

Chemicals as Instruments

A Language Game

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Abstract: Meaning is use: Wittgenstein's well-known dictum is used as starting point for a language game on the English word 'instrument' in historical discourse. In this way it is possible to collect a set of words (and corresponding objects) so heterogeneous that the likening 'chemicals as instruments' does not seem misplaced. Looking for a better understanding, three classes of chemicals are considered: solvents, indicators, and reagents (just a couple!). The first two classes comprise chemicals, which create new experimental conditions (as the classical air pump), or measure 'something' (as the classical thermometer). The third class is more peculiar to chemistry, in that reagents are typical chemical instruments for operating at the microscopic level. In addition, a second language-game is proposed, and it is stressed the deep epistemological difference between physics, which creates 'phenomena', and chemistry, which synthesizes substances.

Keywords: *chemicals, instruments, language game, epistemology of solvents, physical phenomena, chemical substances.*

It might be said: if you have given yourself a private definition of a word, then you must inwardly undertake to use the word in such-and-such a way. And how do you undertake that? Is it to be assumed that you invent the technique of using the word; or that you found it ready-made?

L. Wittgenstein, *Philosophical Investigations*, § 262

Introduction

In November 1996, on behalf of the European Science Foundation (ESF), a workshop on 'Chemistry Laboratories, New Technologies and Education' was organized in Lisbon. On that occasion I presented a joint paper on

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'Cannizzaro's Laboratory Life', in which an ample part was dedicated to the scientific and personal relations between Cannizzaro, his master Raffaele Piria and his best friend, Cesare Bertagnini. While I was studying the correspondence of these three excellent organic chemists, I was deeply stricken by the fact that every chemical substance was extremely precious for them, as it is evident in the following episode. At the very moment in which he was able to work in the new laboratory in Alessandria (a provincial town in Piedmont) Cannizzaro offered his master and his friend samples of cyanamide, a compound he had obtained for the first time during his Paris stay. This project too, simple as it seems, had its difficulties because "some ether" was necessary for the preparation and he had resigned himself "to prepare it at once and to purify it in order to get it anhydrous". In the same letter, which I have quoted, he describes in great detail his preparation method, and remarks that for the different steps of the synthesis it was necessary to have ready anhydrous ether, mercury cyanide, methylamine hydrochloride, methylcyanide, potassium cyanide and methyl-sulphate (Rambaldi and Cerruti, in press). This list means that even an apparently simple synthesis was a challenging task for a peripheral, isolated researcher. A second fact, which was recurrent in the scientific life of the Italian trio, was the search for sellers of chemicals and instruments, wherever they were living, visiting or simply traveling; moreover the double trade of chemicals and scientific instruments is well documented for many European countries in the eighteenth and nineteenth century (Brock 1992, pp. 185-193). This very concrete situation convinced me that those chemicals and instruments were – not only for Cannizzaro & Co. – the same thing from the point of view of laboratory life. After some time of private use of the likening 'chemicals as instruments' I convinced myself that a language game on the issue could be amusing (at least for me), and the present paper is the 'result' of the game. My intentions will be clearer, if we read one of Wittgenstein's definition of language game: "the term '*language game*' is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or of a form of life." Among many examples of language games the Austrian philosopher mentions "Forming and testing a hypothesis" and "Making a joke; telling it" (Wittgenstein 1986, § 23). It is evident that 'chemicals as instruments' may be more than a hypothesis, as for me and Joachim Schummer (1997, p. 85), or may seem to be simply a joke, as (I suppose) for many philosophers of physics.

In the above quoted ESF workshop and in other research contexts (Franklin 1989, Rothbart and Scherer 1997), attention has been given to the epistemological problems of using more or less sophisticated chemical instruments, and of course historians have always spoken of chemical instrumentation *stricto sensu*. In discussing the birth of the "cabinet of Baconian instruments" in the seventeenth century, Kuhn recalls "the rapid

introduction and exploitation of telescopes, microscopes, thermometers, barometers, air pumps, electric charge detectors, and numerous other new experimental devices". In this cataloguing vein Kuhn adds: "The same period was characterized by the rapid adoption by students of nature of an arsenal of chemical apparatus previously to be found only in the workshops of practical craftsmen and the retreats of alchemical adepts", and concludes: "In less than a century physical science became instrumental" (Kuhn 1977, pp. 44-45). It is noteworthy that Kuhn's does not mention by name any apparatus of the 'chemical arsenal'. In this context one has to mind the (historical) fact that the named physical instruments aroused spirited arguments between learned men of any sort, just in order to settle the scientific meaning of the instruments' use. Nothing of similar happened for the chemical apparatus, including the many different instruments used in distillation, the fundamental chemical operation of the period (Partington 1961, pp. 82-89). The chemical apparatus were neither 'philosophical' nor 'mathematical' (Hackmann 1989), anyway not enough to attract the critical attention of learned people. In a later period, an ingenious experimenter and apparatus innovator as Priestley positively wrote: "By philosophical instruments [I mean] the air pump, condensing engine, pyrometer (with which electrical machines are to be ranked) and which exhibit the operations of nature" (1775, cited in Hackmann 1989, p. 42). Through the use of the mercury pneumatic trough Priestley had brought to *perfection* the study and the manipulation of water-soluble gases (Brock 1992, p. 101; my italics approve Brock's opinion); however in the quoted passage from *The History and Present State of Electricity* he does not mention the principal instrumental means of his own discoveries as 'philosophical'.

The almost neglecting attitude towards the philosophical relevance of chemical apparatus has some reason. It is not due to epistemological disdain, but – in my opinion – to a crucial aspect of chemical research: the constant, and many times controversial, use of a great number of reagents, of 'chemical substances' which in their countless transformations constitute, at the same time, the beginning and the end, the cause and the aim of chemistry. Kuhn himself comes near to this disciplinary focal point when he examines the impact of new instrumentation on the development of the "new Baconian sciences". The attention to attraction phenomena of magnets and rubbed amber led to non-classical fields of systematic investigation as magnetism and electricity. "Both these fields were dependent for their subsequent development upon the elaboration of new, more powerful, and more refined instruments. They are typical new Baconian sciences." After this statement Kuhn remarks: "Chemistry presents a case of a different and far more complex sort. Many of its main *instruments, reagents, and techniques* had been developed long before the Scientific Revolution. But until the late sixteenth century

they were primarily the property of craftsmen, pharmacists, and alchemists. Only after a reevaluation of the crafts and of the manipulative techniques were they regularly deployed in the experimental search for natural knowledge.” (Kuhn 1977, pp. 46-47, my italics)

Kuhn’s excerpt contains several important suggestions. The relation of chemistry with the development of instrumentation is different and more complex than that of other physical sciences. In chemistry instruments, reagents, and techniques are an epistemic trinity, divisible only with difficulty: the couple instruments/techniques is inseparable in any experimental discipline, but it is the necessary presence of a multitude of reagents which is uncomfortable in chemistry (for the reductionist philosopher). Finally, Kuhn hints at an imposing *change of meaning*: a great part of the equipment of certain arts or crafts became in due course the instrumentation of experimental laboratories. I will now try to understand more deeply the kind of meaning to which the change refers.

1. Instruments: meaning is use

Wittgenstein’s well known definition of meaning (*Bedeutung*) is found in § 43 of the *Philosophical Investigations*, and Anscombe’s English rendering is: “the meaning of a word is its use in the language”. Just after this sentence we read: “And the *meaning* of a name is sometimes explained by pointing to its *bearer*.” (Wittgenstein 1986, pp. 20-21; italics in the text) Wittgenstein’s suggestions may be useful in the present research from several points of view. In the first place, we may look at the use of the word ‘instrument’ in the contemporary historiography and philosophy of science in order to obtain a few traits of the meaning of the word, and the correlate traits of the historians’ and philosophers’ interpretation of what is an object named ‘instrument’. In the second place, we will briefly consider the lexical and semantic fields which the word ‘instrument’ belongs to, within the obvious limits of the particular natural language that we are using (*e.g.* English); in this way we will be able to collect the traits of the meaning which are useful for an understanding of the likening ‘chemicals as instruments’. In the third place, we will later reflect on Wittgenstein’s stress on that particular relationship between the meaning of a name and the bearer of the name which is – often only tentatively – realized by ostension; ‘ostension’ may acquire a particular meaning in the context of chemical research (*vide infra*, section 5). Lastly, I mention here that when we choose a particular instrument we can, at any time, list its actual uses, and describe the rules of use in different experimental circumstances. The list of uses and the description of their rules

are important parts of the meaning of the name of an instrument, moreover the meaning is changing along with the changes in the actual use of the instrument (sometimes during the same process of its 'construction'). This diachronic aspect is important because it reveals several similarities between chemicals and the instruments used in physics or astronomy.

The scope of the current, implicit definition of 'instrument' is well documented in an issue of *Osiris* (Vol. 9, 1994) in which instruments are discussed in different contexts, ranging from authority and audience, to culture at large and life sciences. The instruments treated includes very different 'objects': the classical telescopes (from Galileo to Cassini), Lavoisier's apparatus (calorimeter, gasometer, etc.), devices used in precise electrical measurement, compasses of different kinds, the Stanford's supervoltage X-ray tube, charge-coupled devices used in the Hubble space telescope, the observational instrumentation of the Imperial Astronomical Bureau of the Ming dynasty (armillary sphere, gnomon, water clock, etc.), and so on, through a very dubious, and only imagined instrument as Castel's ocular harpsichord, and the demonstration devices in Georgian mechanics (including Atwood's machine). An analogous amazing variety of devices is found in the important book edited in 1989 by Gooding, Pinch and Schaffer, under the programmatic title *The Uses of Experiment*. Here we found, near the ubiquitous Atwood's machine, "the biggest machine in the world" for accelerating elementary particles, planned by CERN in the first 1950's (Krige 1989, p. 401). In the same book two very interesting essays discuss in particular the relevance of the uses of instruments for an understanding of the science endeavor. Their titles too are evocative: "Scientific instruments: models of brass and aids to discovery" (Hackmann 1989) and "A viol of water or a wedge of glass" (Bennett 1989).

Many of the 'instruments' discussed in *Osiris* and in *The Uses of Experiment* were connected with *measurement procedures* of physical nature (including Lavoisier's apparatus); other were constructed in order to *create and observe new, unusual phenomena* (in our times typically the particle accelerators, but in the seventeenth century also Boyle's air pump). These two aspects, obvious in our days, were not always so granted. Referring to the Scientific Revolution, and commenting on the essays of the quoted *Osiris* issue, van Helden and Hankins stress that "In the seventeenth century it was unclear how instruments like the telescope, microscope, and air pump should be used to obtain natural knowledge. [...] the instruments were new and there was no established convention for using them or for validating their results. [...] the new instruments of the Scientific Revolution seldom measured anything, at least not at first." (van Helden and Hankins 1994, p. 3)

The historical perspective discloses a whole collection of disparate 'objects' which functioned or function as scientific instrument. Usually the

contemporary philosophical perspective has been dramatically narrower, focusing on the research results (in connection with a so-and-so theory) and not on the experimental procedures which led to these results. The editors of *The uses of experiments* stated: “The *invisibility of instruments* is [...] an important if paradoxical consequence of experimental achievement. Recovering the role of instruments in experiment represents an important advance in the understanding of how scientists achieve certainty.” (Gooding *et al.* 1989, p. 5; my italics) However, the attention to the actual laboratory practices has been present in the philosophical analysis also before the ethnological study of the *Laboratory Life* by Latour and Woolgar (first edn. 1979). I refer, for example, to Michael Polanyi. After he contrasts “the supposed supremacy of experiment over theory” and illustrates the “power of scientific theory over scientific facts” (Polanyi 1983, p. 167; 1st edn. 1958), he discusses the relation between science and technology and introduces the notion of ‘practical performance’ to which “three kinds of observable things” belong: “(1) materials, (2) tools, including all manner of installations, and (3) processes”; “hammers, engines, houses, railways, are tools or installations”. A page later he states: “Technology teaches only actions to be undertaken for *material* advantages by use of *implements* according to (more or less) *specifiable rules*”, and in a note he explains that “The word ‘implements’ is meant to designate all three classes of useful things: materials, devices and processes” (Polanyi 1983, pp. 175-176).

To be sure, Polanyi’s separation between science and technology is too sharp, especially in the case of chemistry, a science that was always cultivated by scientists proud of their practical performances in laboratory. But I agree with Polanyi’s focus on ‘implements’, and with his wide definition of the things useful for research, because in the epistemological analysis of experimental procedures it is crucial to appreciate that – generally speaking – no ‘instrument’ may function isolated from other instruments. Distinctions have been made between devices and systems, in that systems are composed of devices put together for a practical performance, whereas devices are themselves ‘single entities’ (references in Smith and Tatarewicz 1994). However, it is highly questionable whether a bit of scientific equipment was or is *really* a *single* entity; anyway, scientists have worked at any time with systems of devices of many different kinds. It was so at the same beginning of the Scientific Revolution: it is sufficient to look at the famous tables which illustrates the experimental devices used by Boyle for his *New Experiments Physico-Mechanical, Touching the Spring of the Air* (1660). To this, the second aspect stressed by Polanyi is also to be added, namely the specifiable rules to be followed in the actual use of instruments. These rules are not at all simple, or stated once for ever. In this context the arguments developed by Bridgman regarding the measurement of temperature by a thermometer are important,

because Bridgman considers the use of a very simple, well-known instrument (a bulb thermometer), and demonstrates the difficulty of being sure about what one is actually measuring (Bridgman 1927).

In Hacking's "primer on the epistemology of experiment" (definition in Fuller 1989, p. 132), we found a good example of the multiplicity of names (and, implicitly, of 'objects' referred to) used in the description of the scientific activity by means of 'instruments'. At the beginning of the chapter dedicated to 'observation' Hacking uses three different terms, whose generality may be appreciated reading them in their immediate context: "Often the experimental task, [...] is less to observe and report, than to get some *bit of equipment* to exhibit phenomena in a reliable way. [...] The good experimenter is often the observant one who sees the instructive quirks or unexpected outcomes of this or that *bit of the equipment*. You will not get the *apparatus* working unless you are observant. [...] We usually observe objects or events with *instruments*." (Hacking 1990, pp. 167-168, my emphasis) The coherence of Hacking's discourse assures us that 'instrument', 'apparatus', and 'bit of equipment' belong to the same lexical field. To these terms we can add the other ones used by the authors of the preceding quotes: 'device', 'demonstration device', 'observational instrumentation', 'machine', 'models', 'tool', 'installation', 'implement'. It is a fundamental principle of the linguistic field theory that the value of a word is recognized only when the word is considered together with the other ones, which are 'near' or 'in opposition' to it (Geckeler 1971). It is obvious that in many contexts of the scientific discourse 'instrument' cannot be substituted by 'installation', but many other contexts might be found where the substitution is feasible without any difficulty (I think, for example, of installations for the research in hydraulics, as the test-tanks). We can now look for the traits of the semantic field which are able 'to attract' so different words in the same lexical field.

For the aims of the present paper it is not necessary to go beyond a simple consideration: all the listed 'useful things' function as 'instruments' when, and only when, they are used according to a set of rules in the context of a scientific and/or technological research. It is a particular knowledge function that gives something the status of an instrument. (If an alembic is used as a knick-knack, it is no more an instrument; it is a piece of furniture.) Thus, the semantic traits, which bring together those disparate words, must be connected to the practical, *particular use* of the referred 'objects' – and to the *particular rules* of their use (of the objects *and* of the words). An instrument has to exhibit "phenomena in a reliable way", but it may also disclose "instructive quirks or unexpected outcomes"; it is used to "observe objects or events" (Hacking, as quoted above), both "for *qualitative* observations" and "for *quantitative* measurement of properties" (Krige 1989, p. 402; italics in the text). Thus "The instrument must be able to isolate,

physically, the properties of the entities that we wish to use” (Hacking 1990, p. 265); eventually, and classically, scientific instruments make “visible what could not be seen by the unaided senses” (Hackmann 1989, p. 31). To these very general traits, another one, extremely important from the historical and epistemological points of view, is to be added. Van Helden and Hankins point out that “Because instruments determine what can be done, they also determine to some extent what can be thought”. The authors stress a positive aspect: “Often the instrument provides a possibility; it is an initiator of investigation.” (Van Helden and Hankins 1994, p. 4) I agree, and later we will see several related examples, but I also suggest that there is a dark side of the question, namely that many fashioned instruments have erased other valuable knowledge procedures, and that their academically profitable use has concealed other research opportunities.

In this section we have seen that several traits of the semantic content of the word ‘instrument’ are present in many other words, and that these particular traits became pertinent and evident when those words are used in the scientific/technological discourse. In the next three sections I will consider the uses of two classes of chemicals (solvents and indicators), and of few other substances: it will be easy to demonstrate that in many cases their scientific and technological use correspond to that of an ‘instrument’.

2. Solvents

If the meaning of the word ‘instrument’ has engaged us for a while, for the meaning of ‘chemical’ (*n.*) it is convenient to assume that the word is innocent and unpretentious, so that it is enough to open two or three ‘chemical’ (*adj.*) books in order to discover the essential uses of the word. The first choice is *The Merck Index*, whose sub-title is *An Encyclopedia of Chemicals, Drugs, and Biologicals*. The most conspicuous part of the huge book is “The Merck Index of Chemicals, Drugs, and Biologicals”, which is of 1741 pages and includes 10,330 monographs. In the Editor’s preface Susan Budavari tells us that over 2,000 monographs “describe common organic chemicals and laboratory reagents”, and that “An additional 1,000 monographs focus on the elements and on inorganic chemicals” (Budavari 1996, p. v). The use of mine of large numbers is somewhat rhetorical, but it is comfortable to know that there are thousands of *common* chemicals whose use wait for an epistemological analysis. The second book, which I propose to consult, is an old favorite of mine, published in 1973 by Reuben and Burstall. *The chemical economy* was an extraordinary and successful effort “to place the chemical industry in its social and economic perspective”, and the authors stated that their

aim was “to help the non-specialist [...] rather than to look good in the eyes of professionals who are always reluctant to believe that the eternal verities of their subjects can be encompassed in less than one thousand closely-printed pages” (Reuben and Burstall 1973, p. vii). This highly compact book lists in its compound index 428 substances of industrial interest, so we get a second sort of philosophical comfort: hundreds of chemicals have a social (albeit sometime controversial) meaning. The third (and last) book helpful for a direct contact with the chemical literature may be the first volume of a series published by Pizey under the title *Synthetic Reagents*. The program of the series assures that “Every volume will contain a small selective group of versatile and often used reagents[,] in general it will contain an example of a reductant, an oxidant, a solvent, halogenating material, and some other versatile reagents” (Pizey 1974, not numbered page). The first reagent discussed by Pizey is $\text{HCON}(\text{CH}_3)_2$, dimethylformamide or, in short, DMF. Here, at last, we may plunge in the depth of the chemistry complexity (but all around us the world is not simple; see Hoffmann *et al.* 1997).

DMF is described by Pizey as “an extremely useful and versatile liquid. It may be employed as a solvent for a wide variety of reactions or it may be used chemically, for example, as a formylating agent [...]. In certain cases DMF need only be present in catalytic quantities to accelerate the rate of reaction” (Pizey 1974, p. 4). These are the first lines of a long monograph of 99 pages, in which we learn that DMF is useful as a solvent or catalyst in 11 classes of reactions, divided into 58 sub-classes of reactions and preparations. In their turn, the synthetic uses are divided into 6 classes and 27 subclasses. The enormous ‘chemical heritage’ concentrated here by Pizey is more unfolded in the 566 quoted references. Pizey wrote his review a generation ago, and by then, without any doubt, the cumulated knowledge about DMF has increased; however, the packed *Merck Index* devotes only 33 lines to this ‘common chemical’. At the end of the monograph n. 3292, we read a syntactically condensed report about DMF: “Solvent for liqs and gases. In the synthesis of organic compounds. Solvent for Orlon and similar polyacrylic fibers. Wherever a solvent with a slow rate of evaporation is required. Has been termed the *universal organic solvent*.” (Budavari 1996, p. 549, added italics) The almost alchemic wording ‘universal solvent’ might evoke a long history, but this is not the case. DMF had first been synthesized in 1893, but even in 1955 a well-known manual on *Organic Solvents* dedicated to DMF only seven lines with two meager references (Riddick and Toops 1955, p. 450). What has changed a laboratory curiosity into a celebrated universal solvent?

After the discovery of nylon attempts were made to obtain fibres from acrylic polymers, but a major obstacle was the difficulty of spinning the melted polyacrylonitrile, because it is decomposed by heat. In 1942, in

Germany, H. Rein discovered the possibility to spin polyacrylonitrile when it is dissolved in DMF, and the fibre was produced for the first time in 1946 by Du Pont, under the trade name *Orlon* (Giua 1962, p. 165). The industrial production of acrylic fibres and the correlated DMF production gained importance only in the 1950s (Eberling 1980, p. 263). “Since then, many new applications have been found”, tells Eberling, who lists seven different ways of using DMF, including that of “reaction medium”, so deeply discussed by Pizey. In this respect the mass production of DMF put into the hands of chemists a new research instrument, to which the definition of “initiator of investigation” proposed by Van Helden and Hankins may be aptly applied. Many marvellous things could be told about the sensible uses of DMF, yet in the word ‘*medium*’ used by Eberling we have found a key word for our epistemological analysis, and we can proceed in this direction.

Shapin and Schaffer (1985) discussed the invention, development and experimental use of the air pump as a thread for “an exploration into the status of instruments in the seventeenth century” (Van Helden and Hankins 1994, p. 3). With the air pump Boyle created a new kind of ‘space’, in which strange phenomena could be produced, re-produced, studied and, possibly, explained (by the *Spring of the Air*, for example). Two centuries later sophisticated vacuum pumps led scientists to study new rays, particles, etc. in evacuated tubes. Even more fascinating is the history of C.T.R. Wilson’s cloud chamber, which Ernest Rutherford ranked as “the most original and wonderful instrument in scientific history”. Originated by Wilson’s interest in the phenomena of weather, the cloud chamber was used by generations of cosmic-ray physicists and then, briefly, by accelerator physicists; in their hands it “gave concrete meaning to the many newly discovered particles that inaugurated high-energy physics” (Galison and Assmus 1989, pp. 225-226). The ‘vacuum’ of Boyle’s air pump and the ‘fog’ of Wilson’s chamber were new, out of the ordinary *media*, in which new and unusual phenomena were created, observed and, eventually, measured.

The obvious, ‘simplest’ function of a solvent is that of dissolving other substances. Sometimes this plain fact may be surprising, such as the solvent potency of DMF towards acetylene. The history of electrochemistry even demonstrates that the switch from water to nonaqueous solvents completely changed the observed electrolytic phenomena. Thus, nonaqueous solvents supplied new and unusual *phenomenal media*, with an epistemic function completely similar to that of the contemporary Wilson’s chamber. The theoretical impact of *this* development of electrochemistry is shown by the first part of Lewis’ article in which he proposed his famous ‘cubic atoms’ (Lewis 1916). The starting point of Lewis’ considerations was an analysis of the effect of the interaction solvent-solute on the solute’s molecular

constitution, and of the significance of the solvent's dielectric constant for this kind of interaction.

Many uses of solvents are technological as in paints, printing inks, dry cleaning, etc. (Stoye 1993); in the present paper I neglect these application, in order to concentrate our attention on other *instrumental activities* of solvents. (a) A fundamental use of certain solvents is to permit reactions between other substances that – under different conditions – would not be able to react, or would react in a different way. Thus, in many cases, solvents provide *reaction media*, tailored on chemist's synthetic needs: on a single laboratory bench, in otherwise equal vessels, different solvents furnish distinct environments in which the same substances have unlike interactions. (b) The use of solvents in recrystallization is a classical method of the purification of substances. The substance to be purified as well as the impurities are dissolved on heating; the pure substance crystallizes out on cooling the solution, whereas the impurities remain dissolved. Here the same system behaves differently at two different temperatures; *distinct states* of the same medium cause unlike behavior of the concerned substances. (c) For the separation of mixtures of substances, extraction processes are employed, utilizing the unequal distribution of dissolved substances in two solvent phases. The separation may be intended for the removal of impurities or for the concentration of a delicate compound (*e.g.* penicillin with methylisobutylketone) (Stoye 1993, p. 464). In this process *distinct contiguous media* control a molecular distribution without the help of any demon of Maxwellian kind. In section 4 we will see a bright application of this process by Girard and Sandulescu (for the isolation of sexual hormones).

In conclusion, the instrumental function of solvents is 'environmental': to modify the properties of the media in and by which other chemicals – in molecular, colloidal, or dispersed states – are involved in chemical operations of various kind. In the next section we will consider some aspects of measurement in chemistry.

3. Indicators

The history of measurement practices in chemistry is long and varied, and from the 1760s onward it frequently became the measurement of volumes and weights of substances obtained by analysis and synthesis. The measures received an even greater interest because of the experimental work of great researchers as Scheele and Priestely, and assumed various theoretical significance for Lavoisier (*e.g.* the conservation of mass during the chemical reactions) and for Cavendish (*e.g.* the equivalents of reacting substances).

Decades after decades, an increasing number of physical constants were measured both for practical and theoretical aims, as the determination of equivalent weights, the purification and characterization of unknown substances, the recognition of old ones. After the 1810s the 'catalogue' of routine measurements made in many chemical laboratories was fairly long: if the determination of the quantitative elementary composition was the most important, the composition was followed also by other more or less relevant data: melting and boiling points; densities of solids, liquids, vapors and gases; solubility in different solvents at different temperature; crystallographic measures, etc. In general all these practices essentially aimed at the identification of a substance, and of adding to its knowledge a more or less standard set of properties. In this respect Kuhn's dictum is appropriate: "If measurement ever leads to discovery or to confirmation, it does not do so in the most usual of its application." (Kuhn 1977, p. 192) In the context of the present paper, I leave out the epistemological analysis of these kinds of measurement, which I have mentioned only to remind the scope of the quantitative side of classical chemistry. Here I prefer to recall a few points about the history of indicators because these chemicals have been employed, for centuries, almost exactly as thermometers have been used for the measurement of temperature.

In the 1630s, the Italian physician Angelo Sala used the color changes of vegetal substances as means of recognition of acids (Walden 1952, p. 13). Three decades later Boyle described the curious color changes of the syrup of violets, a blue substance which turned green with alkalis and red with acids. In his *Experimental History of Colours* (1664) he employed this property of the syrup for giving an operational definition of acids: an acid was a substance which turned syrup of violets red (Brock 1992, p. 62). Boyle also employed dry test papers on which drops of the solution were placed – the first 'spot-test'; the encyclopedic Partington quotes the names of more than twenty natural dye matters used by Boyle as indicators (Partington 1961, p. 534). The origin of Boyle's application of the vegetable dyes in chemical experiments may be traced to the widespread use by dyers and painters of the same or similar vegetable dyes, whose 'natural' color was altered by acids or alkalis, and applied in the new form. Brock quotes several cases, as the manipulation of the purple juice of the iris, which was combined with alum and used in manuscript illumination as the 'iris green'; Boyle's already mentioned utilization of small strips of paper saturated with litmus may have been borrowed from a technique developed by painters (Brock 1992, p. 178). The practical origin of vegetable 'indicators' emphasizes the scientific relevance of Boyle's attitude: the artisan looked for and applied the new color *as a substance*, to obtain cloth or paper of that particular color; the chemist looked at the appearance of the new color *as a sign*, pointing at the mysterious properties of acids and alkalis. It is also important to remark that

Boyle used the indicators for quantitative measurement, since neutral points could be determined.

The quality control of several industrial chemicals such as soda and bleaching powder induced Gay-Lussac to make a serious attempt of introducing volumetric analysis in the chemical laboratory practice. The French chemist, among many other activities, was an untiring inventor of instruments and implements for scientific use (Partington 1964, p. 82), and made important contributions to the apparatus of volumetric analysis, particularly by his burette. However Gay-Lussac's chemistry was flatly experimental, unresponsive to theoretical discussion (Bensaude-Vincent and Stengers 1995, p. 142), thus his standard solutions for volumetric analyses were not based on the use of equivalents, but on metric measurements of quantities of gases, liquids and solids; because of this severe limit the technique was of very limited usefulness. He employed several methods for indicating the end point of the studied reaction, for example litmus in acid-base titration and indigo in redox reactions (Crosland 1978). Standard or 'normal' solutions, based on equivalent weights, were introduced by Alexander Ure; thus, after 1840s, there was no need for other weighings, and this kind of measurement became truly volumetric. In 1855, in his *Lehrbuch der chemisch-analytischen Titrir-methode*, Carl Mohr systematized the application of volumetric analysis to acidimetry, alkalimetry, permanganatometry and iodometry. However Mohr's proposal had limited application because of the lack of suitable indicators (Brock 1992, pp. 183-184). But this obstacle was overcome when organic chemists produced many dyes which could be employed as indicators. For example, phenolphthalein was synthesized by Baeyer in 1871, while he was trying to obtain plant products (Partington 1964, p. 780). In 1885 Ostwald's electrochemical study of diluted solution led to the famous 'dilution law', and through this law to the 'ionic product' for water; however the pH concept was born only after a long time, proposed in 1909 by the biochemist Sørensen (Brock 1992, pp. 383-385).

At the beginning of this section, I suggested a parallel between indicators and thermometers. In my opinion, the comparison is particularly well suited in the case of the acid-base indicators. They were used for centuries without any knowledge of what they were measuring. In the simplest application they indicated two opposite states (acid, non-acid), whose chemical meaning was defined by certain other properties, but whose chemical causes remained mysterious until the end of the nineteenth century, when several theories were proposed. At last, in the first years of the twentieth century it was made clear that the color of indicators is connected with the hydrogen ion concentration. Also thermometers were used for centuries, before William Thomson could give the thermodynamic definition of temperature in 1848. Notwithstanding the evident difficulties indicators and thermometers were used as

measuring instruments for long periods before their epistemic status were settled.

4. Reagents for identification and isolation

From the 1870s onward, a complex network of chemical knowledge permitted the planning and the realization of the first great syntheses of organic natural substances. Several distinct theoretical threads contributed to the network, and a couple of them needs to be mentioned here, especially the development of the concept of functional group and the growing applicability of the molecular structure theory. Both processes were exceptionally cumulative because they positively interacted with each other, and were propelled by a strong interaction with the experimental work. On the one hand, every fragment of experimental knowledge could be interpreted (at least tentatively) in the light of the functional group/molecular structure scheme; enriched by this interpretation the single 'bit' of information contributed to the explosive growth of the preparative organic chemistry. On the other hand, the theories of organic chemistry were constantly invigorated by the flow of results and problems coming from the laboratory bench. The consistency of this knowledge network was fully demonstrated at the beginning of the 1880s by Baeyer's indigo synthesis, and confirmed in the 1890s by the industrial success of Heumann's indigo synthesis (Cerruti 1989, Antoniotti *et al.* 1989).

The close, theoretical and practical connection between functional groups and organic synthesis is already present in Berthelot's manifesto on the *Chimie organique fondée sur la synthèse*, where he founded his classification of organic compounds on a limited number of *fonctions chimiques*, which characterized a substance as alcohol, aldehyde, acid, etc. (Berthelot 1860, pp. lxxxi-cxxxi). Later, and not without difficulties also for a 'structuralist' as Kekulé (Partington p. 526), the organic chemists working on synthesis described (and constructed) the molecular structures in terms of functional groups (hydroxyl, carbonyl, carboxyl, etc.). It is beyond the aims of the present paper to give a complete epistemological study of the constructive use of chemicals as instruments for the building of molecular structures (*vide infra*, section 6). Here I prefer to follow two lateral, but important, paths, that of the functional group identification and that of the separation of a compound from similar substances. In many cases, as in the following one, the two paths blend with one another.

Emil Fischer prepared for the first time phenylhydrazine, $C_6H_5 NH NH_2$, in 1875. Later, between 1878 and 1884, he returned several

times to the study of this compound, because he was not certain about the structure (on which initially he was wrong; Partington 1964, p. 823). Meanwhile he had begun to study the constitution of the many sugars which had been isolated from vegetal and animal sources, but the first important results were obtained only in 1884, when he was able to use a new reagent, useful for the recognition and isolation of sugars. In fact he discovered that his phenylhydrazine reacted with aldehydes and ketones to form new compounds (hydrazones), and also that, with an excess of the same reagent, sugars turned into 'dream' substances, *i.e.* yellow crystalline solids, sparingly soluble, and with definite melting points. Fischer named a member of the new class of compounds *Phenylglukosazon*, and in the same page of this 'baptism' he stressed that the formation of osazones and the precipitation of the colored material happened also in much diluted solution. Thus, these reactions could also be used as a qualitative test to distinguish between the sugars according to the different resultant osazones (Fischer 1884, p. 580).

We may appreciate the relevance of the new research instrument when we look at Fischer's knowledge situation (in respect to the sugar problem) at the moment of the discovery. A few details of that situation are easily available because in June 1890 the great organic chemist gave the Deutsche Chemische Gesellschaft a lecture on the synthesis of sugars, in which he also told the chemical history of sugars before his own recent successes. The most important point for us is the following: after Kiliani's contributions of 1880, Fischer thought that the structural knowledge of sugars was sufficient for successfully synthesizing them. He tried it, and a certain *Syrup*, which was the outcome of a series of reactions, gave the usual reactions of sugars. Thus Fischer was sure to have been successful, but he was not able to get the final proof of the synthesis: the isolation of the product. As he stated in 1890, by then, in 1880 it was "completely impossible (*ganz unmöglich*) to separate a similar artificial product from a mixture with other organic compounds, and to characterize it as a chemical individual". Many years later Fischer was still feeling the distress of the moment (in 1880) and the following achievement (in 1884): "All efforts to isolate a pure preparation from the raw product remained unsuccessful. It was successful for the first time four years later, after a useful means to this end was found in phenylhydrazine." (Fischer 1890, p. 2177)

Fischer's experience, vexing as it was, stresses three important points of the knowledge procedures of the organic chemist: (a) the synthesis, or, if you like, the experiment ends up only with the isolation of the substance, whose identity as *chemisches Individuum* has to be confirmed with a succeeding, different and independent procedure; (b) the isolation of the synthesized substance is indispensable also when the substance itself manifests its presence by suitable reactions; in a sense it is essential for the chemist to

prepare an 'ostensive definition' of the substance (*vide infra*, section 5); (c) the reagents which are used for the identification and the isolation of the substance in question are fully integrated in the synthesis experiment, which fails if they are not available.

We will stick to the topic of chemicals used for identification, because improving an instrument is a 'natural' consequence of its use in scientific research. The reagents for aldehydes and ketones were improved even after several decades of regular usage in every organic chemistry laboratory.

At the beginning of the 1930s, a conspicuous part of the organic chemistry community was studying two new and remunerative classes of compounds, namely hormones and vitamins. The therapeutic, pharmaceutical and industrial relevance of these compounds was obvious, and the research on the sexual hormones was the epicenter of the involved social and economic interests. In 1933 two researchers, André Girard and Georges Sandulescu, both working in the Laboratoires Françaises de Chimiothérapie, obtained a series of French patents on two new reagents, denominated *réactif T* and *réactif P*. Both compounds were an evolution of the substituted hydrazines proposed by Fischer, as it may be seen by their chemical constitution: $(\text{CH}_3)_3\text{NCl} - \text{CH}_2 - \text{CO} - \text{NH} - \text{NH}_2$ for the reagent T and $(\text{C}_5\text{H}_5\text{N})\text{Cl} - \text{CH}_2 - \text{CO} - \text{NH} - \text{NH}_2$ for the reagent P. In a note published in the *Helvetica Chimica Acta* the two scientists told the story of their research aims and of the successful application of the reagents (Girard and Sandulescu 1936).

The problem, which had been solved by Girard and Sandulescu, was typical of the chemistry of natural substances: separating the substances of interest from others very similar. The particular case faced by the two biochemists was the isolation of ketonic sexual hormones from mare urine concentrates, and the solution of the problem was found in combining two different chemical tools: a specific modification of Fischer's hydrazones and the use of a two liquid phase extraction. In describing their research, Girard and Sandulescu did not conceal the great satisfaction of having reversed the usual train of thought of the application of Fischer's reagents. Instead of looking for less and less soluble compounds, *tout au contraire*, they aimed at reagents which could easily react with the carbonyl group and resulted in compounds which were highly soluble in water, but very scarcely soluble in organic not hydroxylated solvents. A neat difference of stability against acids permitted to distinguish the ketone derivatives from the aldehyde derivatives, in that the products of the first kind were quickly and completely decomposed, whereas the products of the second type were preserved. The 'liberated' ketones could be extracted with an organic solvent, and thus separated from the aldehydes, which remained bound to the reagent.

The patented chemicals (Budavari 1996, monograph n. 4436) were formidable instruments for the separation of ketones from aldehydes, both in

industrial production (*réactif P*) and in laboratory research (*réactif T*). A spectacular outcome of the new reagents was their application to the ‘cortin problem’ by the Swiss organic chemist Tadeus Reichstein, who published his sixth contribution about the adrenal cortex compounds just after Girard’s and Sandulescu’s paper. At the beginning of the article Reichstein warmly thanks Girard, because he “had the kindness (*Freundlichkeit*) not only to permit the use of his reagent to this end [to separate ketones from the mixture], but also to put at disposal his specific experience even before the publication” (Reichstein 1936, p. 1107). What a kindness! Girard’s reagents were precious means of research in Reichstein’s hands. The Swiss chemist *repeated* all the separation process, from the gland extracts to the crystalline compounds, and, as we can read in the title, he was able to add to his noteworthy collection of corticosteroids a compound which he called *Substanz F.a*. The chemical properties of this substance corresponded to those of the Compound E, the pride of Kendall, the American rival of Reichstein in the hunt of cortin. With a single move Reichstein had been able to simplify the tedious process of separation, to confirm the preceding results, and to produce at home the rival’s single substance unknown to himself – until then. It must be mentioned that, many years later, the Compound E became the wonder drug, cortisone (Cerruti 1998).

With the story of Fischer’s and Girard’s reagents, it is finished the short series of examples dedicated to the use of chemicals as instruments. Before the conclusions I should like to face a sensible (at least, for me) problem: to confront the knowledge games played by experimental chemists and physicists.

5. Substance versus Phenomenon

To my knowledge, the opposition between ‘substance’ and ‘phenomenon’ has little philosophical or linguistic tradition. In the philosophical jargon, both words have a long and complex history, which could begin with the Greek philosophy, if we accept *substantia* as the translation of Aristotle’s *ousia*. However, in the current cultured parlance the fate of the two words has been very different. ‘Phenomenon’ irresistibly recalls only ‘noumenon’; in this case time stopped with Kant, and the subsequent deep meaning evolution of the term in Hegel, Husserl and Heidegger has not touched the (naive) supremacy of the lucky couple phenomenon/noumenon. Anyway, as we will see in a moment, the term is flourishing in the more or less philosophical comments on the ‘objects’ of the experimental (instrumental) activity. Much less exciting is the use of ‘substance’. The word has completely lost any old

philosophical significance, and now ‘substance’ is used with much specialized meanings (e.g. in linguistics), or has plainly the lay meaning of ‘chemical substance’. Thus, if I go beyond the noisy incongruence of summoning together a Latin and a Greek root, and thus – unusually – ‘substance’ is set against ‘phenomenon’, it is because I intend to prospect the possible opposition analyzing the descriptions of physical and chemical experimental practices.

It is easy to confirm the fact that ‘phenomenon’ is a successful, perhaps dominant, word in the epistemological turns of historiographical and philosophical discourses. I have collected some quotations, and, as a proposed language game, and in order to remark the ‘epistemological distance’ between chemistry and physics, I have occasionally inserted the forms ‘*substance(s)’ into the sentences. In linguistics the asterisk denotes an *ungrammatical*, or *unacceptable* expression (Lyons 1979, p. 506). I leave up to the reader the game task of replacing the words and adapting the rest of the sentences to the usual semantic content of ‘substance’ (and related forms) in the chemical discourse. Here are the quotations:

(1) “the telescope and the microscope [...] revealed hitherto unsuspected phenomena (*substances) and structures”; (1’) “there was no reason why the phenomena (*substances) recreated in the laboratory with models (*substances) should be the same as the natural ones.” (Hackmann 1989, p. 40)

(2) “[P]henomena (*substances) are realized in and by instrumental (*substance) manipulation”; (2’) “Barlow’s magnetic compass and Wilson’s cloud chamber had no self-evident character: their users had to work hard to fix what that meaning might be, and this fixity was revised by others. This activity is prior to, and enables, experimental efforts to model, imitate and measure phenomena (*substances).” (Gooding *et al.* 1989, p. 2)

(3) “Building new instruments (*substances) opens up new phenomenal (*substance) corners of the world.” (3’) “There can be little doubt that more phenomena (*substances) are available for our appreciation, study and control than were available in the past. Here then is one account of cumulative progress in science.” (Baird and Faust 1990, p. 170)

(4) “Experimental facts are produced as *meaningful* facts about the world of natural phenomena (*substances)”. (4’) “The [...] element, which endows experimental findings with meaning and significance, is a phenomenal (*substance) model, a conceptual understanding of whatever aspect of the phenomenal (*substance) world is under investigation”. (4’’) “[P]henomenal (*substance) models are both an input to experimental practice – the experiment is designed with a phenomenal (*substance) model or models in mind – and more closely specified, an output.” (Pickering 1989, p. 277)

(5) “[M]ost of the phenomena (*substances) of modern physics are manufactured.” (5’) “[T]he phenomena (*substances) of physics [...] are the keys that unlock the universe. People made the keys – and perhaps the locks in which they turn.” (5’’) “Talk about creating phenomena (*substances) is perhaps

made most powerful when the phenomenon (*substance) precedes any articulated theory, but that is not necessary. Many phenomena (*substances) are created after theory.” (Hacking 1990, pp. 228-229)

The preceding twelve excerpts contain about three hundred words. A detailed analysis of the quotations would be very long and perhaps boring, however several results might be quickly verified by the willing reader. Except only few cases (in 1, 2' and perhaps in 5", second occurrence) 'substance' and analogous forms may always substitute 'phenomenon' and its derivatives. For example, with the due semantic substitution we can read: (ex 4") "Substance models are both an input to experimental practice and more closely specified, an output"; and: (ex 5) "Most of the substances of modern chemistry are manufactured". In other cases, 'substance(s)' may substitute '(physical) models', 'instrumental', and 'instruments'. In three cases (1', 2, 3) we may realize a double substitution with a single word, where in the discourse on physical experiments we read two different words. For example (2) sounds: "Phenomena are realized in and by instrumental manipulation"; after the substitutions we have (ex 2): "Substances are realized in and by substance manipulation." The English fluency of (ex 2) is dubious, but not its meaning.

Two results of this language game are interesting. The first one supports the thesis of this paper, namely that in the epistemological discourse chemical substances may be treated as instruments. The second result is more general, and independent by the first, although obtained while we were looking for it. *Phenomena* are generally accepted, and philosophically discussed, as the aims and the results of experiments in physics; in general, *substances* are the aims and the results of the most important experimental practices in chemistry. The epistemic difference of the two kinds of results is tremendous, as we may appreciate referring to the possible use of *ostensive definition* (Pickering 1989, p. 276, and references therein)

If Russell were being right on 'object words' and 'dictionary words', our new game would end up in a single move. The name 'sucrose' could be learnt ostensibly, *i.e.* by pointing to a heap of the chemical substance; while by no means it could be possible to point to anything and to say: "Here is the particle named J/psi." Thus 'sucrose' would be an object word and 'J/psi' would be introduced as a dictionary word, after a long theoretical explanation, the exhibition of many diagrams and the discussion of countless technicalities. Unfortunately, it is easy to demonstrate that also the identification of a macroscopic object (for example, a piece of furniture) requires a sophisticated understanding (Palmer 1983, pp. 22-23). As Wittgenstein advises in his *Philosophical Investigations* (§ 33): "you must already be master of a language in order to understand an ostensive definition." But Wittgenstein gives also an important clue for the solution of our ostensive problem; if "human beings [...] agree in the *language* they use", then "That is

not agreement in opinions but in form of life” (*ibid.*, § 241, italics in the text). Following this thread we can modify our ‘ostensive definition’; the chemist again points to the heap of sucrose, but now he gives his interlocutor (say, a biochemist) a spoon of the substance and some rules of appropriate use (say, it may be put in the coffee or used in a study on fermentation). In this case the two chemists participate in the same form of life (chemical research), in which samples of substances are frequently transferred from a laboratory to another. In the case of the high-energy physicists, the game rules are very different. In November 1974 the Italian physicists working at Frascati with the ADONE machine looked carefully at the decay of a J/psi of 3.1 GeV (Sutton 1985, pp. 85-100). They shared the same form of life with their American colleagues, but in order to study the new heavy meson they had to ‘produce’ the particle at home, because neither the scientists working with SPEAR at Stanford, nor the other discoverers, at the Brookhaven Laboratory, could have sent to Frascati a spoon of ‘phenomena’ called J/psi. Of course the impossibility of an ostensive handing over of high energy particles is not a disproof of their reality; Ronald Giere has demonstrated by a very good analysis how the reality of a high energy electron beam is constructed, and, for safety reason, highly respected, by physicists and technicians working at the cyclotron of the University of Indiana (Giere 1988).

“Any definition can be misunderstood” (Wittgenstein 1986, p. 14), moreover “every model is, by definition, incomplete” (Hoffmann *et al.* 1997, p. 22). Thus, the different knowledge games played by experimental chemists and physicists may not be reduced to a simple set of rules. The two activities are so much different from one another for many reasons, including the fact that chemistry and physics have unlike knowledge aims, and almost invariably work on different ontological levels. My candid metaphor of the spoon intends pointing to distinct regional ontologies.

6. Conclusions and perspectives

To consider chemical substances in connection with scientific instruments is not a novelty. An important example is found in the *Geschichte der organischen Chemie* by Paul Walden. During a discussion of the growth of the synthetic organic chemistry, Walden debates the received view of the instruments’ function in the history of the “experimental sciences”. According to this view the development of the physical sciences has been characterized by an “expansion of our senses”, and “the numerous physical apparatus (*physikalischen Apparate*) certainly represent such expanded, refined

‘organs’; thus, by the same token, “the old and new mechanical tools (*mechanischen Werkzeuge*) are well directed ‘artificial organs’”. According to Walden, if it is applied to chemistry this scheme has to be extended, because “the hundred thousands ‘new bodies’, artificially prepared by experimental chemistry, are for the most part unnatural structures (*naturfremde Gebilde*)”, and thus are not an “expansion of our senses” but an “expansion of nature” (Walden 1941, p. 30, emphasis in the text). Walden creates a context of discussion about the function of instruments in the progress of science, puts in that same context physical apparatus, mechanical tools and synthetic chemical bodies, and distinguishes the first two kinds of instruments (*Apparate, Werkzeuge*) from the third one (*Gebilde*). It is clear that Walden’s rhetorical intentions are different from mine, but – in my opinion – his epistemological analysis is not divergent from mine.

In any case, in the present research I was not looking for novelty (see in fact Schummer 1997, p. 85). My desire was to play a language game, which could give a better understanding of the equation ‘chemicals as instruments’. The ‘results’ of the game may be stated in the order of the preceding sections: the multitude of terms for ‘instrument’ is so multifarious that uniting ‘chemicals’ with the crowd is not preposterous; important classes of chemicals such as solvents and indicators have an epistemic function very similar to classical instruments, such as the air pump or the thermometer; at last, the use of specific reagents for chemical operations such as separation and identification points to the crucial ontological level where chemicals function as instruments: the microscopic, molecular level. The ‘opposition’ between ‘substance’ and ‘phenomenon’, and so between chemistry and physics, is simply a bonus at the end of the game.

If the likening ‘chemicals as instruments’ will not remain of my private use, I intend to follow two directions of enquire. The preparative organic chemistry embraces an enormous number of reactions (including many ‘name reactions’). The *birth* of this classical part of the bench chemistry waits for an epistemological analysis, which could examine how and why organic chemists became able to retouch the molecular maps almost at will. A second prospective is opened, and day by day enlarged, by supramolecular chemistry. In this field substances (molecules) have been dubbed with the name of tools: this explicit language demonstrates that chemists are molecular mechanics.

References

- Antoniotti, P.; Cerruti, L.; Rei, M.: 1989, 'I chimici italiani nel contesto europeo, 1870-1900', in: V. Ancarani (ed.), *La scienza accademica nell'Italia post-unitaria*, Angeli, Milano, pp. 113-190.
- Baird, D.; Faust, Th.: 1990, 'Scientific Instruments, Scientific Progress and the Cyclotron', *The British Journal for the Philosophy of Science*, **41**, 147-175.
- Bennett, J.A.: 1989, 'A viol of water or a wedge of glass', in: Gooding *et al.*, pp. 105-114.
- Bensaude-Vincent, B.; Stengers, I.: 1995, *Histoire de la chimie*, La Découverte, Paris.
- Berthelot, M.: 1860, *Chimie organique fondée sur la synthèse*, Mallet-Bachelier, Paris.
- Bridgman, P.W.: 1927, *The Logic of Modern Physics*, Macmillan, New York, chapt. 3.
- Brock, W.H.: 1992, *The Fontana History of Chemistry*, Fontana, London.
- Budavari, S. (ed.): 1996, *The Merck Index. An Encyclopedia of Chemicals, Drugs, and Biologicals*, Merck & Co., Whitehouse Station.
- Cerruti, L.: 1989, 'Chimica e cultura chimica ai tempi di A. Sobrero. Un caso esemplare: sintesi e struttura dell'indaco', *Atti del Convegno in celebrazione del centenario della morte di Ascanio Sobrero*, Accademia delle Scienze, Torino, pp. 39-71.
- Cerruti, L.: 1998, 'La difficile ricerca della 'cortina'. Struttura chimica e attività fisiologica, 1928-1954', *Atti del VII Convegno Nazionale di Storia e Fondamenti della Chimica*, in press.
- Crosland, M.: 1978, *Gay-Lussac. Scientist and bourgeois*, Cambridge UP, Cambridge, chapt. 9.
- Eberling, C.L.: 1980, 'Dimethylformamide', in: Kirk-Othmer, *Encyclopedia of Chemical Technology*, vol. 11, Wiley, New York, pp. 263-268.
- Fischer, E.: 1884, 'Verbindungen des Phenylhydrazins mit den Zuckerarten', *Berichte der Deutschen Chemischen Gesellschaft*, **17**, 579-584.
- Fischer, E.: 1890, 'Synthesen in der Zuckergruppe', *Berichte der Deutschen Chemischen Gesellschaft*, **23**, 2114-2141.
- Franklin, A.: 1989, 'The epistemology of experiment', in: Gooding *et al.*, pp. 437-460.
- Fuller, S.: 1989, *Philosophy of Science and Its Discontents*, Westview, Boulder.
- Galison, P.; Asmus, A.: 1989, 'Artificial clouds, real particles', in: Gooding *et al.*, pp. 225-274.
- Geckeler, H.: 1971, *Strukturelle Semantik und Wortfeldtheorie*, Fink, München.
- Giere, R.N.: 1988, *Explaining Science. A Cognitive Approach*, Chicago UP, Chicago.
- Girard, A.; Sandulescu, G.: 1936, 'Sur une nouvelle série des réactifs du groupe carbonyle, leur utilisation à l'extraction des substances cétoniques et à la caractérisation microchimique des aldéhydes et cétones', *Helvetica Chimica Acta*, **19**, 1095-1107.
- Giua, M.: 1962, *Trattato di Chimica Industriale*, vol. 6, t. ii, UTET, Torino, chapt. 2.
- Gooding, D.; Pinch, T.; Schaffer, S. (eds.): 1989, *The uses of experiment. Studies in the natural sciences*, Cambridge UP, Cambridge.
- Hacking, I.: 1990, *Representing and intervening. Introductory topics in the philosophy of natural science*, Cambridge UP, Cambridge (1st edn. 1983).
- Hackmann, W.D.: 1989, 'Scientific instruments: models of brass and aids to discovery', in Gooding *et al.*, pp. 31-65.
- Hoffmann, R.; Minkin, V.I.; Carpenter, B.K.: 'Ockham's Razor and Chemistry', *HYLE*, **3**, 3-28.
- Krige, J.: 1989, 'Why did Britain join CERN?', in: Gooding *et al.*, pp. 385-406.
- Kuhn, Th.S.: 1977, *The Essential Tension. Selected Studies in Scientific Tradition and Change*, University of Chicago Press, Chicago.

- Lewis, G.N.: 1916, 'The Atom and the Molecule', *Journal of the American Chemical Society*, **38**, 762-785.
- Lyons, J.: 1979, *Introduction to Theoretical Linguistics*, Cambridge UP, Cambridge.
- Palmer, F.R.: 1983, *Semantics*, Cambridge UP, Cambridge.
- Partington, J.R.: 1961, *A History of Chemistry*, vol. 2, Macmillan, London.
- Partington, J.R.: 1964, *A History of Chemistry*, vol. 4, Macmillan, London.
- Pickering, A.: 1989, 'Living in the material world: on realism and experimental practice', in: Gooding *et al.*, pp. 31-65.
- Pizey, J.S.: 1974, *Synthetic Reagents*, vol. 1, Horwood, Chichester, chapt. 1.
- Polanyi, M.: 1983, *Personal Knowledge*, Routledge, London (1st edn. 1958).
- Rambaldi, G.; Cerruti, L.: 1998, 'Cannizzaro's Laboratory Life. The Troubled Making of a New Experimental Tradition', in press.
- Reichstein, T.: 1936, 'Über Bestandteile der Nebennieren-Rinde. VI. Trennungsmethoden, sowie Isolierung der Substanzen F.a. H und J', *Helvetica Chimica Acta*, **19**, 1107-1126.
- Reuben, B.G.; Burstall, M.L.: 1973, *The chemical economy. A guide to the technology and economics of the chemical industry*, Longman, London.
- Riddick, J.A.; Toops, E.E.: 1955, *Organic Solvents. Physical Properties and Methods of Purification*, Interscience, New York.
- Rothbart, D.; Scherer, I.: 1997, 'Kant's *Critique of Judgment* and the Scientific Investigation of Matter', *HYLE*, **3**, 65-80.
- Schummer, J.: 'Challenging Standard Distinctions between Science and Technology: The Case of Preparative Chemistry', *HYLE*, **3**, 81-94.
- Smith, R.W.; Tatarewicz, J.N.: 1994, 'Counting on Invention: Devices and Black Boxes in Very Big Science', *Osiris*, **9**, 101-123.
- Stoye, D.: 'Solvents', *Ullmann's Encyclopedia of Industrial Chemistry*, vol. A 24, VCH, Weinheim, pp. 437-505.
- Sutton, C. (ed.): 1985, *Building the Universe*, Blackwell, Oxford.
- Van Helden, A., Hankins T.L.: 1994, 'Introduction: Instruments in the History of Science', *Osiris*, **9**, 1-6.
- Walden, P.: 1941, *Geschichte der organischen Chemie seit 1880*, Springer, Berlin.
- Walden, P.: 1952, *Chronologische Übersichtstabellen zur Geschichte der Chemie von den ältesten Zeiten bis zur Gegenwart*, Springer, Berlin.
- Wittgenstein, L.: 1986, *Philosophical Investigations*, Blackwell, Oxford; tr. by G.E.M. Anscombe.

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