Chapter 13
Reductionism and the Relation Between Chemistry and Physics

Hasok Chang

Abstract The relationship between physics and chemistry is one of the perennial foundational issues in the philosophy of chemistry. It concerns the very existence and identity of chemistry as an independent scientific discipline. Chemistry is also the most immediate territory that physics must conquer if its "imperialistic" claim to be the foundation for all sciences is to have any promise. I wish to enhance the anti-reductionist position concerning the chemistry–physics relation with three arguments inspired by the works of some leading twentieth-century chemists. (1) The very foundation of quantum chemistry is classical, and its roots go back to the organic structural chemistry of the 1860s. (2) Chemists exploit for their own purposes the conceptual resources provided by physics; this may or many not involve deducing chemical theory from physical theory. (3) Even physics itself is much more disunified than it may seem, and therefore constitutes a dubious basis for reduction as it is normally envisaged. I will also suggest that a careful consideration of the physics–chemistry relation points to some productive ways in which we can move beyond the reductionism debate as it is traditionally construed in philosophy and science.

Keywords Reductionism • Reduction • Quantum chemistry • Structural chemistry • Linus Pauling • Schrödinger's equation • Integration

13.1 Introduction

In this paper I wish to re-visit the relation between chemistry and physics, and use this case to throw some new light on the general philosophical and scientific debates on reductionism. The nature of the chemistry–physics relation is not a new issue, and it has indeed been a central concern in the philosophy of chemistry. There is much existing work on the topic, and I do not pretend in this brief paper to give a comprehensive survey of it, nor even engage with all of what I consider the

---

H. Chang (✉)
Department of History & Philosophy of Science, University of Cambridge, Cambridge, UK
e-mail: hc372@cam.ac.uk

© Springer International Publishing Switzerland 2015

T. Arabatzis et al. (eds.), *Relocating the History of Science*, Boston Studies in the Philosophy and History of Science 312, DOI 10.1007/978-3-319-14553-2_13

best work.[1] So I must begin by asking myself what I may hope to add to such a well-rehearsed debate.

My sense that there might be something useful to say originates from my study of Kostas Gavroglu's work on the history of quantum chemistry, much of it in collaboration with Ana Simões, which is conveniently collected and synthesized in their impressive recent volume *Neither Physics Nor Chemistry: A History of Quantum Chemistry* (Gavroglu and Simões 2012).[2] What Gavroglu and Simões's work has shown me is that the reductionism debate can still be significantly enriched and refreshed through a renewed attention to the history of science. So this article owes to their work not only much of its detailed content about quantum chemistry, but also the inspiration to take a more general integrated historical–philosophical look at the reductionism debate.

Before I enter into specific arguments, it makes sense to emphasize why the chemistry–physics relation is such an important issue. In the realm of scientific practice, the ambition of reductionism has most often been expressed in the form of "physics imperialism"—the attempt to take all other sciences as applications of physics. Ernest Rutherford has notoriously expressed his disdain for sciences that could not be ultimately reduced to physics: "All science is either physics or stamp collecting" (Birks 1962, 108). It may be considered fitting punishment that he was given the Nobel Prize in *chemistry* in 1908. "Biology is nothing but applied chemistry, which is nothing but applied physics" and such thoughts constitute a familiar refrain of reductionists at the lab bench and in the schoolroom.

Serious philosophers, too, have entertained exactly such thoughts. For example, in their classic reductionist manifesto, "Unity of Science as a Working Hypothesis", Paul Oppenheim and Hilary Putnam (1958, 9) express their expectation that the science of social groups will be replaced by the science of the individual multicellular organisms that constitute such groups, that the science of individual multicellular organisms will be replaced by the science of the cells that constitute such organisms, and so on, eventually down to the science of the elementary particles that ultimately make up everything. In this "imperialist" project, physics will not get anywhere if it cannot conquer the territory traditionally covered by chemistry, which may be seen as the next-door neighbor of physics in the geography of scientific disciplines.

Gavroglu and Simões remind us that some of the most important architects of quantum physics and chemistry did concern themselves with this task of reduction. Already in 1923, *before* the establishment of what we now know as the standard forms of quantum mechanics, Max Born thought he could see the way forward: "we have not penetrated far into the vast territory of chemistry; yet we have travelled far enough to see before us in the distance the passes which must be traversed before

---

[1] For a recent survey of the literature, see Hendry (2012); an earlier and more concise view is given in Hendry (2008). Most notable individual views include Scerri (2008), sec. A; van Brakel (2000), Chap. 5; and Hettema (2012).

[2] For my own and other reviews of this book, see Chang et al. (2013).

physics can impose her laws upon her sister science" (quoted in Nye 1993, 229). Some even considered it already achieved in principle almost as soon as Schrödinger's quantum mechanics was formulated. Walter Heitler, just after starting his pioneering work in early quantum chemistry published in 1927, declared in a letter to Fritz London, his collaborator in that work: "We can, then, eat Chemistry with a spoon" (quoted in Gavroglu and Simões 2012, 22). I take it that the sense of this image was that chemistry would offer so little resistance that not even a knife and a fork were required. In Paul Dirac's memorable conceit expressed in 1929: "The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble" (quoted in Gavroglu and Simões 2012, 9).

From the viewpoint of chemistry, its alleged reduction to physics is even more serious business. For the physicist, any actual failure of reduction is not so troubling: it does not threaten physics itself, and can easily be blamed on the imperfection of our mathematical techniques or the particular complexity of given systems of interest. For the chemist, however, a successful reduction of chemistry to physics would threaten to remove the very *raison d'être* of chemistry as an independent scientific discipline. But since even those quantum chemists who seem to base their work entirely on physics can happily work in chemistry departments that operate very independently from physics departments, the alleged reduction may not be such a dire concern in practice. For the philosopher of chemistry, however, there is not even this practical comfort. If chemistry really is reducible to physics and there is no independent chemistry in the intellectual sense, then there is no real need for a philosophy of chemistry. Moreover, there would not even be practical inertia pushing for its perpetuation, since philosophy of chemistry lacks the kind of institutional and industrial backing that keeps chemistry itself going. All this may explain why there *is* so little philosophy of chemistry (compared to philosophy of physics or philosophy of biology), and why most of what there is leans towards a sort of defensive anti-reductionism.

13.2 Benefits of a Historical Perspective

As announced above, one key inspiration that I have received from the work of Gavroglu and Simões is to take history more seriously in thinking about the chemistry–physics relation. Similar inspiration also comes from the work of Bernadette Bensaude-Vincent and Jonathan Simon (2012, esp. Chap. 8). The immediate insight that a look at the longer-term history gives us is that the relationship between chemistry and physics has not been a fixed thing. Knowing something about the longer-term history of this changing relationship will help us contextualize our current situation and put it into better perspective. It is a familiar point among historians of science that disciplinary labels are fluid. "Chemistry" has been a changing and evolving category, and "physics" even more so. Even when we consider periods during which each of these two disciplines was well enough defined, it is important to recognize that the boundary between them has been shifting. Some topics that are very important for the identity of each discipline have shifted between the two. Two hundred years ago heat and electricity were clearly chemical subjects (and even chemical substances), even though they were also treated by physics to the extent that there was such a thing as "physics". Atoms had to be made respectable in chemistry first through a long struggle in the nineteenth century, before physicists could begin to find useful ways of engaging with them.[3] Such boundary-shifts make it harder to say that chemistry as such can be reduced to physics as such; at best we may be able to argue that some particular phases of chemistry have been reduced to particular phases of physics.

When we take such a particularist look, at least as many non-reductive moments as reductive ones are seen in the chemistry–physics relation. The early triumphs of physics, such as those achieved by Newton, had little relevance to chemistry. Successful notions of attraction in chemistry, usually under the rubric of "affinity", were conceived without regard to any "underlying" physics; the best reductive attempt, by Claude Berthollet around 1800, was respected but abandoned by most chemists, despite Berthollet's high prestige. John Dalton's thinking was firmly grounded in physics when he proposed his atomic theory, but most chemists who took up atomism made a "chemical" version of it, dropping Dalton's notions about the sizes and shapes of atoms and the physical forces that affect their combinations. Gilbert Newton Lewis's use of electrons and electron-pairs in the explanation of chemical bonds might seem like an exemplary instance of the application of physics to chemistry, but Lewis made almost no use of the physical properties of electrons, and had no physics with which to explain why electrons would form pairs. Addressing an even more important instance of alleged reductive success, Eric Scerri (2007, 248) declares: "It is indeed something of a miracle that quantum mechanics explains the periodic table to the extent that it does at present. But we should not let this fact seduce us into believing that it is a deductive explanation." And orbitals, deemed to be unreal by any rigorous quantum mechanical reckoning, continue to have enormous importance in theoretical and experimental reasoning in chemistry. These are just a few of the most important non-reductive moments in the relatively recent history of the chemistry–physics relation.

A closer look at the history also makes it plain that the standard framework of the reductionism debate in the philosophy of science is not very helpful in thinking about the chemistry–physics relationship (or the relationship between *any* empirical sciences, for that matter). I will discuss this matter in more detail in Sect. 12.4 below, but for now consider the canonical "correspondence" view of reduction by Ernest Nagel (1961, 1974) and others. Here is one of Nagel's early formulations (1953, 541): "The objective of the reduction [of one science to another] is to show that the laws or general principles of the secondary science are simply logical

---

[3] On the futility of early atomic theories, see Chalmers (2009).

consequences of the assumptions of the primary science." As will become clearer in the discussion to follow, the full academic relationship between scientific disciplines is one between sets of practices with different objectives and values, not only (or even primarily) between sets of propositions as standard accounts of reduction would have it. Consider, for example, organic synthetic chemistry: what are its "laws or general principles"? Even if we could articulate such a set of propositions, merely stating those propositions would simply miss the point of the endeavor. Even if we look at much more theoretical and physics-friendly areas of chemistry, it is not clear that the main use of physics for chemistry has been in the deduction of chemical propositions from physical propositions.

13.3 Three Points on the Modern Chemistry–Physics Relation

The preliminaries stated above will now help me focus, reinforce and reframe three anti-reductionist points that can already be found in extant literature.

13.3.1 Quantum Chemistry Is Founded on Classical Chemistry

Anyone who wants to challenge the reduction of chemistry to physics must tackle the case of quantum chemistry head-on. Whatever the historical situation has been, it may seem that we now have a clear verdict that chemistry has been reduced to quantum physics. Modern physics tells us what atoms are, and how they combine with each other—isn't that all we need to know in chemistry? More specifically, the Schrödinger equation specifies how electrons distribute themselves around atomic nuclei, and that is the basis of all the information that chemistry requires. So the remaining challenge of chemistry is the solving of the very complicated Schrödinger equations of multi-electron systems. While chemists' actual research practices involve much else and much more than the Schrödinger equation, many chemists still pay lip-service to the idea that all of their knowledge is ultimately founded in the Schrödinger equation.

Against that naïve and dogmatic reductionism, the first and most powerful anti-reductionist point concerning quantum chemistry is that it cannot be practiced without making use of the knowledge gained in pre-quantum chemistry. Gavroglu and Simões's discussion of Linus Pauling in this context is particularly instructive. Pauling, whose pioneering and significant role in the development of quantum chemistry nobody would deny, thought of this field as a direct continuation of nineteenth-century organic structural chemistry, dubbing it “modern structural chemistry”.[4] He declared in 1970: “The theory as developed between 1852 and 1916 retains its validity . . . . It has been developed almost entirely by induction (with, in recent years, some help from the ideas of quantum mechanics developed by the physicists). It is not going to be overthrown” (Gavroglu and Simões 2012, 251).

What Pauling points out in such statements is that the quantum-chemical calculations are built on the presupposition of molecular structures, established for chemical reasons long before there was quantum mechanics, rather than vice versa. Two steps need to be distinguished in order for us to appreciate this point fully. First, as R. Guy Woolley among others have observed for many years (Sutcliffe and Woolley 2012, and references therein), the typical method of quantum-mechanical treatment of molecules begins with the Born–Oppenheimer approximation, which separates out the nuclear wavefunction from the electronic wavefunction ($\Psi_{\text{total}} = \Psi_{\text{nuclear}} \times \Psi_{\text{electronic}}$). Additionally, it is assumed that the nuclei have fixed positions in space. In this “clamping-down” approximation, the atomic nuclei are treated essentially as classical particles; as Olimpia Lombardi (2013) points out, this picture is non-quantum in a very fundamental way as the simultaneous assignment of fixed positions and fixed momenta (namely, zero) to them violates the Heisenberg uncertainty principle. But without such classical scene-setting, the quantum calculations are quite impossible.

The difficulty here is not only about the practicalities of calculation, and the clamping-down of nuclei is not merely an approximation. Aside from assuming that the nuclei are fixed, it is necessary to know *where* exactly the nuclei in question should be placed. Otherwise it is not possible to specify the potential function, which needs to be inserted into the Schrödinger equation, whose solution determines the wavefunction of the electrons in the molecule. In other words, without knowing the locations of the nuclei in the molecule, it is impossible even to *set up* (not to mention *solve*) the Schrödinger equation. So we must ask: on what basis do quantum chemists make their initial assumptions about the positions of the nuclei? The answer is: the molecular structures determined, from purely chemical reasoning, in classical structural chemistry! Initially assuming that the nuclei are fixed in their “classical” places, chemists are then able to use quantum mechanics to calculate further aspects of the molecules such as precise bond lengths and energies, and also reason about the cases when nuclei are not exactly fixed. This is the kind of progressive development that I have called “epistemic iteration”, which I think is quite common in science (Chang 2004, Chap. 5). To my knowledge, there are no cases in which an ab initio solution of a multi-electron and multi-nuclei molecular Schrödinger equation has yielded the structure of any molecule with

---

[4] This is what he calls the subject in the subtitle of Pauling (1960). See also Gavroglu and Simões (2012, 77).

any complexity to speak of. And we should not expect such an achievement, because protons and neutrons, which make up nuclei, do not even obey the Schrödinger equation.

The use of the "nucleus-clamping" approximation is a well-established point, among those who have considered the reductionism question in the context of quantum chemistry. I would like to stress an additional and slightly different fact: the very structure of Schrödinger's quantum mechanics is classical in a similar kind of way, and this is *not* something that arises because of the need for approximations in a complex multi-particle system. Look at the basic time-independent Schrödinger equation for one particle, which is the starting point of all further work in wave mechanics:

$$-\frac{\hbar^2}{2m}\frac{d^2\psi(x)}{dx^2} + V(x)\psi(x) = E\psi(x)$$

Already striking is the fact that this is an equation for just one particle, not for the interaction between two particles. This is in contrast to the basic equations of Newtonian gravitation, Coulombic electrostatics, etc., whose fundamental laws tell us *explicitly* about interactions between two bodies. In the Schrödinger equation, the interaction that the particle in question has with any other particle is only expressed indirectly through the potential function V (which forms part of the Hamiltonian H).

In classical mechanics or electrostatics, it is clear what the potential function signifies, and where its values come from: first we calculate the force on a test body due to the interactions according to the relevant force law, and integrate the force function over distance in order to obtain the potential energy. But if we ask where the potential function V inserted into the Schrödinger equation comes from, the answer is curious. Those who have the experience of suffering through basic quantum mechanics will remember things like the square potential well (infinite or finite), which are pedagogical devices for showing how the equation might be solved, not anything intended to describe the properties of real systems. And then we get the one real system whose Schrödinger equation is exactly solvable, namely the hydrogen atom. In this case V is taken to be simply the Coulomb potential that we know from classical electrostatics!

$$\mathrm{V(r)} = -\mathrm{e}^2/4\pi\varepsilon_0\mathrm{r}$$

The fundamental assumption giving rise to that potential function is not only that the force involved is the good old Coulomb electrostatic attraction, but also that the nucleus is a point-particle fixed in one position. So one might say that Schrödinger's quantum mechanics, right from its very first use for a real-life system, was *born* with the nucleus-clamping assumption. It should be stressed again that this is not something that arises from the need for approximation, but something woven into the very fabric of elementary quantum theory. The theoretical framework of Schrödinger's wave mechanics does not allow any scope for theorizing about the state of the nucleus at all.[5]

13.3.2 Physics Itself Is Not So Reductionist

My second main point about the modern chemistry–physics relation concerns the nature of modern physics itself. I will argue that physics itself is not so reductionist, and therefore it is not a suitable basis for the reduction of chemistry (or anything else). "Physics imperialists" should put their own house in order first, before trying to take over any other sciences. The first pertinent observation here is that there are many branches of physics itself that are not reduced to the most fundamental theories of physics.[6] In most areas of condensed matter physics, mid-level concepts such as Cooper pairs or phonons have been necessary in order to explain important novel phenomena such as superconductivity and semiconductivity. It has also not been trivial to apply the advanced theories of microphysics to mundane macroscopic phenomena; consider, for example, the task of giving a quantum-mechanical account of the absorption of an electron by a metallic surface.

We should also not forget that the most fundamental theories of physics are also not unified with each other. Quantum gravity is still a project very much in progress, with no clear promise that it will be concluded successfully. I am not claiming that the ultimate unification of physics will never be successful. No one can be confident in such predictions about the long future of science, and concerning the plausibility of an ambitious goal (such as the grand unification of all fundamental physics), the burden of argument is on those who are actually pursuing that goal. The point is not to *presume* that unification will be successful, and not to reason entirely on the basis of such a presumption. In the here-and-now of physics, it is important to note that many important and successful uses of physics draw from various parts and levels of physics, which have not been unified with each other in general. A good example illustrating that point is the global positioning system (GPS), which draws from Newtonian mechanics (for satellites), quantum mechanics (for atomic clocks), and special and general relativity (for the correction of atomic clocks), without any attempt to unify those theories on the whole.

Returning to the chemistry–physics relation more specifically, a key point to note is that chemistry does *not* use the most fundamental and up-to-date theories of physics. Trying to use quantum chromodynamics or superstring theory to do

---

[5] Incidentally, it is also important to remember how *parochial* the original remit of the Schrödinger equation was. It is a grave mistake, committed by Schrödinger himself among others, to imagine that it is some sort of a fundamental law that can tell us about all of the universe. That is where the philosophy of physics in the twentieth century went seriously wrong.

[6] A classic statement of anti-reductionism by a renowned working physicist is Anderson (1972).

chemistry would be absurd, and even trying to use quantum field theory would be quite futile. The greatest benefit for chemistry comes from using the physics of a century ago. Somehow, Bohr in 1913 and Schrödinger in 1926 (and not Heisenberg in 1925) hit the right notes for chemistry. In this context, Schrödinger's theory is not simply an approximation to Dirac's, Feynman's, or Yang and Mills's, or any other more advanced theory. In fact we do not have sufficient evidence that using more advanced physics for chemistry would give us better results, even if we could handle the mathematics. And it is also not the case that Schrödinger's theory is at an emergent level of ontology in relation to more fundamental theories of physics. It deals with the same level as the more advanced quantum theories, at least when it comes to electrons.

13.3.3 Chemists Exploit Physics

My third main point concerning the modern chemistry–physics relation, already hinted above, is that chemists *use* physics in their practice but they do not surrender or submit to it (contrary to what some physicists may imagine). This is the case even for many of the chemists who pay lip-service to the fundamentality of quantum mechanics for their enterprise, and who may subscribe to reductionism in their more philosophical moments. I would even say that chemists *exploit* physics (by which I do not mean that they necessarily exploit *physicists*.)

A longer-term historical perspective is helpful here. For at least two centuries chemists have made good use of physics, without thereby turning into physicists or turning their subject into physics: we can think about Lavoisier's use of weights, Davy and Berzelius's use of electrostatic theory to explain chemical combinations, Dulong and Petit's use of specific heat measurements to help determine molecular formulas, organic chemists' use of melting and boiling points to help them distinguish similar substances, and so on.

When we consider twentieth-century chemistry, Linus Pauling again gives us a very useful clue.[7] Pauling's great success in shaping the early directions of work in quantum chemistry was due to his ability to *use* quantum-mechanical ideas to help him do better *chemistry*, rather than turning chemistry into physics in any real sense. It may have been the focus of some early pioneers of quantum chemistry, such as Heitler and London, to treat chemical systems as exercises in quantum physics. But it is important to note that this was typically not the viewpoint of the later and more successful quantum chemists. Gavroglu and Simões tell us that Pauling's emphasis on the tradition of structural theory was due to its continuing

---

[7] In addition to the work of Gavroglu and Simões, I have benefited greatly from the informative article by Martha Harris (2008) on Pauling, Lewis and the chemical bond.

importance in organic chemistry, and its newfound usefulness in the applications of chemistry in biology and medicine—one is reminded of his great success in elucidating the alpha-helix structure of proteins and his role in the race to solve the problem of DNA structure. In the "integration" of the sciences that he advocated, to be achieved through the sharing of tools and methods, Pauling saw chemistry, not physics, as occupying the central place (Gavroglu and Simões 2012, 119). It is significant that Pauling dedicated his masterpiece *The Nature of the Chemical Bond* (Pauling 1960) to Lewis, whose pioneering work on electron-based conceptions of chemical bonding (the cube/octet and the pairing of electrons) were based on no detailed theories of physics and created before the arrival of full-fledged quantum mechanics (see Arabatzis 2006, Chap. 7).

Pauling's anti-reductionism concerning the chemistry–physics relation was shared by Charles Alfred Coulson, the leading pioneer of quantum chemistry in Britain, who said in 1970 that "one of the primary tasks of the chemists during the initial stages in the development of quantum chemistry was to escape from the thought forms of the physicists" (Gavroglu and Simões 2012, 1). While clearly deploying theoretical resources borrowed from quantum mechanics in his chemical work, Coulson thought it was important not to think entirely like the physicists who developed those resources. Generally speaking, while most twentieth-century chemists accepted that the momentous developments in physics had some serious implications for the practice of chemistry, there were a variety of ways in which that relevance of physics to chemistry was understood and developed.

A general note might be useful here, about the notion of "fundamentality". It is a mistake to think that being "fundamental" implies having higher status or importance—literally, it should mean being lower! Seriously, and less metaphorically, what does it mean to be "fundamental"? That is not easy to state unequivocally, but one main sense is that if A is fundamental to B, then A is necessary for B. There are many forms of "necessity": A may be a material requirement for B to be possible (say, the foundation of a building in relation to the rest of it); A may be a necessary tool to enable B (say, the calculus for Newtonian celestial mechanics, or the hammer for hanging a picture on the wall); or A may be something one must know before one can learn B (say, arithmetic for algebra). In all these cases, B may be the really important thing or activity, supported by A. That was probably the notion that Pauling and others had about chemistry (B) in relation to physics (A). In Korea, where I grew up, there is a traditional saying that the farmer is the foundation of heaven and earth (of society, less grandiosely). That does not at all mean that traditional Korean society actually gave high status or paid much respect to farmers; it only meant that the rulers recognized clearly that farming was fundamental and without it the whole society would collapse. That is not quite the image that physicists have when they boast that physics is a more fundamental science than chemistry. When reductionists say that physics is fundamental to chemistry, they tend to make an unspoken assumption that physical theory is sufficient to tell us everything that chemical theory says. If we look at the practice of chemical theory and experiment, it becomes obvious that physical theory is necessary but not sufficient for performing what chemists want to achieve.

13.4 Beyond Reduction: Philosophical Viewpoints

I would now like to take a slight step back from the specific points that I have made about the modern chemistry–physics relation, and articulate some general philosophical viewpoints that will render the points made so far both more convincing and more deeply meaningful.

13.4.1 Aims of Chemical and Physical Activities

Implicit in my discussion so far has been the inclination to look at what chemists *do* with physics, rather than focusing on the logical relationship between the propositions contained in physics and the propositions contained in chemistry. It is my general philosophical ambition to understand scientific practice in a fuller sense, rather than just focusing on the propositions involved in it and scientists' belief in them. (This is one of the general ways in which I try to improve on standard Anglophone philosophy of science of the last several decades.) Practice consists of various epistemic activities, and these include the activities that constitute theorizing. But *theorizing* (as opposed to *theory*) is a very different thing from simply believing or not believing certain propositions, and involves a lot more than the postulation of propositions and the deduction of propositions from each other.

One of the most important things to examine when one looks at scientific practices, and what one misses by only paying attention to propositions and beliefs, is the aims that scientists are trying to achieve in their epistemic activities. Looking at the relation between chemistry and physics, we must ask whether chemists and physicists have significantly different aims in their practices. If so, chemistry and physics do not at all become the same thing even if their practices should involve exactly the same propositions. My view is that the aims of physics and chemistry are diverse and overlapping, but not identical enough to warrant a true merger of the two fields.

Chemistry has a broad range of interrelated aims, which do not seem to map neatly on to the aims of physics. One point that is very often noted, at least ever since Marcellin Berthelot's famous statement that chemistry "creates its objects", is that *making* has been a preoccupation of chemistry (Bensaude-Vincent and Simon 2012, Chap. 6). This is not only about the synthesis of new substances. On the one hand it also involves making substances that are already well known, and on the other hand there is also a focus on the creation of new processes and reactions. As well as synthesis, let's not forget about the venerable old aim of *analysis*, or more broadly the *identification* of substances, whether it is done by means of chemical reagents, nuclear magnetic resonance, or anything else; the other side of the analytical coin is the study of the properties of the identified substances, which remains a bedrock activity of chemistry. And the *classification* of the substances so characterized has been an important aim of chemistry over the centuries, even though it is now widely considered a straightforward and pedestrian business.

Do the aims of physics differ radically from these aims of chemistry? This is a complicated question. The usual perceptions of the difference here are based on an extreme and impoverished view of what physics practice is really all about, such as the following by Steven Weinberg (1994, 6): "Our present theories are of only limited validity, still tentative and incomplete. But behind them now and then we catch glimpses of a final theory, one that would be of unlimited validity and entirely satisfying in its completeness and consistency." Even though there is certainly that desire for grand unified theories among at least some physicists, physics also very much shares the aims of chemistry that I have discussed above. However, we should also note that there are some important differences in how the aims of physics and chemistry are concretely manifested, because of the differences in subject matter, methodology, and emphasis in the two fields.

13.4.2 Reduction as an Aspect of Inter-System Relation

The question of reduction should be understood as one aspect of a broader and more complex question concerning the relationship holding between two systems of practice.[8] The starting point of my discussion here will be a recognition that there are different ways in which two systems can relate to each other. Looking at the various types of inter-system relations, we can ask which of those relations may incorporate something like reduction.

Elsewhere, in my discussion of pluralism in science (Chang 2012, Chap. 5), I distinguished three modes of interaction between co-existing systems of practice: integration, co-optation, and competition. These are real and distinct and beneficial modes of interaction, which should be considered as seriously as the reductionist holy grail of unification. And within the category of what is colloquially called "unification", there are different types of events, which I might call "merger" and "acquisition". In a merger, two systems come together on an equal footing and make a new system that is more general. A good example would be how the work of Michael Faraday and then James Clerk Maxwell brought together the previously separate sciences of electricity and magnetism. In what I call acquisition there is an unequal relationship that takes place between two systems; one is dissolved, and absorbed into the other. This is what happened, for example, when Kepler's astronomy of planetary orbits got absorbed into Newton's gravitational physics. Unification, either by merger or acquisition, is different from what I mean by "integration" (akin to what Otto Neurath called the "orchestration of the sciences"), in which the interacting systems remain intact in themselves but they are brought

---

[8] What I mean by a "system of practice" is defined in Chang (2012, Chap. 1), and the notion provides an analytic framework for the entire work.

together in joint application to address a specific problem. The construction of the global positioning system, mentioned above, is a good example of integration. In co-optation, isolated elements from one system are adopted by another for its own purposes. For example, Lavoisier adopted Priestley's production of de-phlogisticated air and Cavendish's production of water from the combustion of inflammable air, and turned those results from phlogiston chemistry into key components of his own system. In the relation of competition, the competing systems affect each other's behavior without adopting specific content from each other. We might say that the competition between catastrophism and uniformitarianism in geology exhibited this pattern.

Now, having distinguished the five modes of inter-system relation, we can ask which modes describe best the relation between chemistry and physics. The traditional view might be that physics has now acquired chemistry. I think the various points I have made and referred to so far are sufficient to discredit this view; all in all, the epistemic activities of chemists remain very distinct from those of physicists, and it is not at all the case that chemistry has become part of physics as a practice. I think Martha Harris (2008) is correct in saying that in the early to mid-20th century quantum chemistry has created a synthesis (or, hybrid) of chemistry and physics (rather than reduction). But in what exactly did this synthesis consist?

Pauling's remarks quoted above suggest that what happened in the development of quantum chemistry was a case of chemistry co-opting elements from physics. There is also a serious question to be raised, regarding whether there was a co-optation of results from chemistry by physics; it could be argued, for example, that the development of the concept of spin in physics owed a good deal to the co-optation of Lewis's electron-pair idea. But the other direction of co-optation is more obvious. If we look back at Lewis's practice it is quite obvious that he takes the discovery of the electron from experimental physics, and makes use of it in his chemistry in ways that did not relate at all to any credible physics of the electron.

Concerning the more mature phases of quantum chemistry, however, my view is that what we have is a case of integration, between classical structural chemistry and the Schrödinger formulation of quantum mechanics. It is more than co-optation (of physics by chemistry), as the kinds of questions addressed in quantum chemistry go well beyond the kinds of questions raised in classical chemistry. It is not acquisition (of chemistry by physics), because of the fact that classical structural chemistry remains a separate and independent system of practice (though no longer an active research field), and provides those essential nuclear positions in the molecules that we wish to study using quantum mechanics. Rather, what we have is the bringing together of the two systems in order to handle a particular class of problems. And "quantum chemistry" is a bit of a misnomer, because it suggests that this field of study treats all of the chemical questions conceived in the quantum mode. But if we consider other areas of chemistry to which quantum considerations might be relevant, for example electrochemistry and solution chemistry, it is evident that we require a different synthesis, at least because thermodynamics has to enter the picture in an essential way. This is, of course, only a reminder that I should never have conceived my topic so broadly as to claim to comment on “the relation between chemistry and physics”!

Finally we can come back to the question of reduction: does the relation of reduction hold within these modes of inter-system relation? Here I am staying with the classic correspondence view of reduction, and asking if the key theoretical propositions of one system can be deduced from the key theoretical propositions of the other system. I will accept, without pedantic questioning for the moment, that when we are tempted to call a development “unification” (merger or acquisition) there is reduction being achieved. If what we have is acquisition, then the propositions of the acquired system would be deducible from the propositions of the acquiring system. In merger, the propositions of either system would be deducible from the propositions of the new system formed by merger. At the other end of the spectrum, there would be no reduction expected within competition. There would also not be wholesale reduction in integration or co-optation, because the two systems remain separate and independent. However, interesting questions can be raised regarding the possibility of partial reduction. In the case of integration, can a significant portion of the propositions (at least in some specific version) in either of the two systems be deduced from propositions in the hybrid system? In that case it could be considered that a certain truncated version of the initial system has been reduced to the hybrid system. A similar question can be raised for co-optation, too: can the co-opted elements (if they are propositional) be deduced back from the propositions of the co-opting system? These questions provide subjects for future research.

13.4.3 Reduction Versus Replacement

As the discussion above should make clear, I am not interested in making a dogmatic claim against the possibility or actuality of reduction in the relation between various scientific systems of practice. In fact I do think that reduction, when it happens, should be considered an achievement. This is because the various inter-system interactions that may incorporate reduction have their benefits. These benefits are conceptualized in the context of my arguments for pluralism in science, which have been elaborated elsewhere (Chang 2012, Chap. 5, esp. 279–284). However, we can only make a proper appreciation of reduction if we get rid of one common major misconception. This is the assumption that when one theory has been reduced by another, the reduced theory should now be discarded. There is also a related assumption that when two systems of practice have been unified, there should be only one system remaining as a result.

One overall point that my perspective on the issue should make clear is that the logical relation between two theories (or sets of propositions) is not at all the same as the broader relation between the systems of practice in which the theories are respectively embedded. So, even if a reduction relation holds and the reduced theory may be deemed redundant in its content, there may well be good reasons not to abandon the system of practice that incorporates the reduced theory in question. One clear reason would be that the different systems of practice involved fulfill different kinds of aims.

But there are also reasons to retain a system of practice associated with a reduced theory even if the two systems pursue the same kind of aims. It can easily happen that different systems of practice can satisfy the same kind of aim, but in different ways. For example, there are different formulations of classical mechanics (Newtonian, Lagrangian, Hamiltonian, Hertzian) that express the same basic content and address the same kind of physical situations, but that are suited for different types of problems and that give us different kinds of intuitive understanding. This is also the case for different formulations of quantum mechanics (Heisenberg, Schrödinger, Dirac, Feynman). And such alternatives can develop in very different and differently fruitful ways. For example, the Newtonian formulation of classical mechanics could not have provided a formal framework suited for quantum mechanics the way the Hamiltonian, and then the Lagrangian formulation did. So it would have been a very foolish thing to discard Hamiltonian mechanics just because it can be reduced to Newtonian mechanics (or vice versa). When we do have really quite different systems, it may still happen that they satisfy the same aim to similar degrees. They can also set up a competition that is beneficial, keeping up a critical awareness of alternatives and preventing a sense of complacency.

These considerations lead to a more fail-safe recommendation: when reduction happens, there should be no presumption that the reduced theory ought to be discarded. Instead, we should ask what functions the reduced theory has served, and if those functions can all be served by the reducing theory. Beyond that, we should ask if it is clear that the reduced theory has no hope of being used in hitherto unknown systems of practice that will deliver new benefits. And we should also ask if we can be reasonably certain that all the actual and potential systems associated with the reduced theory will not stimulate other systems positively by competition. If the answer is "yes" to all these questions, then we may consider discarding the reduced theory *if* it is too costly to maintain it.

This stance is consonant with a position that I have called "conservationist pluralism": retain previously successful theories and paradigms (systems of practice) for what they were (and are still) good at, and *add* new ones that will help us make new and fresh contacts with reality (Chang 2012, 218). The main idea here is that if a system of practice was once adopted as a good one by a group of serious, honest and self-critical scientists, then it is likely to continue to serve the useful functions that it once served, unless nature actually changes in a radical way. (This is the same practical assumption we make in everyday life, even though we know about the problem of induction.) Recall Pauling's confidence: "The [structural] theory as developed between 1852 and 1916…is not going to be overthrown" (quoted in Gavroglu and Simões 2012, 251). The point is that we should not discard something as good as nineteenth-century structural theory, even though we have the option to do so. A good system of practice is difficult to create; we should not hastily throw it away when another one comes along, because the old and the new are not likely to do exactly the same work, so keeping the old is not a waste of effort and resources. This is why scientists have retained Newtonian mechanics in physics and orbitals in chemistry, etc., despite paying ideological lip service to reductionism and saying that Newtonian mechanics is really incorrect and orbitals do not really exist.

13.5 Concluding Thoughts

Briefly in conclusion: I think it is time to get beyond the reductionism debate, without dismissing it as unimportant or irrelevant. If we recognize and cultivate reduction as merely one of the possible productive modes of interaction between different fields of study and between different systems of practice within a given field, we will see that there is a great deal of philosophical, historical and scientific work to do—in recognizing different varieties of those interactions, studying how specific cases of interaction worked out, and how they might be improved.

I also hope that this paper can serve as an instance of the idea that paying proper attention to chemistry may help us gain a new philosophy of science. I think it is especially useful to look back at physics from the new perspective gained from our consideration of chemistry, as it will help us go beyond some distortions that were introduced to philosophy of science when a particular perspective on physics became dominant within it. This is in accord with Gaston Bachelard's proposal for a re-orientation of philosophy of science as "metachemistry", remedying the shortcomings of "metaphysics".[9]

Acknowledgments For helpful comments and discussions on earlier versions of this paper I would like to thank Ana Simões, and Lucía Lewowicz, Olimpia Lombardi and other participants of the 2013 Summer Symposium of the International Society for the Philosophy of Chemistry in Montevideo, and members of the Philosophy of Chemistry graduate seminar at Cambridge. I would also like to thank the following people for all I have learned from them personally on topics relevant to this paper: Robin Hendry, Eric Scerri, Joachim Schummer, and Rom Harré.

References

Anderson, Philip W. 1972. More is different. *Science* (new series) 177(4047): 393–396.

Arabatzis, Theodore. 2006. *Representing electrons: A biographical approach to theoretical entities*. Chicago: University of Chicago Press.

Bensaude-Vincent, Bernadette, and Jonathan Simon. 2012. *Chemistry: The impure science*, 2nd ed. London: Imperial College Press. 1st ed. 2008.

Birks, John Betteley (ed.). 1962. *Rutherford at Manchester*. London: Heywood.

---

[9] See the exposition and development of this idea in Nordmann (2006); see also Chang et al. (2010).

Chalmers, Alan. 2009. *The scientist's atom and the philosopher's stone: How science succeeded and philosophy failed to gain knowledge of atoms*. Dordrecht: Springer.

Chang, Hasok. 2004. *Inventing temperature: Measurement and scientific progress*. New York: Oxford University Press.

Chang, Hasok. 2012. *Is water H_2O? Evidence, realism and pluralism*. Dordrecht: Springer.

Chang, Hasok, Alfred Nordmann, Bernadette Bensaude-Vincent, and Jonathan Simon. 2010. Ask not what philosophy can do for chemistry, but what chemistry can do for philosophy (review symposium on *Chemistry: The impure science* by Bensaude-Vincent and Simon, 1st ed.). *Metascience* 19: 373–383.

Chang, Hasok, Jeremiah James, Paul Needham, Kostas Gavroglu, and Ana Simões. 2013. Historical and philosophical perspectives on quantum chemistry, (review symposium on *Neither physics nor chemistry: A history of quantum chemistry* by Gavroglu and Simões). *Metascience* 22: 523–544.

Gavroglu, Kostas, and Ana Simões. 2012. *Neither physics nor chemistry: A history of quantum chemistry*. Cambridge, MA: MIT Press.

Harris, Martha L. 2008. Chemical reductionism revisited: Lewis, Pauling and the physico–chemical nature of the chemical bond. *Studies in History and Philosophy of Science* 39: 78–90.

Hendry, Robin Findlay. 2008. Chemistry. In *The Routledge companion to philosophy of science*, ed. Stathis Psillos and Martin Curd, 520–530. London/New York: Routledge.

Hendry, Robin Findlay. 2012. Reduction, emergence and physicalism. In Woody et al. (2012), 367–386.

Hettema, Hinne. 2012. *Reducing chemistry to physics: Limits, models, consequences*. Groningen: Rijksuniversiteit Groningen.

Lombardi, Olimpia. 2013. Chemistry and physics: Reduction or inter-theory links?, presentation at the Summer Symposium of the International Society for the Philosophy of Chemistry, Montevideo, Uruguay, 31 July 2013.

Nagel, Ernest. 1953. The meaning of reduction in the natural sciences. In *Readings in philosophy of science*, ed. Philip P. Wiener, 531–549. New York: Charles Scribner's Sons.

Nagel, Ernest. 1961. *The structure of science: Problems in the logic of scientific explanation*, Chap. 11. New York: Harcourt, Brace & World.

Nagel, Ernest. 1974. Issues in the logic of reductive explanations. In *Teleology revisited*, 95–113. New York: Columbia University Press.

Nordmann, Alfred. 2006. From metaphysics to metachemistry. In *Philosophy of chemistry: Synthesis of a new discipline*, ed. Davis Baird, Eric Scerri, and Lee McIntyre. Dordrecht: Springer.

Nye, Mary Jo. 1993. *From chemical philosophy to theoretical chemistry: Dynamics of matter and dynamics of disciplines, 1800–1950*. Berkeley/Los Angeles: University of California Press.

Oppenheim, Paul, and Hilary Putnam. 1958. Unity of science as a working hypothesis. In *Concepts, theories, and the mind-body problem*, ed. H. Feigl, M. Scriven, and G. Maxwell, 3–36. Minneapolis: University of Minnesota Press.

Pauling, Linus. 1960. *The nature of the chemical bond and the structure of molecules and crystals: An introduction to modern structural chemistry*, 3rd ed. Ithaca: Cornell University Press.

Scerri, Eric R. 2007. *The periodic table: Its story and its significance*. New York: Oxford University Press.

Scerri, Eric R. 2008. *Collected papers in the philosophy of chemistry*. London: Imperial College Press.

Sutcliffe, Brian T., and R. Guy Woolley. 2012. Atoms and molecules in classical chemistry and quantum mechanics. In Woody et al. (2012), 387–426.

van Brakel, Jaap. 2000. *Philosophy of chemistry: Between the manifest and the scientific image*. Leuven: Leuven University Press.

Weinberg, Stephen. 1994. *Dreams of a final theory*. New York: Vintage Books, first published in 1992.

Woody, Andrea, Robin F. Hendry, and Paul Needham (eds.). 2012. *Philosophy of chemistry*. Amsterdam: Elsevier.