy of science, Cartwright's capacities and nomological machines indicate the possibility of *rapprochement* via metaphysics. And as Gilson's commentary on teleological and mechanistic views of the natural world indicates (above), the framework that can accommodate two apparently competing frameworks is to be preferred.

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ENDNOTES

¹ Jordan (2004) incorrectly attributes Descartes' example of the wax to the *Discourse on Method* (1637/1968); in fact, it appears in *Meditation 2* (1641/2013). Descartes does not argue directly from the wax to the *cogito* as Jordan implies (pp. 19-20), since they appear in two separate works.

² Long (2007), however, appears to retreat from this extreme position: "discouraging or reducing diversity (as opposed to chaos) is almost always a bad idea in any walk of life, and theoretical and methodological diversity in SLA research is likely to be no exception" (p. 164). Or perhaps the approach to SLA advocated by Firth and Wagner qualifies as "chaos" and should be discouraged.

³ For one example, take Coulomb's law (Cartwright, 1999, p. 59) according to which "The force exerted by one point charge on another acts along the line between the charges. It varies inversely as the square of the distance separating the charges and is proportional to the product of the charges. The force is repulsive if the charges have the same sign and attractive if the charges have opposite signs" (Tipler & Mosca, 2008). This requires a particular experimental apparatus to test.