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CLOSED CIRCLES OR OPEN NETWORKS?: COMMUNICATING AT A DISTANCE DURING THE SCIENTIFIC REVOLUTION

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As many historians have remarked, the new philosophy of the seventeenth century self-consciously privileged the depiction and description of things and events over discussion and debate between antagonists. But new "matters of fact" were often unpredictable, strange, or anomalous, things that could never have been constructed from learned argument.[1] Because they could be discovered only through observation or investigation, the truth or error of reports about new matters of fact could not be judged according to reason alone. Establishing new knowledge about nature therefore depended upon multiple and sometimes ambiguous ways of judging the reliability of testimony about matters of fact.

One method of establishing reliable testimony has elicited a great deal of historical work in recent years: the creation of strong social bonds among communities of investigators. Robert G. Frank, Jr, for instance, emphasized the techniques of investigation that William Harvey acquired and utilized, which he communicated personally to others in England with whom he worked closely. The people who imbibed his "tacit knowledge" at Harvey's feet became a circle of Harveians, which slowly broke up after his death.[2] Other historians have emphasized a somewhat different kind of close community: societies of gentlemen who witnessed demonstrations or observations together and could collectively vouchsafe their veracity. Published communication about what had taken place within these closed circles often stressed the reportage of "virtual witnessing" to create the impression in readers of having been there, too.[3] Still others have, in different ways, stressed social status as a guarantor of credibility: whether the gentlemanly status of the main body of Fellows of the Royal Society, the aristocratic status of an investigator's patrons, the social legitimacy of a religious order or university faculty, or the etiquette and honour of courtly life, social prestige is said to have established the credibility of matters of fact asserted by members of a group.[4] Such views suggest that an élite established credibility within their powerful but closed circles, and then communicated what

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they agreed upon to others as true matters of fact.

Whether intended or not, many of these approaches have implicitly left the impression that the new philosophy was created within closed circles: that like Francis Bacon's "Lamps", "Inoculators", and "Interpreters of Nature", a small cadre of insiders determined what counted as truths of nature. But Bacon's *New Atlantis*, like Hobbes's *Leviathan*, was written for a monarch with pretensions to absolutism and the desire to control ideas and events. As it happened, however, hosts of people, of many different social and geographical locations, were involved in the establishment of matters of fact. For example, despite a reputed disdain for businessmen among the English gentlemen, the straightforward accounts of merchants were on occasion counted among the models of creditworthiness, even for the aristocratic Robert Boyle: "you will be invited to look on this account, though not yet as compleat, yet as very sincere, and on that score Credible", he wrote, not because it was written by a philosopher "to broach a *Paradox*, or serve an *Hypothesis*", but because it was set down "by a Merchant or Factor for his Superiors, to give them an account of a matter of fact".[5] By emphasizing the relationship between sincerity and credibility, Boyle may have been playing on the ancient notion that simple people were not prejudiced by theory or clever enough to lie credibly.

Michel de Montaigne elaborated this point in his famous essay on cannibals. There he reported as true what his servant told him after having spent ten or twelve years in Brazil, explaining: "This man I had was a simple, crude fellow — a character fit to bear true witness; for clever people observe more things and more curiously, but they interpret them; and to lend weight and conviction to their interpretation, they cannot help altering history a little.... We need a man very honest, or so simple that he has not the stuff to build up false inventions and give them plausibility; and wedded to no theory. Such was my man...."[6] The mark of truth might come from first-hand reports more generally (as in the case of Gonzalo Fernández de Oviedo's or Bartolomé de Las Casas's accounts of the New World[7]), or from someone who had a maker's knowledge of a thing.[8] In this spirit of valuing direct and first-hand testimony of the unsophisticated, Boyle might declare that many useful medicines might be found "from the writings of so ingenious a people as the *Chineses*" as well as by following up on the practices of "Midwives, Barbers, Old Women, Empericks, ... and partly by the *Indians* and other barbarous Nations".[9]

It is therefore important to broaden the discussion of credibility to include many factors well beyond those of closed circles or high social status. Formal and informal institutional arrangements, and social relationships, were important in the development of the new philosophy,[10] but as the examples above suggest, élite virtuosi such as Boyle might well accept reports from people he had never met as true, even from those half-way around the world whose writings he could not read, or even from "old women" down the street with whom

he might never exchange a word socially. Boyle's acceptance might indeed constitute acts of appropriation, but he was nevertheless aware of taking knowledge from those outside his immediate social circle.

Attention to the ways in which matters of fact were communicated becomes of especially pressing importance if one wishes to approach the development of the new philosophy as an international movement. Local and personal witnessing of experiments might indeed help to create bonds of trust among members of the Royal Society in London, but different milieux created different patterns of local investigation and reporting elsewhere in Europe.[11] Moreover, as the case of Antoni van Leeuwenhoek reminds us, foreigners who had never visited the Royal Society of London — who even spoke no Latin, French, or English, and who had no gentlemanly status — might be made members of the Society. They too might have the results of their investigations read in meetings and reported in the *Philosophical transactions*, and they might be considered to be leading practitioners of the new philosophy. Despite differences among the social sites for the production of knowledge, and despite differences among the social ranks of the virtuosi and *savants*, credible information and ideas spread widely throughout Europe. Without the ability to place trust in reports of matters of fact that had not been personally experienced by people like oneself, the new philosophy would have remained fragmented and isolated in local social and geographical spaces.

The concept of networks suggests an alternative to thinking in terms of circles and classes. Some of Bruno Latour's work, for instance, has used network theory to open up the closed circles of a local laboratory to include a variety of people and products (even non-human actants) in the making of knowledge.[12] A further stimulus comes from modern social network theorists.[13] Indeed, one of the most useful observations for the analysis below was made by Mark Granovetter over twenty years ago. Granovetter noted that most investigations into social networks have taken strong ties for granted, studying the ways in which close friends and kin of one neighbourhood or another interact and exchange words. He noticed, however, what he termed "the strength of weak ties".[14] When people sought employment, for instance, they often received a great deal of crucial information from acquaintances rather than from those within their inner circle.

One of Granovetter's more intriguing findings on social networks appeared in the observation that information or rumour might circulate quickly among the members of a group, but seldom entered or exited a group by means of strong ties. The explanation for this observation attributes the pattern to the fact that strong social ties often close people in tight circles of interaction. Granovetter noted, therefore, that "whatever is to be diffused can reach a larger number of people, and traverse greater social distance ... when passed through weak ties rather than strong". Drawing on a study of "hysterical contagion" in a southern

textile plant, Granovetter further generalized that the most influential innovators tended to be "individuals with many weak ties", since they were "best placed to diffuse" their ideas. Put another way, "Weak ties are more likely to link members of *different* small groups than are strong ones, which tend to be concentrated within particular groups". Paradoxically, then, "weak ties, often denounced as generative of alienation ... are here seen as indispensable to individuals' opportunities and to their integration into communities; strong ties, breeding social cohesion, lead to overall fragmentation".[15]

Several urban language studies have reiterated the importance of Granovetter's observation on the significance of weak ties. Sociolinguistic findings in both Philadelphia and Northern Ireland, for instance, "emphasize the need for acknowledging the importance of loose knit network ties in facilitating linguistic innovations". An inverse correlation appears to exist between stable communities bound by strong ties and linguistic change: linguistic change occurs — and has occurred — most rapidly in communities with multiple weak ties.[16] Linguistic "innovators" tend to be marginal, whereas the "early adopters" are more socially central.[17] Social network theory therefore not only (*à la* Latour) points to the importance of links between people in multiple social and geographical locations, but suggests that new information and ideas, like new words, tend to come from people with many weak social bonds.

In the material that follows, we seek to explore the workings and possible significance of weak ties in the international exchange of scientific information during the late seventeenth century. Our primary aim is to suggest that weak ties furnished the basis for a communication strategy that addressed the problems created by new "matters of fact" reported by observers of untested credibility. In a sense, communications based on weak ties met one of the problems of communicating with "strangers" by incorporating individuals into personal networks of weak ties maintained by correspondence.[18] Effectively, the virtuosi of the late seventeenth century dealt with the difficulties of knowledge reported by strangers by establishing contacts that created at least a minimal level of personal relationship.

Tracing out the workings of communications based on weak ties will lead us to consideration of their significance. Certainly, as the circles, societies, and academies of the late seventeenth century took shape and began to work toward the dissemination of scientific knowledge, we should expect to see some evidence of change in communications networks based on weak ties. In fact, such appears to be the case, as the correspondence of Henry Oldenburg and others reveal in conjunction with the passing of scientific information in the period before and after the founding of the Paris Academy of Sciences. We will examine the importance of weak ties, how they were established, and how they worked not only to convey new matters of fact but to establish their credibility. Our examples focus mainly on how English virtuosi communicated with people across

the Channel, but we also venture some larger generalizations in the conclusion about the significance of these patterns for understanding early modern scientific organization. It is arguable that without the proliferation of weak ties, the international movement called the scientific revolution could never have happened.

TRAVELLING, PERSONAL CONTACT, AND CREDIBILITY

That credibility as well as information flowed from one site to another grew from interpersonal trafficking in the "philosophical commerce" of the day. Just as with the flow of coin, the flow of information needed to be examined carefully before being accepted, for it might well be clipped, debased, forged, or otherwise worth less than it appeared. Moreover, as with merchants, so with philosophers: travel not only brought them in touch with new knowledge and things but established the personal credit upon which international exchanges could take place. During visits, people of learning not only traded information but sized each other up and decided whether to trust one another as accurate judges of natural events. From these personal contacts grew in turn the networks of correspondence that sustained the philosophical commerce over longer stretches of time and place. Travel, more than any other activity, established the weak ties by which knowledge could be exchanged.

Many examples can be brought out to illustrate the ways in which information and credibility flowed through weak ties established by travel. Let us begin with one that linked Java, the Netherlands, and England. It concerns the "discovery" of moxibustion by Hermann Busschoff, a Calvinist minister newly resident in Batavia (now Jakarta), then the capital of the Dutch East Indies. Busschoff had suffered for many years from the gout. He finally allowed his wife to persuade him to seek help from a local "Indian doctress" who reputedly had a method to treat his condition. On his toe, this woman burned moxa, which he described as "a very soft and woolly substance, made by a very skilful preparation out of a certain dried Herb", formed into "a little pellet ... which is scarce of the bigness of a small white pea, at one end somewhat sharp, and at the other end flat", placed with the flat end on the skin and then set alight. It quickly burned, leaving only a bit of oil on the skin. He quickly found relief from the pains of his gout by this method.[19]

Busschoff sent a manuscript explaining this therapy to his son back in Utrecht, along with samples of the moxa. When his son published the treatise in Dutch in 1674 to develop a market for the moxa, Constantijn Huygens, Secretary to the Prince of Orange, was among those who noticed. He wrote to Henry Oldenburg at the Royal Society, sending along a copy of the book.[20] The Royal Society had already heard a vague report about the use of moxibustion in Japan communicated via the Dutch,[21] so when the virtuosi heard Huygens's report about Busschoff's method, they asked that a translation of Busschoff's treatise

be published (which appeared in 1676 along with a treatise by one of the noted Amsterdam surgeons).[22] Huygens also struck up a correspondence with Busschoff himself,[23] while recommending the use of moxibustion for the gout to the English ambassador in The Hague, Sir William Temple, who also experienced relief from it.[24] The Dutch microscopist Antoni van Leeuwenhoek quickly turned his attention to investigating moxa with his little lenses, sending a report to the Royal Society.[25] The interest among the virtuosi in moxibustion also soon led Thomas Sydenham to mention it favourably in his book on the gout,[26] while physicians in Germany began to use it with apparent success, too.[27]

The story above is generally about how a medical practice used in Asia came to be adopted for a time in England. It began with a minister's willingness to let his wife talk him into trying something new; in a sense, we begin with a strong tie. On the other hand, the name of the local practitioner who treated him was never mentioned, certainly suggesting what might be called a weak tie between the two; the relationship between wife and practitioner is not known.[28] The minister transmitted the account of his treatment to his son in the Netherlands, as well as in conversation to friends in Batavia: more close ties. But there it might have ended, had not a web of weak ties, all founded on personal meetings, brought it to the attention of other groups.

Busschoff's treatise was printed and fell into the hands of strangers; some of them in the Netherlands tried it out, tried to find out more about the practice and the reporters of the practice by drawing on their connections to visit the son and strike up a correspondence with the minister. They informed their acquaintances elsewhere and locally, including their English acquaintances, about a new matter of fact; the English in turn used their own ties to gather more information about the practice and about those who reported its success; and after hearing from many credible people about its beneficial results, they accepted the new remedy as a good one. Weak personal ties established between people in England and the Netherlands, between people in the Netherlands and in Batavia, and between the Dutch élite and ordinary local people in Batavia, lay behind the public acceptance of a new medical treatment; these weak ties were in turn founded on personal encounters that developed from geographical mobility.

The geographical mobility of early modern people was great. From the Middle Ages through the early modern period, students and scholars, like merchants, journeymen, and job-seekers, had often taken to the road.[29] The *peregrinatio academica* and related kinds of travels were crucial both to individual personal development and to the spreading of knowledge.[30] A few prominent examples will remind us of the personal toing-and-froing between England and the Continent to gather knowledge. Large numbers of early modern English and Scottish students travelled abroad for their educations, seeking out Italian, French, Flemish, and Dutch universities. Thomas Linacre and other Henrician humanists brought back the latest scholarly techniques from Italy. A few decades later,

the young John Dee travelled to Louvain to study mathematics with the noted Gemma Frisius. He brought back not only news of novel methods for applying mathematics to nature, but a brass astronomer's staff and brass astrolabe invented by Gemma, and two globes — one of the heavens and one of the earth — made by Gemma's pupil, Gerard Mercator.[31]

Others brought back not only new learning and instruments, but the tacit knowledge learned only from working under the tutelage of an adept. William Harvey learned several important techniques in Italy — such as vivisection and ligation — and brought them back to London. He then passed them on to his followers.[32] Knowledge about how to construct an air pump travelled by newsletter from England outward, but its replication and calibration had to be carried out by personal inspection.[33] Travel also stimulated emulation: the London College of Physicians was in part inspired by Italian examples, as the Royal Society may later have been, while a delegation of French and Dutch *savants* in turn visited the Royal Society in 1663, helping to provoke the foundation of the Paris Academy of Sciences.[34] Furthermore, the wars of the seventeenth century caused large migrations between Britain and the Continent, transforming the *peregrinatio academica* into something closer to a general movement: Jan Comenius, Samuel Hartlib, and Henry Oldenburg came to England during the Thirty Years' War, and Thomas Hobbes, the Cavendishes, and many others, including Charles Stuart himself, fled to France and the Low Countries to avoid war and revolution in England. The former group brought the latest Central European enthusiasm for grand learned projects with them, while the latter learned about corpuscularianism in France and the Netherlands.[35] This massive exchange of people caused many to take up new practices and ideas. For instance, one of the Scottish royalist exiles, Sir Robert Moray, spent his time in Maastricht immersed in alchemy; after his return in 1660, he worked with the king in the laboratory that Charles constructed in the palace at Whitehall.[36] The mass royalist exile of the 1640s and 1650s firmly established the English version of the Grand Tour, on which young gentlemen and aristocrats of all sorts (and a few ladies) travelled through France to Italy and back, taking note of worthy matters and sometimes even meeting scholars.[37]

Travel helped to shape three things: learning, experience, and judgement. The movement of philological techniques of scholarship among European regions during the Renaissance depended upon scholars from northern lands travelling to Italy to learn from the great humanists, so that by the later sixteenth century, northerners like Petrus Ramus and Justus Lipsius made travelling for learning a foundation-stone of their educational views.[38] In that spirit, among the committees established by the Royal Society of London in the 1660s was the Correspondence Committee, which not only began to collect information from foreigners; it also began to run through the published reports of voyages in order to collect new matters of fact, and it drew up various questions and

instructions for people travelling abroad so that they would bring back accurate and useful information from their experiences.[39] Some even argued that knowledge could be gained through the transformative experience of travelling itself. The Danish Paracelsian scholar Peter Severinus urged his readers to "sell your lands, burn up your books, buy yourself stout shoes, travel to the mountains, search the valleys, the deserts, the shores of the sea, and the deeper depressions of the earth".[40] In a similar vein, Francis Bacon declared that "Travel in the younger sort is part of education; in the elder a part of experience".[41] Meetings with people outside one's otherwise closed social circles during travel might even cause fundamental shifts in judgement. Marin Mersenne, for instance, became convinced during a trip to the Low Countries that people of good character might be excellent scholars even if they held to a different faith.[42] Tutors who took the sons of lords and gentlemen on study tours were therefore instructed to make sure that they did not limit themselves to educating their pupils in reading and writing alone; they were to develop their pupil's judgement. "[I]t was important to 'study men as well as books', to use the terms of Edmund Verney's father, or otherwise his son's 'learning [might] make him rather ridiculous than esteemed'. Lord Burghley too informed his son's tutor [before a trip abroad]: 'I mean not to have him scholarly learned but civilly trained.'"[43]

During the course of travel, many new acquaintances were made. Introductions to eminent people would be sought, but more ordinary people were encountered as well. For many, the personal meetings that resulted from travel established multiple weak ties that provided the foundation for later correspondence (to which we turn below).

They also helped people to evaluate matters of credibility. One of the most important ways to judge whether a report was true or not, of course, was to judge the veracity of the reporter. While people naturally included rank and its associated behaviours in the calculus of judging character — seventeenth-century Europeans did not even pretend to equality — they also noted other outward signs (such as bearing, dress, and cleanliness), inward attributes (such as demonstrated reliability, moral probity, experience, or even simplicity), and religious belief.[44] As Sir Matthew Hale put it, English juries had to judge the credibility of witnesses, noting not only their "Quality" but their "Carriage, Age, Condition, Education and Place of Commorance" [i.e., residence] as well,[45] and these and other considerations were commonplace for judging people. On the other side of the coin, outward signs of poor character pointed to likely fraud or cheating.

Character and characteristics were linked. Medical quacks, for example, could best be recognized not from bad medicines but from bad behaviour. Doctor Eleazar Duncan advised a gentleman to beware practitioners who exhibited "loquaciousness", "haste", "forwardness", and "boastfulness".[46] In the literature of the period, the connections between inner attributes and outward marks were

constantly used to create drama: Shakespeare's Richard III is perhaps only the most nasty physical example of a crafty and mendacious nobleman, while various simpletons who acted as they ought might (like King Lear's jester) speak truth to power. Consequently, in order to know someone's character, judgements needed to be made that ordinarily took shape only on the basis of personal interaction or on the basis of knowledgeable character witnesses. The meetings that took place during travels therefore established acquaintances and friendships that could be mobilized in the interests of establishing the credibility of persons, and by implication the credibility of what they reported.

Consequently, when reports from strangers arrived among groups of virtuosi, they often found it desirable to check on the character of the reporter, which they did by mobilizing their weak ties. For instance, at the meeting of the Royal Society of London on 18 January 1682, one the Fellows, Theodore Haak, brought in and read a Latin letter from Willem Ten Rhijne, a Dutch physician practising in the East Indies at Batavia, written to the recently deceased Henry Oldenburg.[47] In it, Ten Rhijne asked to be informed of events in England, and gave a brief account of some observations he had made on the use of moxibustion[48] and acupuncture among the Japanese, on which he said he could elaborate. He wanted to know whether the Royal Society would be interested in publishing the manuscript he had written on the subjects.[49] Given the previous interest of the Dutch and English in moxibustion, and the vague reports of acupuncture that had also circulated, the virtuosi pricked up their ears.

Despite their enthusiastic interest, however, they first set out to ascertain whether the letter had come from a person who could be trusted to tell the truth. In anticipation of this, Ten Rhijne had not only signed his letter with his credentials (M.D. and member of the governing Council of Justice for Batavia); he established a person-to-person connection. Ten Rhijne wrote the virtuosi that they could find out more about him by talking to one of his friends, a Dutch physician and surgeon working in London, Joannes Groenevelt. Before they went any further with regard to Ten Rhijne's letter, therefore, the virtuosi instructed one of the Society's secretaries, Francis Aston, to talk to this intermediary.[50] At the meeting of the Royal Society of the following week, Aston and another Fellow, John Houghton, reported having met with Groenevelt, who both confirmed that Ten Rhijne was a physician holding posts of great responsibility in the Dutch East Indies and added that he had also been a star pupil of the great Leiden chemist and professor of medicine, François de le Boë Sylvius, who was already known to members of the Royal Society. Having thus had assurances of Ten Rhijne's good character and ability, the members of the Royal Society took him up on his offer, began a direct correspondence with him, and printed his book on moxibustion and acupuncture in London.[51]

Thus, behind the networks of correspondence that covered much of the globe in the early modern period lay personal contacts, usually weak ties. Given the

real interest of the virtuosi in moxibustion, the Royal Society might have authorized its secretary to reply to Ten Rhijne without any references, but such a practice would have risked placing their faith in someone of an unknown character. While the new philosophy was meant to be demonstrative and public, not all eyes counted equally. The question of whose reports to trust could be best resolved by judging the person. But while social rank and educational credentials counted heavily, trust was best built on personal visits — even at second or third hand, as when character references came from an intermediary who had already been judged worthy. At the bottom of the new philosophy, then, one finds visits, even when letters arrived from half-way around the world.

Written correspondence therefore ordinarily presumed personal connections, even if at one or two removes. For instance, one of the Royal Society's most famous foreign correspondents was Antoni van Leeuwenhoek. Leeuwenhoek had been introduced to the Royal Society by the Dutch physician Reinier de Graaf (already well known to Oldenburg through other intermediaries) in a letter to Oldenburg of 28 April 1673. A fellow citizen of Delft, De Graaf had pressured Leeuwenhoek into putting his early microscopic investigations on paper. Therefore, enclosed with De Graaf's letter of reference was Leeuwenhoek's first written report of an observation with his single lens microscope.[52]

The virtuosi discussed Leeuwenhoek's letter at their meeting on 7 May, and Oldenburg printed it in his *Philosophical transactions* of 19 May. But true to form, fascinating observations from an unknown person, even someone introduced by a person of De Graaf's stature, would not suffice to assure the virtuosi of the accuracy of his representations. Following De Graaf's letter introducing Leeuwenhoek, then, some influential members of the Royal Society tried to get further information about this unknown amateur observer. Constantijn Huygens, the Secretary to the Prince of Orange, consequently paid a visit to Leeuwenhoek on behalf of his English colleagues — even in the midst of the Third Anglo-Dutch war — and wrote a report about him back to Robert Hooke on 8 August 1673. Huygens had not only a weighty personal reputation because of his position, but was well known personally to many of the English virtuosi, having received many English visitors and having visited England several times himself, most recently as a member of the delegation that visited the Royal Society for a considerable time in June 1663.[53] Huygens wrote that he had found Leeuwenhoek to be "a modest man, unlearned both in sciences and languages, but of his own nature exceedingly curious and industrious". After describing Leeuwenhoek's microscopical method, Huygens returned to a description of the man, saying that Hooke would "not be unpleased with confirmations of so diligent a searcher as this man is, though always modestly submitting his experiences and conceits about them to the censure and correction of the learned". Only after this second personal confirmation of Leeuwenhoek's good character

did Oldenburg invite him to communicate his observations regularly. Leeuwenhoek afterwards sent his observations in letters in Dutch to Oldenburg, which various members of the Royal Society translated into English before publication in the *Philosophical transactions*, while Fellows of the Royal Society visited Leeuwenhoek when travelling in the Netherlands: Thomas Molyneux, Hans Sloane, and Francis Vernon among others.[54]

In addition to establishing personal connections with the London virtuosi (which were clearly of a weak rather than strong variety), Leeuwenhoek and others found it fruitful to mention reputable witnesses of their most important experiences in communications to the Royal Society. Leeuwenhoek's letter of 7 September 1688, for example, was very excited: he announced that he had observed the circulation of the blood in the "external gills of young frog's larvae, in the tail of older larvae and in the ends of the toes of young and adult frogs". The capillaries had been discovered by Marcello Malpighi in 1661, and had been seen by Leeuwenhoek in late 1683 through the examination of dead specimens; in his observation of 1688, however, Leeuwenhoek actually saw the blood streaming through the capillaries. He considered this one of his most important discoveries. Consequently, he carefully mentioned in his letter five witnesses, including his neighbour, the Dutch natural philosopher Cornelis 's Gravesande, the local magistrate Cornelis Vallensis, and Anthonie Heinsius, later the Grand Pensionary of Holland. None of these people were co-investigators or expert microscopists, but all had reputations beyond reproach and therefore stood as character witnesses who could vouch for the credibility of Leeuwenhoek's report.[55]

Once one becomes aware of how important personal judgements of one another were to the virtuosi's pursuit of the new philosophy, the letters and printed texts of the period begin to take on new colourations. An examination of the correspondence of Henry Oldenburg reveals that virtually all of the letters sent to him began through a personal contact. For instance, the printed correspondence for the year from 1 January to 31 December 1665, consists of 114 letters.[56] Somewhat over half (sixty-seven) were sent to Oldenburg rather than being from him. Of these sixty-seven incoming letters, about a quarter (seventeen) came from eight foreign correspondents, the rest being from Englishmen.[57] Of these eight foreign letter-writers, three were French: Adrien Auzout wrote Oldenburg six letters,[58] and Henri Justel and Pierre Petit wrote one letter each.[59] All three of the Frenchmen had begun their connections with Oldenburg in previous years, during Oldenburg's period of residence in France. Two correspondents were Dutch: Christiaan Huygens and Benedictus Spinoza.[60] Both the Dutch correspondents had long been known to Oldenburg, the first through Sir Robert Moray, the second from a personal encounter in Holland. Oldenburg also heard from three Germans: Johann Hevelius, Johann Daniel Major, and Philipp Jacob Sachs von Lewenheimb.[61] Oldenburg's correspondence with the astronomer

Hevelius had been initiated by him in 1663 at the request of members of the Royal Society who knew Hevelius through personal networks.

Of the eight foreigners writing letters to Oldenburg, then, only two first initiated contact in 1665: the two German physicians, Major and Sachs. Both were members of the German Collegium Naturae Curiosorum, a loose society of investigators of nature from the various German provinces:[62] Major came from Breslau, Sachs from Hamburg. Both men, however, wrote to Oldenburg only after another member of the Collegium, Theodorus Jacobi, also of Hamburg, had visited the Royal Society at the behest of a Senator, Mr Hofmann of Hofmanswaldau. Jacobi returned from London with news that the virtuosi had heard of the Collegium from Sachs's book,[63] and he had told Oldenburg about Major and Sachs. Upon returning, then, Jacobi orally passed on Oldenburg's invitation to begin a correspondence, resulting in the two letters. The two new foreign correspondents of 1665, then, began to write Oldenburg at Oldenburg's own request, following a visit to London on the part of a colleague who could vouch for both sides of the connection.

With this general picture of Oldenburg's correspondence as broadening bit by bit through personal contacts, one can see the correspondence of "the Royal Society" growing not so much from impersonal reputation as from travel.[64] As people from the Continent visited England, or as English virtuosi travelled on the Continent, new connections were established in a widening network. The Royal Society's foreign correspondence had, then, begun with the (often weak) personal relationships brought by Oldenburg and other foreigners to England, or developed by English royalist exiles living in France, the Netherlands, and Germany during the interregnum.

Gradually, based upon acquaintances vouching for the credibility of others, letters began to be exchanged, widening the network of weak ties. Italy, for instance, was opened to the Royal Society slightly through the residence of John Finch and Thomas Baines in Florence from 1659 to 1672, more by the visit of Magalotti to London in 1668, and the custom of the Grand Tour.[65] In this sense, the Oldenburg correspondence began like the personal correspondence of the great communicator Marin Mersenne or other *savants*, only gradually becoming something more like the official correspondence of a society.[66] As his associates in the Society heard of other virtuosi through their own networks, Oldenburg became introduced to new people: but only after they were vouched for by people he trusted. Similarly, there was never any official correspondence of the Accademia del Cimento, since communications were carried on by its members privately with their own acquaintances, or through the diplomatic agents of the Grand Duke.[67]

The overwhelming preference for establishing personal relations in order to establish a correspondence is apparent in the frequent letters of reference that passed among acquaintanceships that constituted networks of weak ties. For

example, during Locke's exile in the Netherlands, one of his Dutch friends asked him to pass on a letter to the noted medical professor of Montpellier, Charles Barbeirac, with whom Locke was corresponding after having previously visited that city. "[T]o which please be kind enough to add a letter of introduction recommending me as a worthy person to such a celebrated medical practitioner, for I should much like to correspond by letter with such a great man."[68] So it went: friends introduced others into their circles, vouching for them and extending the networks.[69]

One final example is an exception to prove the rule: the case of how Oldenburg began a correspondence with Marcello Malpighi. By 1667, Malpighi had earned a local reputation as one of the most important naturalists of Italy. Several foreigners tried to see him when visiting Italy, including Henry Sampson, an English nonconformist minister and brother-in-law of the noted English naturalist Nehemiah Grew. With a letter of introduction from Girolamo Barbato, Sampson tried to see Malpighi on several occasions, being disappointed each time by finding Malpighi out visiting patients. Called home before he could meet Malpighi personally, Sampson left him a flattering written message asking him to pass on his work to him in England, promising in turn to inform Malpighi of the work being done by the English. Shortly after Sampson's return to England, Oldenburg wrote to Malpighi inviting him to take up a correspondence with the Royal Society — which Malpighi did.[70] Neither Sampson nor Oldenburg had met Malpighi directly, yet Sampson had gotten close: his scholarly contacts in Italy had told him about Malpighi and obtained for him a letter of introduction. In effect, Oldenburg had good evidence of Malpighi's fine character at three removes — but Malpighi's reputation was so good that even at three removes Oldenburg could invite a correspondence with him. After the correspondence between Malpighi and Oldenburg had begun, when Grew wished to address Malpighi he sent messages through Oldenburg rather than directly, as a Member of Parliament might address the Speaker rather than another Member. Such was the nature of weak ties: they reached many more people than the inner circle of, say, the Royal Society, linking people of different groups as far away as the East Indies, and establishing paths for assessing the personal credibility of reporters as well as conveying information.

SPREADING INFORMATION

Personal meetings therefore often established the weak ties upon which correspondence could be established. Re-reading the intellectual correspondence of the seventeenth century in the light of how such weak ties helped to spread information and credibility points to the importance of certain kinds of information contained in them that would otherwise be missed. By looking at how matters of fact crossed Henry Oldenburg's desk in a particular episode, we can begin to see how networks of weak ties could come to focus on specific

individuals who passed information between different networks of correspondence. These individuals, such as Oldenburg himself, who furnished the points of overlap between correspondence networks seem to have served as nodal points, allowing very broad reach in gathering and disseminating information. The particular case that illustrates this pattern of nodal points in overlapping networks of correspondence extremely well involves an optical instrument known as the "Burning-Mirror of Lyons" that captured the attention of Henry Oldenburg and many others during the summer and early autumn of 1665. Oldenburg was determined to produce an authoritative account of this burning mirror for his *Philosophical transactions*, and his efforts have left a record that clearly shows the workings of both weak ties and the nodal points that could emerge in overlapping networks that spanned very broad geographical areas.

Savants in Paris had already discussed reports of a burning mirror in Lyons and checked the accuracy of the accounts through their own personal networks by the time Oldenburg heard of it through his Parisian correspondents. The first detailed account of the mirror came to Oldenburg in the form of a copy of part of an unsigned letter dated 28 July 1665 [N.S.] that originated in Lyons, in which the Lyonnais correspondent was clearly responding to Parisian queries for more information about the mirror: he named the instrument maker as François Villette of Lyons and offered the Parisians a detailed confirmation of an earlier report they had received.[71] Oldenburg's correspondent in Paris (almost certainly Henri Justel) had simply forwarded the report to Oldenburg. Before enclosing the account of the mirror with his own letter to Oldenburg on Wednesday 5 August, however, Justel had almost certainly read the letter from Lyons at the Thévenot Academy on Tuesday 4 August 1665 [N.S.].[72] Thus, Oldenburg's first report on the mirror was from an eyewitness to its performance in Lyons via the filter of Parisian *savants*. Still, despite the author's efforts to reassure his reader that "every thing you have heard ... is true", this account stretched Oldenburg's credulity at the same time as it piqued his interest. He needed to find out more from the eyewitnesses to such strange happenings.

The letter from Lyons described a spherical mirror that was extraordinarily large and powerful. The combination of an aperture of "two feet six inches and about two lines" with the spherical properties defined in a radius of "four feet eight inches" and a focal length of "two feet four inches" meant that, if the mirror was properly cast and polished, it would give the greatest magnification of any mirror then known. This was a powerful optical instrument. More importantly for Oldenburg, however, the account he had received also detailed impressive performances from this burning mirror. According to the eyewitness in Lyons, it had easily melted copper and silver coins, pieces of brass, "bits of a cast iron kettle", small bits of steel, and small nails. It had failed to melt a large piece of wrought iron, but did reduce glass, stone, and animal bones to calx. It also lit candles "very quickly", and the "thick sticks of wood it set afire in a

moment made a pretty sight".[73] All in all, a very impressive performance — maybe too impressive — or so at least Oldenburg thought.

Oldenburg activated his end of a correspondence network by relaying all the information he had received from Paris to Robert Boyle, seeking his commentary on the extraordinary report coming out of France.[74] Boyle owned a much smaller burning mirror, and Oldenburg wanted his patron, employer, and friend to compare its performance with what was reported for the mirror from Lyons. Nor did Oldenburg limit his efforts at confirmation to Boyle. He also wrote to Sir Robert Moray soliciting an opinion,[75] and more importantly, he also contacted his two primary French correspondents, Adrien Auzout and Henri Justel. From them, Oldenburg wanted further technical details, comparisons with the best Italian mirrors — which common assent rated the best in the world — and further confirmations of this mirror's performance from known and credible people. Above all, he needed to have confidence in the reports, which meant having confidence in the reporters.

In his efforts to solicit opinions and gather information on the burning mirror of Lyons, over the next ten weeks Oldenburg cast his net widely through networks of correspondence based on weak ties. Altogether, from Justel's first (now missing) letter in early August through the end of October when he took the sixth number of the *Philosophical transactions* to press,[76] Oldenburg wrote at least ten letters (and probably several more) soliciting opinions or detailing his efforts to prepare his account for the *Transactions*. During this same three-month period, he definitely received at least eleven letters (once again, probably several more) giving him advice on the mirror. Of the total of twenty-one known letters, at least five involved cross-channel communications based on weak ties. Oldenburg wrote at least two letters to his Parisian correspondents (both lost), and received at least three responses in return. Nor did the twenty-one letters that definitely crossed Oldenburg's desk constitute anything like the total correspondence generated by this mirror, even in the limited period before Oldenburg published his account. Before Justel had forwarded the confirming report he received from Lyons on to Oldenburg, for instance, the Parisians had carried out at least one round of exchanges with correspondents in Lyons. Then, when Auzout and Justel received their requests from Oldenburg for further information, they both wrote off to their correspondents. In Auzout's case the nature of the information he gathered for Oldenburg suggests that he wrote to Italy and received a response before he passed his confirming details back to Oldenburg.

Other networks were also at work in August and September 1665. André Graindorge, for instance, reported the mirror to Pierre-Daniel Huet in words that very closely parallel the first report Oldenburg received. (Graindorge's letters to Huet, in fact, are what tie Auzout, Justel, and the Parisian correspondence linkage with Lyons so closely to Melchisedec Thévenot's Academy.) Nor is it likely Graindorge was alone in spreading news of the mirror back into the

French provinces. One of his colleagues from Caen, Nicholas Croixmare de Lasson, was also attending the Thévenot at this time, and the discussions there inspired him to brag he could "cast a mirror larger than the one in Lyons", a feat he never actually performed. Graindorge's letters also place at least two other provincial intelligencers at the Thévenot during this same period — one from Rouen, one from Dijon.[77] Unless these provincial gentlemen were absolutely immune to the kind of enthusiasm for the mirror that infected Oldenburg, Boyle, Moray, Justel, Auzout, Graindorge, de Lasson, and Huet, it is virtually inconceivable they would have neglected to report this wonderful mirror to their correspondents at home.

Tracking definite exchanges and likely avenues of exchange for news of the burning mirror of Lyons during the late summer and early autumn of 1665 yields the knowledge that — well before Oldenburg published his account in the *Philosophical transactions* — reports and commentary on the burning mirror of Lyons had crisscrossed back and forth across Europe into various correspondence networks that converged in a nodal point, the Thévenot Academy in Paris. These correspondence networks connected Oldenburg (and thus England) to Paris, Paris to Italy, and Paris to at least four French provincial cities. Weak ties founded on personal meetings allowed credibility to be shaped from written reports. Oldenburg was planning to publish his account of the mirror for a scientific community in which news of the mirror had already spread widely. What he was interested in publishing, then, was not simply the first news of the mirror; rather, what he wanted to publish was an authoritative report on a topic many people in the scientific community were already talking about.

Several characterizations of Oldenburg's cross-channel conversations during this period of mid- to late 1665 follow from his epistolary efforts with the burning mirror of Lyons. First, his weak ties with Parisian correspondents attending the Thévenot Academy hooked him into a scientific communications network spanning the southern half of scientific Europe. In fact, the Thévenot Academy clearly furnished the Parisian nodal point for this correspondence network,[78] just as Oldenburg himself furnished a nodal point in London. Second, the communications that crossed his desk, although extensive, almost certainly represented only a part of the total traffic in correspondence that discussions of the mirror generated in the period before he published his account in the *Philosophical transactions*. Third, communications throughout the network involved in supplying information to Oldenburg appears fairly rapid given the scale of the correspondence passing between networks of weak ties.[79] All in all, this episode indicates that in late 1665 Oldenburg's Parisian correspondents had connected him with what we can only call a communications network based on weak ties that checked the credibility of the reporters.

This pattern suggests that further study of weak ties and correspondence networks would be repaid. Henri Justel's communications with Oldenburg, for

instance, take on new colouration. Among twentieth-century historians of science Justel suffers somewhat from a reputation as a gadfly, a superficial commentator on the scientific and literary worlds because of the complex, disparate quality of his letters.[80] They are full of seemingly the most disparate and unconnected concoctions of news, opinion, and rumour any seventeenth-century intelligencer managed to bring together. Yet through the mid- and late 1660s, Henri Justel served as the most faithful of Henry Oldenburg's French correspondents. Indeed, the more than thirty letters Justel is known to have sent Oldenburg between mid-1665 and the end of 1669 rank him as one of Oldenburg's most consistent and regular correspondents over this period. Moreover, given Oldenburg's habit of retailing news from Justel's letters through his own subsequent exchanges with other *savants*, Justel has to be counted among Oldenburg's most important and valued correspondents on the Continent.

Fifteen letters of Justel to Oldenburg have survived intact for the period from early November 1667 through late March 1668. Five of them recommended Pierre-Daniel Huet as a correspondent;[81] interwoven with them are seven other letters in which Justel chronicled the first months of Huet's Académie de Physique de Caen under royal patronage.[82] Moreover, in this group of twelve letters, the two sorts of communication are distinct. Justel either chronicled events and passed on news about the new royal academy in Caen, or devoted his comments to Huet's personal qualities as a potential correspondent: no single letter does both. On the other hand, no single letter across this four-month period was given over entirely either to Huet or the Académie de Physique.

Indeed, no single letter was given over entirely to any one subject. Justel's letters to Oldenburg appear at first reading to offer something like a bulletin board — a free-form, very lightly edited posting of incidental news items. No single letter stands alone in conveying a sense of the communications passing between Justel to Oldenburg. Their communication occurred in a running dialogue, in which the ties between the parties allowed for interpretations of meaning that can be pieced together by an outsider only with difficulty. In such a complex correspondence, repeated references of just a single sentence or so can carry as much meaning as a fully developed discussion on a topic that appears only once. Individual letters therefore start to lose their character as discrete entities and take on the character of parts of a dialogue. Treated in this way, an underlying level of coherence and order surfaces that is far greater than what appears in any single letter.

In Justel's letters to Oldenburg, then, the coherence of communication appears not so much in the individual letters as in the run of the correspondence. The 'texts' of individual letters hold together less clearly than several longitudinal 'texts' that emerge from seemingly isolated fragments culled from a dozen or more separate letters. Each letter, then, carried multiplexed information, and as is the case with all multiplexed communications, finding meaning depends

on decoding a complex signal. Yet Oldenburg seems to have found no difficulty in decoding Justel's messages, and the frequency with which he forwarded extracts from Justel's letters (or the letters themselves) to other correspondents suggests that he had no difficulty in pulling together their longitudinal threads. Careful longitudinal reading of Oldenburg's regular exchanges with long-term individual correspondents such as Robert Boyle, Sir Robert Moray, Christiaan Huygens, Justel, or Adrien Auzout shows that this same "conversational" quality ran through all these exchanges. These correspondents assumed context, and often elevated their epistolary rounds to carry on several colligated conversations in the multiplexed communication each letter allowed. Nor is the Oldenburg correspondence unique in this regard. The same quality clearly appears in the Graindorge–Huet correspondence over a ten-year period; in fact, in the enormous Huet collection housed at the Biblioteca Medicea-Laurenziana in Florence, the complexity to the patterns of colligation in his correspondence dwarfs what appears in the Oldenburg correspondence both in sheer massiveness and in the scope of communications. Oldenburg was dedicated in his pursuit of correspondence, but he was by no means atypical in the mid-seventeenth century. Reading Oldenburg's correspondence with individuals as multilayered conversations transmitted in epistolary bursts raises the likelihood that higher levels of infolding could occur in the patterns of letters passing among several correspondents. Networks founded on multiple weak ties could be very robust.

NEW INSTITUTIONAL FORMS, AND DISRUPTIONS OF PERSONAL EXCHANGES

Yet over the next several years, Oldenburg's communications with the French scientific community nearly failed him. At least, this network failed him if we compare his knowledge of events unfolding at the Académie Royale in Caen to his success in getting information on the burning mirror of Lyons. Moreover, this failure cannot be explained by any slowing in the pace of correspondence. The critical factor is found in the disappearance of a set of personal relationships called the Thévenot Academy. The shifting of institutional bases that occurred with the chartering of the Paris Academy of Sciences brought powerful new forces into the social structure and social practice of science during the 1660s.[83] The Thévenot disintegrated in the spring of 1666 due to the announcement of plans for a French royal academy.[84] The new royal academy would be rooted in different sorts of interactions, ones that pulled a large number of important people out of the network of personal exchanges and weak ties that created the flow of information (and checks on it) so critical for the new philosophy. Substituting the new structure of the Paris Academy of Sciences seemed to do more than simply change the physical location of the major Parisian centre for science. The new institution structured close ties among its members and seems to have disrupted the weak ties of the established correspondence networks.

Given its structure, the Paris Academy of Sciences could not replace the

Thévenot as the nodal point for French scientific communications; indeed, the Paris Academy opened under an official policy of secrecy. Non-members were barred from sessions; all deliberations and discussions were private; and the only news officially permitted was to appear in publications on completed research.[85] Officially, the rules at the Paris Academy banned the kind of exchanges that had occurred over the burning mirror of Lyons, even if the instrument originated in Lyons. That is, had the Lyonnais artisan who fabricated the mirror announced his accomplishment to the world through the Paris Academy rather than the Thévenot, the rules of the new organization would have prohibited discussion with non-members.[86]

Oldenburg's correspondence in the period from the first official word about a new royal academy in March 1666 show that the Paris Academy's ban on communication with outsiders actually proved to be fairly effective.

There is little doubt that Oldenburg wanted news about the new academy in Paris from the moment Justel told him Colbert had made a definite commitment to a royal institution.[87] Over the next three years, however, he learned very little. He continued to correspond with both Auzout and Justel, and while the letters from Auzout came less and less frequently until he finally resigned from the Academy and left for Italy (1668),[88] the pace of Justel's letters continued strong. In fact, if the number of Justel letters that have survived means anything, he was writing to Oldenburg more frequently during this period than during any other in their correspondence. During much of this period he wrote at least once a week. But the rate of letter-writing is less important than either the timeliness or the completeness of the information Oldenburg received. More particularly, longitudinal reading of letters from Oldenburg's French correspondents during this period fails to reveal those longitudinal threads of information on French science that had been such a central feature of his earlier letters.

An edge of frustration (on all sides) crept into Oldenburg's exchanges with Auzout and Justel during this period. For example, after receiving a letter a month on average from Auzout through the first half of 1666, Oldenburg received none from June until December. In the letter he finally received in December, Auzout opened with an apology for his long silence, and when he turned to the new French Academy he had little to say: "Although I had the honour to be appointed by the King as mathematician and physicist, I can't give you any more details than are known to everybody, because we have not been kept fully informed and things have not reached the point we were led to hope for."[89] He then claimed that he could not foresee the start of much activity "before the summer is over". In other words, he did not expect the Academy to get underway with its scientific program until late 1667. In this same letter, Auzout also told Oldenburg that he and others had done very little since the summer of 1666: "while we lacked instruments ... the constant hope of getting them [with the opening of the Academy] resulted in individuals failing to undertake what they would normally have done."

This letter offers an intriguing commentary on French science in the first days of the Paris Academy, not only in what Auzout said, but also in what he left unsaid. Auzout had been one of the great advocates for the Paris Academy of Sciences in the period before its creation; indeed, he is more widely known today for that advocacy than for his considerable scientific work. Still, here he was saying nothing had been done, and little was likely to be accomplished in the next year. Finally, even the date (28 December 1666 [N.S.]) at which he wrote this letter carries significance for what he left unsaid. The official opening of the register at Paris Academy of Sciences occurred on 22 December 1666 — just six days before he penned this letter — yet he reported nothing to suggest the Academy had even started holding its official sessions.

Nor was this letter an aberration. Almost certainly, the next time Auzout wrote to Oldenburg was on 29 December 1667: a full year later. Once again, he opened this letter with a long apology for failing to write for so long, and as part of that apology said: "I am hardly ever in a humour to write. I do not excuse myself with the weight of my work, because you may well believe I have none." Then, after filling his letter with thanks for favours Oldenburg had performed, and a discussion of work he and Jean Picard had done on the filar micrometer in 1665 and 1666, he begged off from reporting any current news: "I have imperceptibly become very lengthy.... You will not take it amiss that I send you no news of our work at the moment."[90] Six months later he resigned from the Academy and left Paris for Rome.

Justel's letters mirror Auzout's change in level of enthusiasm for detailing the accomplishments of French science during this period. Pulling out the thread of his "conversation" with Oldenburg on the new academy's work across the first three years of its history yields the following recital:

May 1666: "They are labouring here on the establishment of an Academy to be composed of members chosen from all kinds of professions. No one knows the details because it is only sketched out."[91]

October 1666: "Several members of the Academy have been named ... nevertheless, they do not yet work as they should."[92]

March 1667: "Our philosophers will soon produce something which is not very important. New discoveries are difficult to make."[93]

December 1667: "I cannot tell you what our academicians are to work at, as they keep it secret...."[94]

April 1668: "A letter on optics addressed to our Academy is being printed. The Academy is being strongly urged to get to work."[95]

May 1668: "Our Academy proposes something considerable as I have just been assured. It is essential that it do something, and produce something worthy of its promise, otherwise the King and the public will have reason to complain of it."[96]

June 1668: "[Our Academy] at Paris will do something in time, receiving aid and having everything necessary for making itself talked about."[97]

July 1668: "The Academy will do something in time. At least it ought to do so."[98]

October 1668: "Our Academy does nothing new."[99]

October 1668: "Our Academy does nothing. Observations were made of the last eclipse, which wasn't much."[100]

December 1668: "Our Society still meets, but has still produced nothing, which causes talk...."[101]

Of course, things could not have been as bad at the Paris Academy of Sciences as Justel's conversational thread suggests — even though Justel's reports square fairly well with those of other contemporary observers such as André Graindorge, who during this same period complained of the Paris Academy: "Ils font grand mystère."[102]

Nevertheless, there was a point to this longitudinal conversation. Justel clearly knew very little about activities at the sessions of the Paris Academy of Sciences. His major triumphs in reporting news on Parisian science during this period came in forwarding a copy the Academy's dissection report on a lion that had died in the king's zoo, in obtaining an incomplete set of architectural drawings for the Observatory over a year after Colbert ceremonially laid the first stone, and in reporting the favours and exhortations Colbert showered on the academicians. Justel simply did not have much information about science at the Paris Academy of Sciences despite the fact that he knew several members, and often mentioned them and their extra-academy activities in his letters to Oldenburg.

Justel had not lost his enthusiasm for science, and it is important to note that he furnished Oldenburg with extensive accounts of dissections performed at the academy in Caen throughout this period when he had very little to report on Parisian science.[103] Following Auzout's arrival in Italy, Justel also relayed his news on Italian science to Oldenburg. Overall, the point that comes through most clearly in the Justel letters to Oldenburg during the first several years of the Paris Academy of Science's history is that he had no meaningful scientific news from Paris itself. He knew only what everyone else outside the Academy knew: virtually nothing. When Justel learned what they had been doing, it was through the Academy's official publications on completed research — hardly the kind of news to fuel his correspondence with Oldenburg.

The only conclusion to draw from this comparison between Oldenburg's cross-channel conversations during late 1665 and those across the next three years is that the Paris Academy and the closing of the Thévenot seriously broke the chain of communications based in weak ties that had allowed Oldenburg's communications with Parisian science, despite the fact that he still exchanged news

with the same correspondents. Moreover, the loss of the weak ties that constituted the Parisian node broke Oldenburg's access to the correspondence network that had penetrated the southern half of scientific Europe.

Oldenburg never did regain access to the kind of news from Paris he had depended on in writing his article on the burning mirror of Lyons. Moreover, he had very little information at all on the Paris Academy of Sciences until the spring of 1669 when Francis Vernon took up his post as secretary to the English Ambassador. Vernon was interested in science, and arrived in Paris armed with questions Oldenburg wanted answered. The kind of information Vernon's first letters to Oldenburg conveyed illustrates just how little the English had learned until this point about the Paris Academy of Sciences. Vernon's news for Oldenburg offers a fitting point on which to conclude this consideration of weak ties and their importance in the communication patterns in the 1660s.

On arrival in Paris, Vernon had quickly established a weak tie with Domenico Cassini, also new to Paris, and this is what he was able to learn from Cassini:

> Hee told mee that the Royal Academie are not as ours in Engld — a great assembly of Gentlemen, Butt only a few Persons wch are eminent, & not in number above 13. or 14. To whose conferences none are admitted of what quality soever who are not of their owne body. & these have likewise a pension From the King of 1500 Livres per annum.... They meet twice a weeke Wednesdays, Fridays & one day is deputed For Physicall, the other For Mathematicall exercises. In the Physicall Academie though they doe not strictly bind themselves to one Theame, Yet that at present they are examining the Doctrine of Coagulation. In the Mathematicall Academie, They are stating the Force of Air & water as to bodies they can beare, Their weight & such like enquiries."[104]

The very general quality of such a report suggests Oldenburg had learned very little about the Paris Academy of Sciences in the three years before Vernon's arrival. Certainly, the contrast with his knowledge of details of work in French science as recently as 1665 is striking. It had been the weak ties formed from personal acquaintance that supported the intellectual exchanges so important to the development of the new philosophy; institutionally enforced close ties seem to have cut significantly into these exchanges.

CONCLUSIONS

We have attempted to elaborate and extend the description of the practices of early modern natural philosophy beyond the small groups and closed circles that have received so much recent attention. The emphasis on scientific practice in much of the recent historical literature, which has focused on the local activities of closed circles of virtuosi, is a valuable corrective to idealized discussions about intellectual discovery. Yet one of its weaknesses has been the implicit,

and sometimes explicit, assumption that all practices are local practices, undercutting the sense of a European-wide movement. We wish to show that studies of scientific practice can also be valuable for understanding the international aspects of the so-called scientific revolution. Moving from the local to the global is not necessarily to move from the personal to the abstract.

In arguing for this view, however, we have adopted the concept of weak ties from the literature on socio-linguistics as well as drawn on an increasingly rich historical literature about travel and correspondence in the Republic of Letters. By means of this strategy we also claim that for the early modern period, at least, the methods of scientific practice and discourse used by the virtuosi differed little from the methods used by other *savants*. We suggest that the sociology of early modern science was therefore little different from the sociology of early modern knowledge more generally; and the practices of the new philosophy depended on tightly knit social bonds no more or less than the practices of other kinds of intellectual discourse. Perhaps experiments necessitated a unique sociology of collective investigation; but perhaps here, too, the evidence suggests that matters of fact were noted through individual work, which was then communicated to larger groups.[105] Moreover, our observations suggest that one of the little — but possibly significant — ways in which the forms of communication of the virtuosi may have differed from those of other *savants* lay in the rapidity of the epistolary exchanges. Like merchants, diplomats, and others concerned with the press of worldly events, Oldenburg and others exhibited a concern to work quickly in order to stay on top of unfolding events. The importance of quick assessment and forwarding of information may suggest some ways in which the participants in the new philosophy adopted methods of work as much like those involved in business as in scholarship.

More generally, communication via letters and printed texts continued to rest on a foundation of oral discourse. Knowing something about the views of others sharpened one's awareness of the issues at stake when a scholar committed his views to paper. To understand fully the latest books meant having to know not only the context of a debate from other publications but to know about the persons involved, or at least being acquainted with them through others. Especially when it came to accepting the integrity of matters of fact — which could seldom be deduced from the already accepted, and were often not immediately replicable — trusting the integrity of reporters was also necessary. The virtuosi therefore frequently had to judge others' characters, so that reports from those known to be prone to exaggeration, sloppiness, fiction, or just plain lying would not be accepted. Social status or common witnessing were not, in themselves, adequate determinants of credibility. Reputation could be established on multiple grounds. One important help in establishing credibility (which we have stressed above, although without meaning to reduce the problem to it alone) was to visit the reporter or someone who knew the reporter. Personal meetings

worked well for sizing up the character of another. Visits therefore helped to establish the integrity of information at the same time that they provided contacts from which correspondence could be initiated. Consequently, travel allowed information to flow to metropolitan centres from around the world as well as from provincial informants and other metropolitan groups.

The international exchange of learning therefore depended upon an exchange of personal visits. From the weak ties formed during those visits, complex networks of correspondence, containing complicated dialogues among acquaintances, could convey and check information. Because these practices are in many ways unsurprising, the "scientific community" of early modern Europe operated in a manner little different from other communities of people, whether scholars, diplomats, merchants, or any others who travelled long distances and followed up their meetings with correspondence.

We would even suggest that the success of the new philosophy depended on the proliferation of weak ties, which could be robust exactly because they were inclusive and pluralistic. From this perspective, the Royal Society of London looks more like a formalized association of several English circles, each with their own connections via weak ties to other people and circles throughout Europe and European bases elsewhere. In this sense it was not unlike the German Academia Naturae Curiosorum, which was made up of diverse investigators throughout German lands (and which gained incorporation from Leopold I). The nature of weak ties also suggests why people in the Netherlands might do excellent natural philosophy without having to be formally associated in a scientific society. The new philosophy grew well in soil fertilized by multiple weak ties. The networks founded on weak ties worked well to establish the credibility of things and people.

From this perspective, the establishment of closed circles only sowed doubts and cut the lines of communication that had been so stimulating.[106] When organizational forms grew up that made new philosophers into a closed circle of royal advisers, the personal visits and spoken exchanges lessened, closing down important parts of the network. Superficially, the opening of the French national academy in 1666 was greeted from the other side of the channel with enthusiasm and expression of a desire to exchange scientific news. In the formal documentary record surrounding the opening of the Paris Academy of Sciences, however, there is little to suggest it exercised any immediate impact on scientific communications between France and England. Nevertheless, such a view overlooks the context of the relationships on which communication depended. The patterns of communication in the correspondences Henry Oldenburg maintained with members of the French scientific community show distinct changes in the mid-1660s — changes clearly associated with the opening of the Paris Academy of Science. Indeed, seen through the Oldenburg correspondence, Paris appears as a nodal point in a communications network spanning the

southern half of Europe. With the coming of the Royal Academy, however, the pattern shifted. Oldenburg's obvious frustration with trying to get news from his French correspondents in the later 1660s speaks to a disruption of scientific communications following from the foundation of the Paris Academy of Sciences. The social form of the new science was being reshaped far more radically in the Sun King's France than in Restoration England; but investigating the later changes in the French Academy's working methods and forms of communication might demonstrate that its members, too, continued to place great weight on the methods of communication associated with weak ties.

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1. For the phrase "matters of fact", see Steven Shapin and Simon Schaffer, *Leviathan and the air pump: Hobbes, Boyle, and the experimental life* (Princeton, 1986), esp. pp. 22–24; on contemporary ideas of "fact", see Lorraine Daston, "Baconian facts, academic civility, and the prehistory of objectivity", *Annals of scholarship*, viii (1991), 337–63, and Barbara Shapiro, "The concept 'fact': Legal origins and cultural diffusion", *Albion*, xxvi (1994), 1–26.
2. Robert G. Frank, Jr, *Harvey and the Oxford physiologists: A study of scientific ideas and social interaction* (Berkeley and Los Angeles, 1980). Frank does not employ the concept explicitly, but implicitly. For the concept of tacit knowledge, see Ludwik Fleck, *Genesis and development of a scientific fact*, ed. by Thaddeus J. Trenn and Robert K. Merton, transl. by Fred Bradley and Trenn (1st publ. 1935; Chicago, 1979); Michael Polanyi, *Personal knowledge: Towards a post-critical philosophy* (Chicago, 1958); and *idem*, *The tacit dimension* (Garden City, N.Y., 1966). For more recent uses of the idea, see H. M. Collins, *Changing order: Replication and induction in scientific practice* (London, 1985).
3. See esp. Simon Schaffer, "Making certain", *Social studies of science*, xiv (1984), 137–52; Steven Shapin, "Pump and circumstance: Robert Boyle's literary technology", *Social studies of science*, xiv (1984), 481–520; Peter Dear, "Totius in verba: Rhetoric and authority in the early Royal Society", *Isis*, lxxvi (1985), 145–61; Shapin and Schaffer, *Leviathan* (ref. 1), 60–69; Jan V. Golinski, "Robert Boyle: Scepticism and authority in seventeenth-century chemical discourse", in Andrew E. Benjamin, Geoffrey N. Cantor, and J. R. R. Christie (eds), *The figural and the literal: Problems of language in the history of science and philosophy, 1630–1800* (Manchester, 1987), 58–82.
4. See esp. Shapin and Schaffer, *Leviathan* (ref. 1), 39, 58–59; Steven Shapin, *A social history of truth* (Chicago, 1994); *idem*, "'A scholar and a gentleman': The problematic identity of the scientific practitioner in early modern England", *History of science*, xxix (1991), 279–327; *idem*, "The house of experiment in seventeenth-century England", *Isis*, lxxix (1988), 373–404; *idem*, "Who was Robert Hooke?", in Michael Hunter and Simon Schaffer (eds), *Robert Hooke: New studies* (Woodbridge, 1989), 253–85, esp. pp. 280–5; Peter Dear, *Discipline and experience: The mathematical way in the scientific revolution* (Chicago, 1995); Mario Biagioli, *Galileo courtier: The practice of science in the culture of absolutism* (Chicago, 1993), esp. pp. 16–18; *idem*, "Scientific revolution, social bricolage, and etiquette", in Roy Porter and Mikuláš Teich (eds), *The scientific revolution in national context* (Cambridge, 1992), 11–54; and *idem*, "Etiquette, interdependence, and sociability in seventeenth-century science", *Critical inquiry*, xxii (1996), 193–238. Also see Dear, "Totius in verba" (ref. 3), 155–6, and Shapin and Schaffer, *Leviathan* (ref. 1), 58–59,

which raise the issue of the gentlemanly status of the members of the Royal Society and credibility, but in a somewhat weaker version than in the later work.

5. Quoted in Dear, "Totius in verba" (ref. 3), 156; for counter-examples of the view that ordinary people were not trusted by gentlemen, see Shapin, "Robert Hooke" (ref. 4), 281; *idem*, *Social history of truth* (ref. 4), 86–95.
6. Montaigne, "Of cannibals", quoted from the translation of Donald M. Frame, *The complete essays of Montaigne* (Stanford, 1958), 151–2. Also quoted in William Eamon, *Science and the secrets of nature: Books of secrets in medieval and early modern culture* (Princeton, 1994), 266.
7. Anthony Pagden, *European encounters with the New World* (New Haven, 1993), 56–68, esp. pp. 66–68.
8. For the best recent statement of the centrality of empiricism and experience in the development of early modern science, see Eamon, *Science and the secrets of nature* (ref. 6).
9. Robert Boyle, *The usefulnesse of experimental naturall philosophy* (Oxford, 1663), Part 2, pp. 220–1.
10. We have both published studies that focus on learned institutions, and we are not disavowing the significance of such work.
11. Shapin acknowledges this possibility in his introduction to *Social history of truth* (ref. 4), p. xvii. Also see Adrian Johns, "The ideal of scientific collaboration: The 'man of science' and the diffusion of knowledge", in Hans Bots and Françoise Wequet (eds), *Commercium litterarium, 1600–1750* (Amsterdam, 1994), 3–22.
12. For instance, see Latour's *Science in action: How to follow scientists and engineers through society* (Cambridge, Mass., 1987), and "Drawing things together", in Michael Lynch and Steve Woolgar (eds), *Representations in scientific practice* (Cambridge, Mass., 1990), 19–68.
13. For recent summaries of the field, see Stanley Wasserman and Katherine Faust, *Social network analysis: Methods and applications* (Cambridge, 1994) and John Scott, *Social network analysis: A handbook* (London, 1991). We want to thank Jennifer Boyce Hendriks for putting us in touch with some of the most significant literature in social network analysis.
14. Mark S. Granovetter, "The strength of weak ties", *American journal of sociology*, lxxviii (1973), 1360–80; *idem*, "The strength of weak ties: A network theory revisited", in Peter V. Marsden and Nan Lin (eds), *Social structure and network analysis* (Beverly Hills, 1982), 105–30.
15. Granovetter, "Strength of weak ties" (ref. 14, 1973), 1366, 1367–8, 1376, 1378. Latour's model suggests that all ties are of equal strength, although different people in a network might accumulate or deploy more of them. For an earlier appreciation of the significance of Granovetter's work in raising questions about scientific circles as units of analysis, see H. M. Collins, "The TEA set: Tacit knowledge and scientific networks", *Social studies*, iv (1974), 165–86, esp. p. 169.
16. Lesley Milroy and James Milroy, "Social network and social class: Toward an integrated sociolinguistic model", *Language in society*, xxi (1992), 1–26, p. 1. Also see Lesley Milroy and Sue Margrain, "Vernacular language loyalty and social network", *Language in society*, ix (1980), 43–70; James Milroy and Lesley Milroy, "Linguistic change, social network and speaker innovation", *Journal of linguistics*, xxi (1985), 339–84; and Lesley Milroy, *Language and social networks*, 2nd edn (Oxford, 1987).
17. Milroy and Milroy, "Linguistic change" (ref. 16), 381–2.
18. The question of how information and ideas are communicated among people who have never met poses a further intriguing problem, but it is one that we will need to set aside here. In

early modern Europe, of course, perhaps the best way of establishing multiple ties among strangers was to resort to print. Words coming from complete strangers in this way had to be evaluated based on a number of criteria, including the expectations of the reader.

19. Herman Busschof [*sic*] and Hermann Roonhuis, *Two treatises, the one medical, of the gout, ... the other partly chirurgical, partly medical* (London, 1676), 73–76; the original was published as Hermannus Busschoff, *Het podagra nader als oijt nagevorst en uijtgevonden, mitsgaders des selfs sekere genesing of ontlastent hulpmiddle* (Amsterdam, 1675); a summary credited to Busschoff was later published in Steven Blankaart (ed.), *Collectanea medico-physica, oft Hollands jaar-register: Der genees- en natuur-kundige aanmerkingen van gantsch Europa, etc. Tweede en derde deel des jaars MCDCLXXXI en LXXXII* (Amsterdam, 1683), 18–20.
20. Thomas Birch, *The history of the Royal Society of London for improving natural knowledge* (4 vols, 1756–57; facsim. Hildesheim, 1968), iv, 119–20.
21. *Philosophical transactions*, iv, no. 49 (19 July 1669), 983–6.
22. Busschof and Roonhuis, *Two treatises* (ref. 19), 73–76.
23. J. A. Wrop (ed.), *De briefwisseling van Constantijn Huygens*, vi: *1663–1687* (The Hague, 1917), letter no. 6995, "Aan H. Oldenburg, A la Haye, ce 16/26 Nov. 1675", pp. 368–9.
24. His story is published in [Sir William Temple], *Miscellanea* (London, 1680), 189–238; Temple's story is summarized in G. Rosen, "Sir William Temple and the therapeutic use of moxa for gout in England", *Bulletin of the history of medicine*, xliv (1970), 31–39 (although Rosen was unaware that "Zulichem" was Huygens).
25. A. Leeuwenhoek, letter of 14 May 1677, *Philosophical transactions*, xii, no. 136 (25 June 1677), 899–95 [*sic* for 905]. Leeuwenhoek possessed a copy of Busschoff's book: L. C. Palm, "Italian influences on Antoni van Leeuwenhoek", in C. S. Maffioli and L. C. Palm (eds), *Italian scientists in the Low Countries in the XVIIth and XVIIIth centuries* (Amsterdam, 1989), 147–63.
26. Thomas Sydenham, *Tractatus de podagra et hydrope* (London, 1683); transl. into English in 1684 by James Drake, and several times thereafter. The passage occurs at the third paragraph from the end.
27. Letter of Dr Johannes Jacob Wepfer to Casparus Sibelius, 20 August 1684, quoted in a letter of Sibelius to Locke, E. S. De Beer (ed.), *The correspondence of John Locke* (8 vols, Oxford, 1976–89), ii, 635.
28. On investigators not mentioning the names of those they worked with, see Shapin, *Social history of truth* (ref. 4), 363–5. We may assume that Busschoff's ties to his wife were strong, so the fact that he does not mention her name suggests the masculine orientation of his correspondence.
29. For a summary of the general importance of geographical mobility of early modern Europeans, see Michael W. Flinn, *The European demographic system, 1500–1820* (Baltimore, 1981); a fundamental study for Britain was E. A. Wrigley's "A simple model of London's importance in changing English society and economy, 1650–1750", *Past and present*, no. 37 (1967), 44–70. Perhaps European regions typified by extended family structures saw somewhat less mobility (Alan Macfarlane, *The origins of English individualism: The family, property and social transition* (New York, 1978)), but the rapid urbanization of Europe due in large part to migration into rapidly expanding urban centres (Jan De Vries, *European urbanization, 1500–1800* (Cambridge, Mass., 1984)) reinforces the sense of population movement.
30. There have been many studies of the *voyage savant*, to which we are most indebted. See, for example, H. Bots, "Het grote boek van de wereld: 'Bron' van Lering en Vermaak", *Kleio*,

xx (1979), 118–25; W. Th. M. Frijhoff, *La Société Néerlandaise et ses gradués, 1575–1814: Une recherche sérielle sur le statut des intellectuels à pertir des registres universitaires* (Amsterdam, 1981); Paul Dibon and Françoise Waquet, *Johannes Fredericus Gronovius: Pèlerin de la République des lettres; Recherches sur le voyage savant au XVIIe siècle* (Genève, 1984); and André Robinet, *G. W. Leibniz Iter Italicum (Mars 1689 – Mars 1690), La dynamique de la République des lettres: Nombreux texts inédits* (Florence, 1988). On travel and natural history, see Paula Findlen, *Possessing nature: Museums, collecting and scientific culture in early modern Italy* (Berkeley, 1994), 155–93.

31. N. H. Clulee, *John Dee's natural philosophy: Between science and religion* (London, 1989), 26–29.
32. Frank, *Harvey* (ref. 2).
33. Shapin and Shaffer, *Leviathan* (ref. 1), 223–82; Alice Stroup, "Christiaan Huygens and the development of the air pump", *Janus*, lxviii (1981), 129–58.
34. Charles Webster, "Thomas Linacre and the foundation of the College of Physicians", in F. Maddison, M. Pelling, and C. Webster (eds), *Essays on the life and works of Thomas Linacre, c. 1460–1524* (Oxford, 1977), 198–222; Gweneth Whitteridge, "Some Italian precursors of the Royal College of Physicians", *Journal of the Royal College Physicians, London*, xii (1977), 67–80; Harold J. Cook, *The decline of the old medical regime in Stuart London* (Ithaca, N.Y., 1986), 71–72; Edward Chaney, *The Grand Tour and the Great Rebellion: Richard Lassels and 'The voyage to Italy' in the seventeenth century* (Geneva, 1985), 341; Letter no. 276, Oldenburg to Boyle, 10 June 1663, in A. Rupert Hall and Marie Boas Hall (eds), *Correspondence of Henry Oldenburg* (Madison, Wis., London, and Philadelphia, 1965–86) (hereafter cited as *CHO*). Also, S. Sorbière, *Relation d'un voyage en Angleterre* (1664), which brought a reply in Thomas Sprat, *Observations on Monsieur de Sorbier's voyage into England* (1665).
35. For a general impression, see Hugh R. Trevor-Roper, *The crisis of the seventeenth century: Religion, the Reformation, and social change* (New York, 1968); Charles Webster, *The great instauration: Science, medicine and reform 1626–1660* (New York, 1975); Robert H. Kargon, *Atomism in England from Hariot to Newton* (Oxford, 1966), 68–77, 134. For a more detailed account of the Newcastle circle in exile, see Chaney, *Grand Tour* (ref. 34), 54–55, 309. Thomas Hobbes, for example, adopted a materialist view during a sojourn in France: Richard Tuck, "Hobbes and Descartes", in G. A. J. Rogers and Alan Ryan (eds), *Perspectives on Thomas Hobbes*, (Oxford, 1988), 11–41 (but *cf.* Perez Zagorin, "Hobbes's early philosophical development", *Journal of the history of ideas*, liv (1993), 505–18).
36. David Stevenson, "Masonry, symbolism and ethics in the life of Sir Robert Moray, FRS", *Proceedings of the Society of Antiquaries of Scotland*, cxiv (1984), 405–31; Alexander Robertson, *The life of Sir Robert Moray: Soldier, statesman and man of science (1608–1673)* (London, 1922).
37. Paul H. Hardacre, "The Royalists in exile during the Puritan Revolution, 1642–1660", *Huntington Library quarterly*, xvi (1963), 353–70; Chaney, *Grand Tour* (ref. 34); John Stoye, *English travellers abroad, 1604–1667*, rev. edn (New Haven, 1989); Jeremy Black, *The British and the Grand Tour* (London, 1985).
38. Stoye, *English travellers* (ref. 37), p. x; E. S. Bates, *Touring in 1600: A study of the development of travel as a means of education* (Boston and New York, 1911); Anna Frank-van Westrienen, *De Groote Tour: Tekening van de educatiereis der Nederlanders in de zeventiende eeuw* (Amsterdam, 1983); Edward Chaney, "Quo vadis? Travel as education and the impact of Italy in the sixteenth century", in Peter Cunningham and Colin Brock

(eds), *International currents in educational ideas and practices* (History of Education Society, Evington, 1988), 1–28; Xenja von Ertzdorff and Dieter Neukirch (eds), Rudolf Schulz (asst.), *Reisen und Reiseliteratur im Mittelalter und in der frühen Neuzeit* (Amsterdam and Atlanta, 1992); Sara Warneke, *Images of the educational traveller in early modern England* (Leiden, 1995).

39. Michael Hunter, *Establishing the new science: The experience of the early Royal Society* (Woodbridge, 1989), 73–121, esp. pp. 93–99; some of the instructions are printed in William Phineas Stearns, *Science in the British colonies of America* (Urbana, 1970), 687–90, 694–707, and in Frantz, *English traveller* (ref. 41), 15–29.
40. Severinus quoted in Eamon, *Science and the secrets of nature* (ref. 6), 161–2; on Severinus, see Jole Shackelford, "Paracelsianism in Denmark and Norway in the 16th and 17th Centuries", Ph.D. dissertation, University of Wisconsin–Madison, 1989.
41. Francis Bacon, "Of travel", quoted from Bacon, in Sidney Warhaft (ed.) *A selection of his works* (New York, 1965), 90. Also see R. W. Frantz, *The English traveller and the movement of ideas, 1660–1732* (1st publ. 1934; Lincoln, Neb., 1967), 30–71.
42. Ferd. Sassen, "De Reis van Marin Mersenne in de Nederlanden (1630)", *Mededelingen van de Koninklijke Vlaamse Academie voor Wetenschappen, Letteren en schone Kunsten van België, Klasse der Letteren*, xiv/4 (Brussels, 1964).
43. From C. D. van Strien, *British travellers in Holland during the Stuart period: Edward Browne and John Locke as tourists in the United Provinces* (Leiden, 1993), 7; on the English gentleman's contempt for pedantry, see Shapin, *Social history of truth* (ref. 4), 114–19; Shapin, "'A scholar and a gentleman'" (ref. 4).
44. Michael Hunter, "Alchemy, magic and moralism in the thought of Robert Boyle", *The British journal for the history of science*, xxiii (1989), 387–410, and *idem*, "The conscience of Robert Boyle: Functionalism, 'dysfunctionalism' and the task of historical understanding", in J. V. Field and Frank A. J. L. James (eds), *Renaissance and revolution* (Cambridge, 1993), 147–59; J. V. Golinski, "A noble spectacle: Phosphorus and the public cultures of science in the early Royal Society", *Isis*, lxxx (1989), 11–39; B. C. Southgate, "'Forgotten and lost': Some reactions to autonomous science in the seventeenth century", *Journal of the history of ideas*, l (1989), 249–68, esp. pp. 266–7; Peter Dear, "Miracles, experiments, and the ordinary course of nature", *Isis*, lxxxi (1990), 663–83.
45. Quoted in Shapiro, "Concept of 'fact'" (ref. 1), 5.
46. Eleazar Duncan, *The copy of a letter written by E. D. Doctour of Physicke to a Gentleman, by whom it was published* (London, 1606), 20–21. For further remarks on controversies about whether simple people might be accounted truthful, see Eamon, *Science and the secrets of nature* (ref. 6), esp. pp. 259–66; and on the issue of character, see Harold J. Cook, "Good advice and little medicine: The professional authority of early modern English physicians", *Journal of British studies*, xxxiii (1994), 1–31; David Harley, "The good physician and the godly doctor: The exemplary life of John Tylston of Chester (1663–99)", *The seventeenth century*, ix (1994), 93–117.
47. Meeting of 18 January 1681/2: in Birch, *History of the Royal Society* (ref. 20), iv, 119.
48. Ten Rhijne had been introduced to the practice by Busschoff.
49. Wilhem ten Rhijne to Henry Oldenburg, 23 July 1681, LBC.8.240–242, Royal Society of London. Because it was received after Oldenburg's death, the letter (3138) is summarized rather than printed in *CHO*.
50. Birch, *History of the Royal Society* (ref. 20), iv, 122, 140.
51. Willem Ten Rhijne, *Dissertatio de arthritide: Mantissa schematica: de acupunctura: et orationes tres, I. de chymiae ac botaniae antiquitate et dignitate. II. de physiognomia.*

III. de monstris (London, 1683). For a more elaborate account of this episode, see Harold J. Cook, *Trials of an ordinary doctor: Joannes Groenevelt in seventeenth-century London* (Baltimore, 1994), 125–8.

52. *CHO*, 2209, De Graaf to Oldenburg, 18 April 1673. In turn, De Graaf's first letter to Oldenburg is *CHO*, 911, 20 July 1668. De Graaf wrote this letter at the urging of someone who had just met Oldenburg, Matthias Paisen (913, *c.* 11 July 1668), and he sent the letter and a copy of his book on the parts of generation via his "dear friend" ("integerrimus amicus") Dr Vincke, who was on his way to visit England. When De Graaf's letter and book were discussed at a meeting of the Royal Society, one of De Graaf's acquaintances, Mr Duijst van Voorhout, was present, perhaps to add further testimony about De Graaf. Moreover, in the ensuing debate over De Graaf's discoveries, De Graaf relied on the testimony of people who had seen his work in person to travel to England as witnesses to his work: ex: *CHO*, 967.
53. For his early experience in England, see Rosalie Colie, *'Some thankfulnesse to Constantine': A study of English influence upon the early works of Constantijn Huygens* (The Hague, 1956); A. G. H. Bachrach, *Sir Constantine Huygens and Britain, 1596–1687: A pattern of cultural exchange*, i: *1596–1619* (Leiden and London, 1962).
54. L. C. Palm, "Leeuwenhoek and other Dutch correspondents of the Royal Society", *Notes and records of the Royal Society*, xliii (1989), 191–207.
55. *Ibid.*; for Shapin's views of Leeuwenhoek's reliance on local testimony of witnesses, see *Social history of truth* (ref. 4), 306–7.
56. *CHO*, nos. 363–477.
57. These numbers would be 18 and 8 if the letter from Cassini is included — but this was a copy of a letter Cassini sent to someone else, included in a letter to Oldenburg from Auzot.
58. *CHO*, nos. 363, 369, 380, 381, 393, 414.
59. *CHO*, nos. 390, 448.
60. *CHO*, nos. 408 and 398, 422, 449, respectively.
61. *CHO*, nos. 375, 404, 413; 361; and 364 respectively.
62. Martha Ornstein, *The rôle of scientific societies in the seventeenth century* (1st pub. 1913; Chicago, 1928), 169–75; F. J. Cole, *A history of comparative anatomy: From Aristotle to the eighteenth century* (London, 1944), 341–69; James E. McClellan, III, *Science reorganized: Scientific societies in the eighteenth century* (New York, 1985), 55.
63. Probably his book on the circulation of the blood, which places the circulation of the blood in the tradition of Athanasius Kircher and the Hermetic philosophy: Philipp Jacob Sachs, *Oceanus macro-microcosmicus seu dissertatio epistolica de analogo motu aquarum ex & ad oceanum, sanguinis ex & ad cor.* (Vratislava, 1664), although perhaps his *Αμπελογραφία sive vitis viniferae ejusque partium consideratio physico-philologico-historico-medico-chymica* (Leipsig, 1661) or his *Γαμμαρολογια, sive gammarorum, vulgo cancrorum consideratio physico-philologica-historico-medico-chymica* (1665), the latter of which we have not seen.
64. We therefore disagree with Mario Biagioli's claim that the Royal Society's network of correspondence was self-consciously developed to make up for their lack of direct princely patronage: Biagioli, "Etiquette, interdependence, and sociability" (ref. 4), 227.
65. On Finch and Baines, see Chaney, *Grand Tour* (ref. 34), 341; W. E. Knowles Middleton, *The experimenters: A study of the Accademia del Cimento* (Baltimore, 1971), 286–91; Middleton (ed. and transl.), *Lorenzo Magalotti at the court of Charles II: His Relazione d'Inghilterra of 1668* (Waterloo, Ontario, 1980); on the failed attempts of Robert Southwell

to establish connections between the Accademia del Cimento and the English virtuosi, see Middleton, *Experimenters*, 282–6.

66. In the same way, the *Philosophical transactions* remained Oldenburg's private venture, but quickly became associated with the Society.
67. Middleton, *Experimenters* (ref. 65), 281; on the correspondence of members, 281–308. At the end of the century Martin Lister reported that the Marquis de L'Hôpital blamed the short lived nature of the Cimento on the small number of members and their "very little correspondence": quoted in Biagioli, "Etiquette, interdependence, and sociability" (ref. 4), 226. For Biagioli's views of the Cimento more generally, see his *Galileo courtier* (ref. 4), 358–62; and *idem*, "Scientific revolution, social bricolage, and etiquette" (ref. 4), 25–32.
68. Dr Casparus Sibelius to Locke, 961, 9/19 September 1687, in E. S. De Beer (ed.), *The correspondence of John Locke* (8 vols, Oxford, 1976–89), iii, 265–6, De Beer's translation.
69. Also see Saskia Stegeman, "How to set up a scholarly correspondence: Theodorus Janssonius van Almeloveen (1657–1712) aspires to membership in the Republic of Letters", *Lias*, xx (1993), 227–43, who notes that Van Almeloveen was able to establish a rich correspondence network without personal travels only by deploying "a whole network of acquaintances and family ties" who prepared his way (p. 233).
70. Howard B. Adelmann, *Marcello Malpighi and the evolution of embryology* (5 vols, Ithaca, 1966), i, 337–8; *idem* (ed.), *The correspondence of Marcello Malpighi* (5 vols, Ithaca, 1975), i, 347–8, 354–7; *CHO*, 740.
71. *CHO*, 390a, Lyons, 28 July 1665, — to Justel.
72. Justel's own letter to Oldenburg detailing the Parisian discussion of the mirror is now lost, as are virtually all of Justel's letters to Oldenburg during this period. The inference that Justel read the letter at the Thévenot derives from the fact that André Graindorge described the mirror in terms that paraphrase the account Oldenburg received. That Graindorge had learned of the mirror from an account read at the Thévenot on Tuesday, 4 August 1665 is clear in the letter he wrote to Pierre-Daniel Huet the next day. A transcription of that letter appears in Leon Tolmer's "Vingt-deux lettres inédites d'André de Graindorge à P.-D. Huet", *Mémoires de l'Académie Nationale de Caen*, n.s., x (1943), 303.
73. *CHO*, 390a, — to Justel, 28 July 1665.
74. *CHO*, 391, Oldenburg to Boyle, 10 August 1665.
75. *CHO*, 392, Oldenburg to Moray, 11 August 1665.
76. Oldenburg's published account appeared in the sixth number of the *Philosophical transactions*, which came off the press in Oxford during the first week of November 1665. *CHO*, 446, 5 November 1665, Moray to Oldenburg.
77. With regard to the Thévenot Academy, it is important to note that, despite a widespread misperception in the literature on French science, this organization continued to operate well beyond mid-1665. See David Lux, *Patronage and royal science in seventeenth-century France: The Académie de Physique in Caen* (Ithaca, N.Y., 1989), 29–56; *idem*, "Colbert's plan for the Grande Académie: Royal policy toward science, 1663–1667", *Seventeenth-century French studies*, xii (1990), 177–88.
78. Although Middleton mistakenly attributed all the Cimento's correspondence with Paris during this period to activity at the Montmor Academy, his identification of Melchisedec Thévenot himself as a primary transalpine contact after 1660 actually supports the significance of the Thévenot Academy in this role (*Experimenters* (ref. 65), 304–9).
79. The communications in this case were routine. Oldenburg expected his news from Paris twice weekly, and when things moved well, he received his news from Paris just five days

from the date on his correspondent's letter. In France, letters passed with comparable speed. The mail, for example, also moved twice weekly between Paris and Caen, and a letter posted at noon on Wednesday in Paris could be read at the Académie de Physique in Caen Thursday evening — something less than 36 hours after it left Paris. A letter posted in Caen on Saturday, was waiting for André Graindorge when he picked it up in Paris on Monday morning — ample time for reading at the next session of the Thévenot Academy late Tuesday afternoon: Lux, *Patronage and royal science* (ref. 77), 31–38.

Thus, for the scale of the communications between Oldenburg and the Parisians attending the Thévenot, the 79 days between the date on the original report from Lyon (28 July 1665 [N.S.]) and the date on Oldenburg's letter telling Boyle he had his final confirmation from France (5 October 1665 [O.S.]) represented an entirely reasonable cycle time for communications (Lyon–Paris, Paris–London, London–Paris, Paris–Lyon and Paris–Italy, Italy and Lyon–Paris, Paris–London). In fact, when Oldenburg wrote to tell Boyle he had "the thing well attested", he revealed he had received the final confirmation from Paris "last week". In other words, the central cycle time for all the exchanges in Oldenburg's cross-channel conversation was something less than 79 days.

80. Justel's reputation is decidedly mixed. A. Rupert Hall and Marie Boas Hall, for example, describe Justel simply as a political correspondent and claim Oldenburg "presumably received some compensation for his trouble" in passing on Justel's news to the office of the Secretary of State because "only monetary gain can explain how Oldenburg could tolerate Justel's nearly unreadable hand and boringly repetitious gossip for so many years" (*CHO*, ii, p. xxv). On the other hand, Harcourt Brown described Justel as one of the most important intelligencers in seventeenth-century science: "The benefit which the work of the Royal Society received from the exchanges between Justel and its successive secretaries is almost beyond calculation; what Justel did directly and indirectly for the dissemination of English books, news and science over the continent of Europe is not equalled before the eighteenth century, and only occasionally surpassed then": Brown, *Scientific organizations in seventeenth century France* (Baltimore, 1934), 162–3.
81. Justel to Oldenburg, *CHO*, 768, 771, 778, 787, 816, 15 February – 18 March 1668.
82. Lux, *Patronage and royal science* (ref. 77), 121–5.
83. For a succinct statement on the historical complexity of this interpretive problem particularly as it applies to the Royal Society see Michael Hunter, "First steps in institutionalization: The role of the Royal Society of London", in *Solomon's House revisted: The organization and institutionalization of science* (Nobel Symposium no. 75; Canton, Mass., 1990), 13–30. For an excellent bibliography on the early history of the Royal Society, see Marie Boas Hall, *Promoting experimental learning: Experiment and the Royal Society, 1660–1727* (Cambridge, 1991). Michael Hunter's works, especially *Science and society in Restoration England* (Cambridge, 1981) and *Establishing the new science: The experience of the Royal Society* (Woodbridge, 1989), remain the most thorough treatment of the early Royal Society.

For the Académie Royale des Sciences, Alice Stroup's recent *A company of scientists: Botany, patronage, and community at the seventeenth-century Parisian Royal Academy of Sciences* (Berkeley, Cal., 1990) goes far beyond earlier work in explaining that institution's role in the social history of French science during the second half of the seventeenth century. For a general review of the interpretive problems encountered in work on the reorganization of science in the 1660s, see David S. Lux, "Societies, circles, academies, and organizations: An historiographic essay on seventeenth-century science", in Peter Barker and Roger Ariew (eds), *Revolution and continuity: Essays in the history and philosophy of early-modern science* (Washington, D.C., 1991), 23–43.

84. Lux, "Colbert's plan" (ref. 77).
85. See Stroup, *A company of scientists* (ref. 83), 199–217; Lux, *Patronage and royal science* (ref. 77), 51–56.
86. André Graindorge put this point succinctly when describing what happened to submissions to the Académie Royale from Caen: "Anything entering the coffers at the academy in Paris never reappears" (Graindorge to Huet, Biblioteca Medicea-Laurenziana, Ashburnham 1866, 624 (20 January 1674)). For a more complete discussion on this point, see Lux, *Patronage and royal science* (ref. 77), 174–9.
87. *CHO*, 526, 26 May 1666 [N.S.], Justel to Oldenburg.
88. *CHO*, 888, 26 June 1668, Auzout to Oldenburg.
89. *CHO*, 589, 28 December 1666, Auzout to Oldenburg [Oldenburg's translation].
90. *CHO*, 730, 29 December 1667, Auzout to Oldenburg.
91. *CHO*, 526, 26 May 1666, Justel to Oldenburg.
92. *CHO*, 573, 13 October 1666, Justel to Oldenburg.
93. *CHO*, 622, 30 March 1667, Justel to Oldenburg.
94. *CHO*, 721, n.d. [December 1667], Justel to Oldenburg.
95. *CHO*, 834, 18 April 1668, Justel to Oldenburg.
96. *CHO*, 841, 2 May 1668, Justel to Oldenburg.
97. *CHO*, 877, 13 June 1668, Justel to Oldenburg.
98. *CHO*, 894, 7 July 1668, Justel to Oldenburg.
99. *CHO*, 978, n.d. [October 1668], Justel to Oldenburg.
100. *CHO*, 994, 10 November 1668, Justel to Oldenburg.
101. *CHO*, 1024, 8 December 1668, Justel to Oldenburg.
102. Biblioteca Medicea-Laurenziana, Ashburnham 1866, 572, 16 January 1668, Graindorge to Huet.
103. Lux, *Patronage and royal science* (ref. 77), 81–139.
104. *CHO*, 1159, 11 May 1669, Vernon to Oldenburg.
105. For example, the work of Robert Hooke as demonstrator to the Royal Society would seem to fit this pattern nicely. See esp. Stephen Pumfrey, "Ideas above his station: A social study of Hooke's curatorship of experiments", *History of science*, xxix (1991), 1–44; Michael Aaron Dennis, "Graphic understanding: Instruments and interpretation in Robert Hooke's *Micrographia*", *Science in context*, iii (1989), 309–64.
106. This suggests that viewing science in action situated in one location (as does the stimulating work of Bruno Latour and Steve Woolgar, *Laboratory life: The construction of scientific facts*, 2nd edn (Princeton, 1986)), without taking account of the many people coming and going to and from other sites, overlooks something essential.