Hindsight, Tradition, and National Styles in the Prehistory of the Leyden Jar

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Abstract: The Leyden jar surprised natural philosophers when it was invented in 1746 because it showed the paradoxical behavior of glass. But similar experiments had been done before, such as those by the Frenchman Charles Dufay. Why was Dufay oblivious to the paradox? The answer involves three kinds of historical explanation that are controversial but fruitful: hindsight, tradition, and national styles.

Take a glass jar, fill it with water, and insert a metal rod in its mouth. Holding the jar in one hand, connect the rod to a source of static electricity. Now touch the metal rod with your free hand. The result, as a range of European natural philosophers discovered in 1746, is a nasty shock.

The shock was intellectual as well as physical. At first, the glass walls of the jar impeded the flow of electrical matter, confining it to the jar; then, all of a sudden, the electrical matter shot through the glass walls and into the body of the experimenter. The glass jar both did, and did not, transmit electrical matter. How could this be? The eighteenth-century answer, provided by Benjamin Franklin and others, was that the forces exerted by electrical matter can exist in places where the electrical matter does not. The forces pass through glass while the matter stays put. To cut a long story short, the Leyden experiment helps to explain the formation of the concept of electric charge.

This story is a little too short, however. The paradoxical behavior of electrical materials had been known for more than a decade before the Leyden experiment arrived on the scene. Sometime around 1730, the French aristocrat Charles Dufay held a metal plate between a charged glass tube and pieces of gold leaf. The gold leaf did not move. The electrical matter must have been *blocked by the metal plate*. Yet Dufay also had great trouble charging a piece of glass that stood on a metal stand. The electrical matter seemed to be *lost through the metal stand*. Metals both did, and did not, transmit electrical matter. Dufay reported these experiments to the Paris Academy of Sciences in 1733. Yet he did not find anything paradoxical about them. Why?

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The answer to this question involves an exotic theory of electricity, a collection of precious stones, and a quirk of French science. The prehistory of the Leyden jar also involves three kinds of historical explanation that are controversial but fruitful: hindsight, tradition, and national styles.[1]

HINDSIGHT

The reason Dufay did not see a paradox in the behavior of metals is that his theory of electricity was not the same as ours. The phrases in italics above—"blocked by the metal plate," "lost through the metal stand"—are not Dufay's. They are natural phrases for us to use because the fundamental distinction we make between electrical materials is between "conductors" and "insulators," between materials that give free passage to electricity and those that do not. Hence the paradox: the metal stand gives free passage but the metal plate does not. For Dufay, however, the fundamental contrast was between "contractors" and "noncontractors," between materials that receive a lot of electricity from charged bodies and those that receive little. He pictured metals as sponges, objects that soak up a great deal of electricity and hold it in place. On this theory, the paradox vanishes. The metal plate soaks up electrical matter that would otherwise be available to move the gold leaf. The metal stand behaves *in exactly the same way*: it soaks up electrical matter that would otherwise fill the piece of amber sitting on it. Dufay did not see a paradox in the behavior of metals because he thought of metals as sponges for electricity, not as conduits of electricity.

This explanation will look fishy to some readers of this journal. Even the question—Why did Dufay fail to notice that metals behave paradoxically?—may seem suspect. One might as well ask why Aristotle failed to articulate the law of rectilinear inertia or why there was no scientific revolution in China in the seventeenth century. Historians of science asked such questions in the past, the argument goes, but right-thinking historians no longer do.

I'm not so sure. In everyday life, we are perfectly capable of asking, and answering, questions about what did not happen. Why did my house not burn down today? Because the fire alarm alerted me to the smoke from the frying pan. Why did the student fail to hand in the assignment on time? Because he slept through his alarm (or so he says). We are capable of explaining even the most improbable nonevents. There are many reasons why I have not traveled to the center of the Sun: the dearth of commercial spaceflight, my modest personal fortune, the physiological challenges of living in extreme temperatures, and so on.

Why then are historians of science wary of asking why such-and-such an event did not happen? Partly, I suspect, because we often deal with mental events. Nonevents are already ethereal entities; nonevents that take place only in the mind are positively ghostly. But the bigger worry is hindsight. We are reluctant to ask why Aristotle did not invent inertia because inertia eventually *was* invented, by Descartes and company, and we are wary of projecting Descartes's ideas on Aristotle. If we have Descartes on our minds when we think about Aristotle, the worry goes, we are bound to conflate the two. And if we ask why Aristotle was *not* Descartes, we are halfway to saying that Aristotle *should* have been Descartes. Our language betrays us: the question "Why did Aristotle fail to invent inertia?" implies that Aristotle failed whereas Descartes succeeded.

These worries are well meaning but spurious. We are perfectly capable of holding the ideas "Aristotle" and "Plato" in our minds without conflating the two. We are equally capable of saying

[1] The empirical part of this essay is based on Michael Bycroft, "Physics and Natural History in the Eighteenth Century: The Case of Charles Dufay" (Ph.D. diss., Univ. Cambridge, 2013); and Bycroft, "Experiments on Collections at the Royal Society of London and the Paris Academy of Sciences, 1660–1740," in *Instituting the Sciences in Early Modern Europe*, ed. Giulia Giannini and Mordechai Feingold (Leiden: Brill, forthcoming in 2019).

"Aristotle was *not* born in Athens" without automatically thinking "Aristotle *should have been* born in Athens." We do not suddenly lose these capacities when we bring Descartes and inertia into the picture. Even if we did lose them, the solution would not be to keep Descartes and inertia out of the picture but to become more disciplined—by phrasing our questions in neutral language, for example. If hindsight is rose-tinted, the solution is to take off the rose-tinted glasses, not to shut our eyes.

TRADITION

Why did Dufay develop the contraction theory, the theory that metals behave like sponges? Because, I suggest, he was part of a tradition of material-driven experimentation that had flourished at the Paris Academy of Sciences since the 1660s. It is easy to be skeptical about traditions, those vague but weighty entities that historians sometimes invoke to explain the behavior of individuals and institutions. Traditions may seem as fanciful as *Zeitgeister*, dialectics, and arcs of history bending toward justice. But traditions can explain things if we are precise about what the tradition is and what it explains.

The French tradition I have in mind was exemplified in the plant project, a decades-long attempt to identify the medically useful substances in plants. The project had three elements: a large collection of plants, most of them grown on a plot created for the purpose at the Jardin du Roi; a uniform experimental procedure, in which the same set of macerations and distillations was applied to each plant, mostly at the academy's laboratory; and an attempt to compare the results of these trials. The aim was to come up with statements of the form "This group of plants behaves like this, whereas this other group behaves like that." An example: "All aromatic plants gave an essential oil, and almost none of the other plants gave one."

The plant project ran from 1668 to around 1700, in parallel with an analogous project on mineral waters. After 1700, the naturalist René Réaumur applied the same procedure to minerals. Dufay followed suit in the 1720s and 1730s. He built up a large collection of precious stones and experimented furiously upon them. The results appeared in papers on phosphorescence, artificial gems, double refraction—and electricity. It was to gems that Dufay first applied his basic distinction between bodies that are easy to electrify by friction (jaspers, opaque agates, the hardest marbles) and those that are hard to electrify (building stones). It was Dufay's habit of systematically testing different materials, and comparing their behavior, that led him to his fundamental theory about contraction—that is, that the bodies that are easiest to electrify by friction are the least apt to contract electricity from a second body. Electrics behave like this, nonelectrics like that.

How does the tradition explain the theory? Partly through a chain of smaller explanations: Dufay experimented like this because Réaumur did; Réaumur did because his predecessors had done so. Partly, also, through shared circumstances, which help to explain why the tradition persisted. The plant project was possible because the academicians had access to the plants at the royal garden; Réaumur's mineral project was possible because he, too, had access to Crown resources—namely, the minerals sent to Paris at the request of the regent of France. Dufay's entry into the tradition is also part of the explanation, though the lines of causation are complicated. Did Dufay enter the academy because he was good at experimenting on materials? Or was he good at experimenting on materials because he entered the academy? There is something to be said for both answers. Dufay entered the academy, in part, because he had already been doing material-driven experiments on Chinese lacquer, experiments that would have appealed to Réaumur. At the same time, being in the academy brought him closer to Réaumur, whose experiments on gems, phosphors, and electricity anticipated Dufay's own. The tradition, embodied in Réaumur, pushed Dufay in certain directions. And it kept pushing as Dufay went along—insights from Réaumur can be found in several of Dufay's electrical papers.

Traditions are suspect because, like gases, they are diffuse and invisible. But, also like gases, they are made up of myriad small interactions that make a difference in the aggregate.

NATIONAL STYLES

Dufay came up with the contraction theory partly because he was French. This may not sound like a very promising explanation, especially in 2019, when historians are less interested in nations than in the networks that joined nations together. But consider the following.

First, there is a long and distinguished tradition of contrasting the French and English scientific styles in the decades around 1700. Voltaire, Pierre Duhem, Thomas Kuhn, Lorraine Daston, Peter Dear, and Larry Holmes are all part of this tradition.

Next, there were no English equivalents to the projects on plants, mineral waters, and minerals that I have described. Consider minerals. There were large mineral collections in England in this period, notably those of John Woodward and Hans Sloane. There were also many notable experimenters. But where is the English Réaumur, the person who took a large mineral collection and did systematic trials on each of the items therein? Not Robert Hooke, Isaac Newton, Stephen Gray, or Francis Hauksbee, who all used minerals at one point or another but not in the way Réaumur and Dufay did. It is true that Robert Boyle did a great many trials on precious stones. But he never took all his gems and did the same thing to each one. Instead, he used each gem as and when it suited whatever argument he wished to make at the time.

This difference is not a coincidence. It can be explained by well-documented differences between the Academy of Sciences and the Royal Society. The former was under direct royal control, meaning that the academicians had access to large, stable, disposable collections, such as the plants at the king's garden and the minerals collected by the regent. The plant project relied on the long-term collaboration of gardeners, naturalists, and chemists, a feat that was harder to achieve at the Royal Society, with its dispersed and fluctuating membership. And the academy's sponsors rewarded utility more than spectacle, whereas the society's sponsors had the opposite preference. Francis Hauksbee would not have survived for long as the Royal Society's curator of experiments if he had nothing to entertain the Fellows but one distilled plant after another.

The fourth consideration is Dufay's work on electricity. Dufay was an excellent networker. He traveled to London twice in the 1730s. He corresponded with the Englishmen Hans Sloane and Granville Wheler. He kept abreast of the electrical experiments of his English counterpart Stephen Gray, and he summarized his own findings in English in the *Philosophical Transactions*. Yet despite all this border-crossing, his papers on electricity are cut from a different cloth than Gray's or Hauksbee's. To put it simply: Dufay focused on materials, the Englishmen on instruments. Gray was interested in conductors because they sent electricity over long distances, Dufay because they allowed him to compare the behavior of metal wires, wood sticks, and lines of thread. Hauksbee's experiments on electricity were driven by the electrostatic generator and the air-pump, Dufay's by pieces of amber, glass, wood, metal, and rock crystal. When Hauksbee filled a glass tube with sand, he saw the sand as a mechanical device for excluding air from the tube. When Dufay replicated this experiment, he saw the sand as a material to be compared to other materials, such as water and bran. The flow of people and texts across the Channel did not end the distinctiveness of French science but put it into sharp relief. If Dufay had been born in London, not Paris, would he have come up with the contraction theory? Maybe, but I doubt it. National styles, like traditions and hindsight, still have something to offer the historian of science who is interested in explanation.