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Children, Insanity and Child Psychiatry c.1800 - 1935

Zbigniew Kotowicz
(CFCUL)
z.kotowicz@yahoo.fr

PART 1 – The 19th century

Psychiatry emerged, all historians agree on this, at the end of the 18th century and the beginning of 19th. First reports of mad children also go back to this time (Crichton 1798, Haslam 1809, Esquirol 1838). However, Leo Kanner's *Child Psychiatry*, incontestably the first child psychiatry textbook proper, appeared only in 1935. The question therefore arises: Why did it take so long (almost a hundred and fifty years)? What happened to mentally afflicted children during that period? Those who have taken a historical look at the 19th century are not in agreement. Kanner himself, who had a good grasp of history, goes so far as to state that there was nothing in that period that could be called child psychiatry (Kanner 1935), but historians paint a somewhat different picture (Neve & Turner 2002, Parry-Jones 1989, Wardle 1991, von Gontard 1988, Stone 1973) and one explicitly rejects Kanner's view (Harms 1962).

In this article, von Gontard's paper 'The Development of Child Psychiatry in 19th Century Britain' will stand out as it is one of the earlier studies, it is very thoroughly researched and, most of all, it was the first study on the subject that this author read; it set the tone for subsequent thought and research, first questions on the subject were formulated after reading von Gontard's article. Here are some of the statements that we find there:

a. 'Many authors have shown that, for most European countries, the origins of child psychiatry can be traced back to the 19th century' (Gontard 1988: 569);

b. 'The diagnosis of childhood insanity was greatly facilitated by the introduction of the term 'moral insanity' in 1835 by James Cowles Prichard (1786-1848). It was to become the most common diagnosis for children' (*ibid.*: 573);

c. 'most children were diagnosed as morally insane, with only exceptional cases of true intellectual insanities: Sir Alexander Morison (1779-1866), one of the acclaimed authorities during the first half of the 19th century, described a 14-yr-old boy with mania (Morison, 1828) and an even rarer case of mania in a 6-yr-old, whose "conduct was violent and mischievous, with incoherence of speech" (Morison 1848)' (*ibid.*: 574);

d. 'Despite its popularity, moral insanity was only one of seven distinct types of childhood insanity in Maudsley's original nosographical system (Maudsley, 1867). The other forms were (1) monomania, (2) choreic delirium, (3) cataleptoid insanity, (4) epileptic insanity, (5) mania and (6) melancholia' (*ibid.*: 580);

e. 'Finally, children began to be admitted to specialised asylums' but 'Despite this process [the massive increase of number of inmates in hospitals in the 19th century], only a few children were actually admitted to the hospitals […] many – especially younger children – must have been seen as outpatients. One can therefore safely conclude that, with the expansion of the institutions and the increase of population, a slowly increasing number of children were seen or hospitalised in asylums during the 19th century' (*ibid.*: 573);

f. '… by 1900, all elements which later merged into child psychiatry had evolved – with the exception only of psychoanalysis and child guidance' (*ibid.*: 570).

Von Gontard's paper is meticulously researched and, no doubt, he would have also included a study on children in Bethlem Hospital in the 19th century had it been carried out before his time. In 1987 Robert Wilkins published his findings based on research in the archives of the hospital. 1067 children and adolescents were admitted in the 19th century into Bethlem. This figure is impressive and it is often quoted in historical accounts. One historian wrote about Wilkins's study: 'Over one thousand children and adolescents were admitted to Bethlem Royal Hospital between 1815 and 1899, and Wilkins has shown that there is remarkable scope for research on such material' (Parry-Jones 86).

Since the insane children were a rarity in asylums, the Bethlem admission figures are strikingly large. It comes, therefore, as a surprise to see that, although this famous institution has been studied thoroughly by historians (O'Donoghue 1914, Masters 1997, Russell 1997, Allderidge 1997, Andrews *et al.* 1997), and although it was unusual in that it often admitted children, the questions that this might pose have slipped through the historians' net. First: what kind of behaviour led these children to be certified insane? What were their backgrounds? How did they fare in the hospital? Second: was Bethlem altogether unique? Or, does the high number of children that it admitted suggest that London had a higher incidence of child insanity than the rest of the country?

Bethlem in the 19th century

The 19th century is particularly interesting in the history of this institution. At the beginning of the century, its reputation was dreadful. In 1815 The House of Commons Select Committee on Madhouses savaged the hospital's practices (Andrews *et al*. 1997). In the same year, Bethlem moved from the dilapidated Moorgate site to Southwark. Conditions in the new location were much improved but the hospital's bad reputation continued. Full change came with the I852 appointment of the first Resident Chief Physician William Charles Hood, which saw the end of the over a century old Monro dynasty. The hospital adopted the 'moral treatment' regime. Various forms of occupational therapy were introduced; a library was set up as well as exercise yards. The hospital was no longer completely isolated from the outside world, either; some patients were allowed to go alone for walks, staff organised group outings, and there were the famous monthly balls to which members of the public were invited. In time the hospital shed its grim reputation and became a far more preferable place to stay in than a County Asylum, so much so, that in 1886 the hospital started admitting 'voluntary boarders', that is, people seeking treatment who had not been certified insane (Gale and Howard 2003: 6). One of the hospital's attractions was that it remained a relatively small institution. While throughout the 19th century hospitals tended to grow in size, Bethlem's population remained stable at around two hundred and fifty inmates (Colney Hatch in North London ended up housing some 2 500 patients). Another attraction was that unlike county asylums, which were isolated on the outskirts of the city, Bethlem was relatively close to the centre of London.

Because of its strict admissions policy, Bethlem had a somewhat unusual population. Those who had been insane for more than a year were not accepted (unless they had been already treated in Bethlem); those suffering from chronic conditions such as general paresis or epilepsy, or patients discharged uncured from other asylums were not accepted either (Thurnam 1845). The maximum stay in the hospital was twelve months. This was sometimes extended, if recovery seemed imminent, although there were some striking exceptions, for example, one seventeen year old boy admitted in 1850 remained in the hospital for four years and three months after which time he was discharged as uncured (CB/49) From the middle of the century, the hospital did not admit pauper lunatics. The consequence of this policy was a higher than average recovery rate. (This caused resentment on the part of county asylum psychiatrists who could not so freely refuse admission. Lockhart Robertson 1865.)

Bethlem's admissions policy was not unique, as the same policy was followed in St Luke's. But children were not accepted at St Luke's (Thurnam 1845), while Bethlem had no age limit. However, there seems to be no evidence that there was an explicitly stated rationale for this policy. No special provisions for children were made and when in the 1880s diagnosis was introduced on the admission forms, children were given the same type of diagnoses as adults. Also, the same admissions criteria applied irrespective of age, which meant that the children admitted to the hospital were very unlikely to have been congenital idiots or epileptic, as was often found in county asylums. This makes the Bethlem cohort a unique sample, both in terms of size and relative homogeneity.

How did children fit into the pattern of the hospital? In which wards were they kept?[1] What types of treatment did they receive? How did their recovery rates compare with the overall population of the hospital? Was there indeed no 'specialist' approach to children, of any kind?

[1] The hospital had eight wards (which were called in the nineteenth century galleries), four for women, four for men. From the early 1880s onwards the lowest galleries on the male and female side (M1 and F1) were divided into section A and B. A good idea of what kind of patients were kept in different galleries can be found in this witty poem written by a patient who obviously harboured a deep dislike for the physician in charge George Savage.

Dear Doctor, when the trumpet sounds
And God proclaims the judgment day
You'll try I know to be at least
Some fifty miles or more away
'Twill be no use, no tree no bush
Will hide you from God's searching eye
With other Savages you'll have
To toddle up your luck to try
You'll go to heaven I believe
But not to galleries II or III
With fifth-rate angels you will be
In lower gallery in M.I.B.
You'll daily scrub the gallery floors
And cleanse your brother angels' sores
You'll smooth their wings and comb their tails
And empty too their slops in pails
In course of years some three or more
You may be raised to gallery IV.
(in Gale and Howard 2003: 23)

Wilkins's study

Wilkins's intention was to determine whether, as the century progressed, there was an increase in hallucinations in the child and teenage population of the hospital. This was carried out with the intention of testing Hare's hypothesis that the dramatic increase in the hospital population in the 19th century was due to a real increase in the incidence of insanity (with a possible viral aetiology) (Hare 1983), rather than a consequence of changes in the society and the widening of the psychiatrists' net, as one historian has argued in response to Hare's hypothesis (Scull 1989). To this effect, Wilkins looked to see whether there was any increase in hallucinations in the young population of the hospital. Hallucinations were chosen as they are one of the 'rank symptoms' of Kurt Schneider; that is, symptoms which suggest a likelihood of the onset of schizophrenia (Schneider 1959).

For the purposes of this research it was essential to return to the archives and to conceptualise the material differently. First of all, Wilkins separates the patients into two groups, the first, aged under 17 and the second, from 17 to 19, in order to make the younger group comparable to patients seen in Child Guidance Clinics (however, Wilkins does not carry out any such comparable study). The under 17 category numbered 235, the youngest being six years old. However, most of the results pertaining to the incidence of hallucinations are given with respect to the entire under 20 years old group. In this study the principal age bracket of the children will be under 15. There are two reasons for this. First, 14 or 15 is the age most frequently mentioned by psychiatrists as the age at which the onset of madness becomes more common; second, statistics of the Reports of the Commissioners in Lunacy divide the groups by five-yearly intervals (ten-yearly in the adult population); keeping the same division in this study will make possible comparisons with nationwide statistics.

Secondly, Wilkins's decision to seek the incidence of 'first rank' symptoms tells us little about the children from the younger group, as few of them exhibited these symptoms; most were found in the 17 to 19 age group. (The same applies to Wilkins's subsequent study, in which the same procedures were followed with respect to the incidence of delusions. Wilkins 1993.) Therefore, concentrating on reported symptoms will not be a suitable method. Because of the lowering of the age to under 15, the number of cases will be significantly lower than the 235 of Wilkins's group and this should allow for an in-depth descriptive analysis, based directly on the patients' notes.

Patients' notes

The red buckram bound ledgers with the patients' notes are held in the Bethlem Royal Hospital Archives in Beckenham. The records begin in 1815, the year the hospital relocated from Moorgate to Southwark; notes from 1824 to 1829 are missing. In the earlier years, separate volumes for doctors who were responsible for the cases (Monro, Morison) were kept. From 1835, male and female cases were kept in separate volumes. At first, there was no standard form, although the doctors followed similar procedures. The notes gave name, sex, age (but not date of birth), two doctors' certificates, date of admission, previous history, possible cause and/or hereditary factors were taken on admission; then followed notes, written at regular intervals (at least in principle) on the patient's progress in the hospital; the entry ended with the date of discharge plus an added note 'discharged cured', 'well', 'discharged uncured', 'taken by friends', or, in some cases, 'died', accompanied in later years by a death certificate. In 1848, a new standardised format of admission was introduced, which collected the same data (it became standard in other hospitals too). In 1875, the notes were expanded and on admission a description of the patient by some relative was taken. The new form also specifically sought to establish whether the patients suffered from hallucinations; in 1888, five types of hallucinations were distinguished (auditory, visual, taste, smell, touch) and were added to the form.

Terminology

The terminology used in this article reflects the one that was current in the 19th century. In those pre-Kraepelinian days, terms such as 'madness', 'insanity', 'mental alienation' were used interchangeably. Nevertheless, despite a certain semantic chaos and vagueness, clinical descriptions that we find in the psychiatric texts show a degree of consensus – those considered insane we would today describe as psychotic, that is, suffering from delusional and/or hallucinatory states, or as manic-depressive, or as severely depressed. However, there was also some confusion. For decades, much to the dissatisfaction of some (Monro 1856), almost every psychiatrist proposed a new classification of mental disturbances, some very elaborate. But the terminology that the psychiatrists used in their everyday day practice was loose. Bethlem's case notes bear this out. A diagnosis was specifically entered on the case notes only in the 1880s, (but it was entered in the admission form already in 1856); it was practically always mania or melancholia. Earlier, the form would state the time of the 'first

attack', and would only describe the patient as 'insane' (or not). The descriptions could be lax. Monro described one 1822 patient as 'slightly insane' who recovered 'perfectly well'. But, as the century progressed, the notes became increasingly more detailed not only in the contents of the form but also in the descriptions of the patients' behaviour in the hospital, and there is evidence of increased clinical sophistication.[2] The quality of these descriptions varied depending on the doctor in charge, however, in another respect, there was a degree of constancy – every request for admission to hospital was accompanied by letters from two physicians. Therefore, there is a description of the behaviour of every Bethlem child, prior to admission, which in the opinion of two physicians justified hospitalisation. These should be a source of valuable information.

Analysis of patients' notes

The archival research brought up a big surprise. Once the cut off point is made at the age of 15, which is when one can speak of children proper, the number whittles down to a mere 58. Over a half of those (32) were 14 year olds, 17 aged 13 years, two aged 12, the same number of 11 year olds, three aged 10, one nine year old and one aged six. How many of them could be judged insane? The youngest patient poses a problem. Alexander Morison wrote this about the girl:

> Eliza A - ; A little girl, aged six years, was admitted into Bethlem Hospital on the 30th August, 1842, labouring under the attack of Mania of ten weeks duration. The case is remarkable, as presenting well marked features of insanity at so early an age. The cause of the attack was stated to be inflammation of the brain, preceded by convulsions. When admitted into the hospital, her conduct was violent and mischievous, with incoherence of speech; occasionally, however, by strongly arresting her attention, a correct reply could be obtained. The first portrait was taken while in this state; a considerable improvement soon take place in her conduct and behaviour, and she began to pay attention to the directions of one of the patients who took charge of her; but still continued decidedly insane. She was ultimately discharged cured, in about two years time, at which time the second portrait was taken. (Morison 1848: 282-3)

(The text is accompanied by two portraits of the girl, one taken at the time of her admission, the second at the time of her discharge, but there is scarcely any difference between them.) In fact, the 'about two years time', was two years and eight months (from 2.9.1942 to 25.4.1845). It is a mystery why she spent so long in the hospital.

[2] A more detailed analysis of this one can find in Suzuki 1999.

Furthermore, the first date of discharge is 16.12.42, that is three and a half months after admission, but this is followed by a note dated 23.12.42, 'Continues in hospital.' The reasons why the girl remained in the hospital are not given. Not a great deal more is stated in the notes, only that prior to admission to Bethlem the girl was hospitalised in St. Georges Hospital in June of the same year after being 'attacked with convulsions' and that she suffered a similar attack when she was 18 months old (CB/28 p.43). What made her 'decidedly insane' is not clear.

The nine year old boy was epileptic and died a little over a month after admission (CB/56 p.27).

All of the three ten year old patients were girls. The first was admitted in 1836 with her 'attack of insanity' coming after a bout of hooping cough and was discharged 'well' after five and a half months. The second showed signs of insanity (mania) after an attack of pneumonia; she was discharged 'well' after a month and a half (CB/32 p.34). The third was admitted after reports of three 'attacks of rigidity', of which the first lasted half an hour the two subsequent ones ten minutes and which were preceded by a 'flighty and restless state'. There was no appearance of these attacks after she was admitted to hospital and although at times she had a 'slight disposition to hysterical laughing', she was described as 'quite rational' and 'very orderly'. She was discharged, 01.2.1850, 'well' after two and a half months in the hospital (CB/45 p.91).

Of the two eleven year olds, the first, a girl, was admitted on 18.10.1850 and discharged a month and a half later as the physician in charge (Morison) did not see any reason for prolonging her stay in the hospital as 'there has been no evidence of insanity since her admission' (CB/49, p. 79). The other patient was a boy admitted on 23.4.1885, after he suffered a 'shock at a funeral'. His one noticeably odd behaviour was that he walked on the sides of his feet. But after a while in the hospital he did this only 'when noticed', otherwise, 'will play in the racket court and then generally runs about with his feet in almost a normal position'. He was discharged after two and a half months as 'recovered' (CB/126, p.43).

Both twelve year old patients were boys. The first, admitted on 24.4.1846 was described as 'decidedly insane', but was discharged after a year as not fit as he was eventually found to be epileptic (CB/32, p.159). The second, admitted 26.10.49 was diagnosed as suffering from mania and was described as 'wild, violent and incoherent. He was discharged 'well' after eight months in the hospital after no symptoms were observed for some 'two or three months' (CB/46, p. 56)

The number of children aged 13 rises to seventeen. Of those, 14 were discharged as 'recovered', one died after two months in the hospital (CB/42, p.45); one diagnosed as suffering from mania was discharged after 12 months (21.1.1958 – 20.1.1959) as 'not

improved', the last one described as weak-minded and kleptomaniac left the hospital after two months as 'not improved' but nevertheless 'relieved' (CB/127, p.116).

This leaves the 32 patients who were 14 years old. Of those five were released as 'uncured', one was found as suffering from 'imbecility'. All the others left the hospital pronounced well.

Before attempting some final assessment of this material, some general comments have to be made. There is no evidence from the notes of any special provision for children; nothing is said about which wards they were kept on, and, in all, they were treated much the same as adult patients. Another point has to be made. Although, as the century progressed, one finds an increased sophistication of the descriptions of patients' conditions with notes at times running to several pages, descriptions of children remained fragmentary and scant; there is nothing that we could call a proper clinical description and, for example, the statement saying that the patient was 'decidedly insane' is hardly illuminating. In all, because of the lack of clear clinical material it is difficult to say with any certainty how many children suffered from proper psychiatric problems. However, one feature stands out, the frequent relatively short stay in the hospital – 37 of the 58 admissions remained in hospital for less than six months – suggests that these children often suffered transitory troubles and that these troubles were often consequence of a physical injury.[3]

However, the real surprise is not just the small number of children that found their way into the hospital, but it is the realisation that these are numbers out of some twenty thousand admissions in that period. This, Wilkins does not say.

Bethlem and County Asylums

How do the numbers of children in Bethlem compare with the situation in county asylums?

The picture is somewhat complicated by the fact that in the 19th century psychiatrists also had to deal with a great number of idiot children. The distinction between idiocy and insanity was already spelt out by John Locke in the 17th century (Locke 1690); it was often reiterated, and some psychiatrists concentrated on working with idiot children in special asylums (Seguin, Voisin, Ireland). The different challenges that the idiot and insane children posed were clear to most psychiatrists

[3] Of the 60 cases of adult patients presented in *Presumed Curable. An illustrated casebook of Victorian patients in Bethlem hospital* (Gale and Howard 2003), which could be taken as a random sample, only four were discharged within less than 6 months.

(but not all, writing in 1898, Beach mixes these two categories). However, few idiot children were housed in special asylums, the majority were found in workhouses, and what was clear to psychiatrists was not always clear to workhouse masters or Poor Law officials who spoke of 'idiots', 'lunatics' or persons of 'unsound mind' almost interchangeably (Digby 1996). Consequently, some idiot children found their way into county asylums. Research carried out in a few of these institutions confirms this.

In his study of Bethlem, Wilkins also looked at admission registers in Brookwood Hospital in Surrey. He found that of the 28 patients under the age of 20 admitted in 1893, 18 were congenital idiots. Findings from another county asylum (Worcester) show that from a total of 6573 admissions in the years 1854-1900, 195 were under the age of s16; some two thirds of those were suffering from epilepsy and/or idiocy (Gingell 2001). A study of a Devon County Asylum reveals that of the total of 101 children under the age of 15 admitted between 1845 and 1914, again, two thirds were diagnosed idiots or imbeciles (Melling, Adair, Forsythe 1997). This study also gives a detailed picture of the dealings between workhouses, Poor Law officials and asylums. Since Bethlem did not accept pauper lunatics, it was not part of this circuit. Of the cases that came directly from their family homes that Melling *et al*. discuss, an epileptic girl who tumbled into the fire would not be admitted, nor would exceptionally violent children, but a 12 year old melancholic boy who attempted suicide probably would.

Nationwide statistics suggest that the numbers in these hospitals are typical. The 54th Report of the Lunacy Commission shows that in the last five years of the century, out of the total annual average of 18437 hospital admissions only 217 were under the age of 15.

Therefore, national statistics and findings coming from research in County Asylums show that there was nothing exceptional about numbers of young patients in Bethlem hospital. And so the view, expressed earlier, that historians who studied Bethlem 'missed' the problem of children in this institution is not correct.

A final word about Wilkins's study is necessary. Wilkins did not have children as such as subject of his research; his aim was to see whether there was an increase in first rank symptoms in the young population of the hospital. Had he specified that the number of children that he studied came from some 20000 admissions, it would be enough to compare these figures with nationwide statistics to realise that it was nothing exceptional. One has to say there was no obvious need for Wilkins to do so and, as it is, the figure of 1069 children and adolescents admitted to Bethlem in the 19th century has entered historiographical folklore.

Von Gontard

We can now return to some of von Gontard's statements quoted earlier (p.1).

The first thing that strikes about one these statements is that they give the impression of an abundance of mad children treated by psychiatrists ('most common diagnosis for children', 'most children'); this is deeply misleading. It is sufficient to consult 19th century statistics, which the Lunacy Commission regularly published, to see how small was the number of children in hospitals, nothing that gives the impression of abundance. Furthermore, there is no sign of an increase of children in hospitals towards the end of the century as von Gontart states in quote e.; in fact, the national statistics indicate a slight decrease of number children in the last decade of the century (or, to be more precise, the number of children admitted nationally stabilised at around 200 per year, while the overall numbers kept growing); in the same decade, in Bethlem, five children under 15 were admitted (out of the total of 2387 admissions).

Further still, the idea that Alexander Morison was one of the 'acclaimed authorities' (on children, von Gontard's text suggests) during the first half of the 19th century gives the impression that there was already a budding speciality. In fact, the two cases that von Gontard refers to are the only cases of supposed child insanity that Morison mentions.

However, von Gontard based his account principally on the writings of the 19th century practitioners. For example, when he states that 'many – especially younger children – must have been seen as outpatients', he repeats the claim already made in that century. In Bucknill and Tuke's *Manual of Psychological Medicine* we find the following statement:

> [I]t may be observed that no age is exempt from attacks of Insanity. Such attacks, it is true, are comparatively infrequent under fourteen or fifteen years of age. Scattered throughout this work, however, will be found a considerable number of references to cases of Insanity under puberty, and they might have been considerably increased. They are met with in private practice much more frequently than in asylums, the statistics of which, therefore, give too favourable an impression as to the frequency of attacks of Insanity in the young. (Bucknill and Tuke 1879: 74)

However, although towards the end of the 19th century some well-off patients would seek help in Harley Street, there is no evidence of regular outpatient activities. In those times psychiatrists mostly operated from inside the asylums and were dealing with those who were delivered to them by the community (with certificates of insanity from two General Practitioners). The idea of the psychiatrist's involvement in the community, as we understand it today, did not yet exist. And then, when one peruses this voluminous

text in search of the 'considerable number of cases of Insanity under puberty', which the authors refer to, one finds that children are specifically mentioned in only two mental afflictions, kleptomania and pyromania. (And anyone who knows children well could have guessed this. In fact, bearing in mind how often children exhibit fascination with fire, the number of them becoming pyromaniac is, if anything, very small.)

There were also other writings. One could begin with a case first reported in German (by John Ernest Greding) that Crichton translated and included in his 1797 *An Inquiry into the Nature and Origin of Mental Derangement*. This extraordinary case speaks of a child born 'raving mad', who, when he was four days (yes, days!) old possessed so much strength in his legs and arms that four women could, at times, with difficulty restrain him. These paroxysms either ended with an indescribable laughter, for which no evident reason could be observed, or else he tore in anger every thing near him, cloathes [*sic*], linen, bed furniture, and even thread when he could get hold of it. We did not allow him to be alone, otherwise he would get on the benches and tables, and even attempt to climb up the walls. Afterwards, however, when he began to have teeth, he fell into a general wasting, and died. (Crichton 1797: 355-6)

This 'case' is quoted a few times by the 19th century psychiatrists without any sign of incredulity (Crichton Browne, Maudsley). On seeing this, it seems prudent to be a little sceptical about what these psychiatrists had to say.

Crichton Browne's 1863 paper is often quoted. One historian discusses it at length and wonders why it failed to receive any attention and suggests that: 'Generally, the acceptance and implementation of ideas depends on the status of the protagonist, a sympathetic climate of opinion, and the readiness of others to take the idea up with enthusiasm: all these must have been lacking' (Wardle 1991: 284). One would like to think that the intrinsic quality and coherence of the ideas should also play some role because when one reads the article, one cannot be but sceptical about what Crichton Browne has to say. The principal problem of Crichton Browne's exposition is that he is arguing the inherent insanity of children judging them from the point of view of adult psychology. And, really, what is one to make of his insistence that insanity can already occur 'in utero',

> Almost all writers on the subject of psychology are agreed as to the extreme rarity of mental diseases before that period of life [puberty], and I am not aware that any one has ever suggested its occurrence in utero. Unfortunately, however, I shall be enabled to demonstrate that insanity does occur in utero, in infancy, and childhood, and that it is by no means so uncommon as supposed. (Crichton Browne 1860: 286)[4]

[4] It should be specified that Crichton Browne speaks of madness 'in utero' in two distinct ways. First, he says that reasons for madness can already be planted in utero if during the pregnancy

Crichton Browne gave the paper when he was a 19 years old medical student and, in fact, he impresses with the thoroughness of his research, his confidence is prodigious; but this is a piece of juvenilia, which might be of interest to those who want to know the details of Crichton Browne's very long professional career, but it is difficult to see how this could be considered an event in the history of child psychiatry.

As for Maudsley's list of types of childhood insanity that von Gontard mentions, it would be interesting to see how he constructs this list. His approach, one could say, is somewhat idiosyncratic. The chapter in which he presents his views on childhood insanity begins with a speculative exposition of the development of the child's nervous system. Then, Maudsley states 'Suppose now that an infant becomes insane immediately after birth, what sort of insanity must it exhibit?' (Maudsley 1867: 260). This is a strange suggestion. Has anyone seen such a child? Has Maudsley seen one? He does not refer in his text to any observations of such children. But, he goes on to quote the case of the child 'born raving mad' from Crichton, and he finds this case altogether credible, as he found there symptoms that confirmed his views on the development of the child's nervous system. Then, in a colourful description, Maudsley compares such a child to insane animals, which includes an image of an insane elephant!

> There is far more power in an insane elephant than in an insane infant, and it is able to do a great deal more mischief; but there is really no difference in the fundamental nature of the madness; the maddened acts are the reactions of morbid motor centres to impressions made on morbid sensory centres; and the whole mind, whether of the infant or of the animal, is absorbed in the convulsive reaction. (*ibid.*: 263)

Later in the text Maudsley describes the seven types of childhood insanity and, one can only marvel how he managed to describe so many of them, based on personal observations of just two children (and the second was added in the appendix, after the text was written) and just over a dozen of cases that he found in other psychiatric texts. What does, however, come through is Maudsley's manifest dislike of children, who 'like brutes, live in the present' (*ibid.*: 269).

Another interesting example, this time from French literature, is a chapter on child insanity, which we find in Benedict Morel's *Traité des maladies mentales*, published in 1860. Morel does mention, but briefly, two cases that he had dealt with personally, and instead, furnished as proof of existence of childhood insanity the fact

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the mother falls ill or suffers a physical trauma (only that examples that he gives are cases of children with forms of idiocy); second, Crichton Browne insists that insanity can already exist in this state. While the first formulation can be accepted, the second leaves one with raised eyebrows.

that there are known instances of children falling prey to a psychic epidemic, of which he gave three examples. The first, the infamous so-called children's crusade of 1212, would not be today accepted, as historians do not think that the events of that year involved children (Raedts 1977), the other two, one an *epidémie démonolâtrique* in 1609 in Basses-Pyrénées (then pays de Labourd), the other a collective outburst of delirium in a hospice for found children in Amsterdam in 1556, would have to be looked at. The argument is in itself interesting, but what is striking in Morel's, as well as other psychiatrists' accounts, is that although he was based in an asylum he could not furnish sufficient clinical material to substantiate his discourse.

Then, there is the question of Prichard's concept of moral insanity, which, according to von Gontard, was the most common diagnosis given to children. The argument goes that in the early period psychiatrists were not keen on the idea of childhood insanity as it was thought that madness is a breakdown of reason and children being pre-reasoning beings should not go mad.[5] This was meant to change with the publication, in 1835, of Prichard's *A Treatise on Insanity* in which the concept of moral insanity is introduced, as it removed any theoretical objections to the idea of childhood madness, and von Gontard claims that this diagnosis became frequently used. It seems that this view has no substance. The diagnosis 'moral insanity' is not used once in relation to children in the Bethlem archives (as stated earlier, it was either mania, or melancholia or dementia). As far as literature is concerned, two things have to be said about the matter. First, it should be pointed out that Prichard did not have children in mind when he introduced the notion of moral insanity; second, when it appeared in relation to children, it was a few decades after Prichard's treatise was published and the term 'moral' had by then morphed into something altogether different to what Prichard meant by it. In the time of Prichard, 'moral' meant what we today would call 'psychological', and so, for example, the famous 'moral treatment' introduced by Pinel at the Salpétrière and the Tukes at the York Retreat, would be today designated as 'psychological treatment'.[6] However, in the second half of the 19th century the term began to acquire ethical connotations and when it was put into use to speak of children, it usually meant 'spiteful' or 'vicious' children (Maudsley 1867, Savage 1881), or as Crichton Browne put it, he who suffers from moral insanity 'suffers from entire perversion of the moral principle, from the want of every good and honest sentiment. He is actuated

[5] This author has not seen this argument put explicitly forward by the psychiatrists of the time, which suggests that this might be the historians' construction rather than a genuinely held view. And, at any rate, the tiny number of reports of insane children was not due to some conceptual difficulty, but simply because such children were a real rarity.

[6] For a succinct but clear exposition of the confusion around Prichard's notion of 'moral insanity' see Berrios 1999.

by impulse, or by the most selfish, depraved, and cruel motives' (Crichton Browne 1860: 314). At any rate, whether used in the sense that Prichard meant it or not, the term 'moral insanity' was liberally used by those who speculated on mad children, but if Bethlem archives are anything to go by, it was not used as a diagnosis given to children, as von Gontard claims.

In all, archival research in Bethlem archives, consultation of national statistics and reading of psychiatrists' texts lead one to conclude that, in the 19th century, cases of children that could be described as mentally ill, to use a modern term, were extremely rare, to the point were one can say that they were practically non-existent.

The absent mad child

For a number of reasons this absence of mad children comes as a surprise.

First, this lack of increase in incidents of childhood insanity must be seen against the massive growth in numbers of the mentally ill in the adult population in the same period (in Britain from around 5000 at the beginning of the century to well over a 100000 at the end). Second, in the same period, children became full medical subjects; first children's hospitals were set up (Great Ormond Street opened in 1852). Third, there was a great supply, so to speak, of marginal children in various institutions such as foundling hospitals, hospitals for idiot children, work-houses. Studies in county asylums indicate that there was some decanting from those institutions into the psychiatric domain, but nevertheless, as far as psychiatrists were concerned, the distinction between idiot and insane children that had been worked out in the first half of the century, remained clear.

In all, psychiatry established itself on the social and medical landscape, a fully-fledged paediatrics emerged, there were great numbers of marginal children in other institutions, we find an increased rhetoric about childhood insanity, but, despite all this, mad children are absent. The upshot of these observations is that it is not in the developments of the 19th century that we should look for the beginnings of child psychiatry. And von Gontard' claim that 'by 1900, all elements which later merged into child psychiatry had evolved – with the exception only of psychoanalysis and child guidance' cannot be accepted.

Katherine Gingell, who carried out the study in the Worcester County Asylum, referred to earlier, makes these two succinct comments:

> Children were treated exactly like adult patients, and therefore asylums did not contribute significantly to the development of the discipline of child psychiatry. (Gingell 2001: 432)

> Social historians […] have stated that changing society, increased industrialisation and declining community tolerance for the mad forced the insane into the asylum, allowing the psychiatrists of the day to assume an expertise and carve out a speciality for themselves. A sub-speciality of child psychiatry did not evolve within this context. […] Modern child psychiatric services have evolved from different roots to these. (*ibid.*: 435)

These are conclusions that this author fully agrees with.

The virtual absence of the mad child in the 19th century is remarkable and it goes against intuition, but this realisation is a singularly important one for our understanding of today's child psychiatry. It goes against intuition until one takes some time to reflect rather than get carried away by the rhetoric of some of the 19th century practitioners. Those who discoursed on child insanity were small in numbers and, throughout the century, we find it frequently stated that insanity under puberty is extremely rare; this was also the view of Leo Kanner, 'Fully-fledged mental illness […] is exceedingly rare before the 15th year of life' (Kanner 1935 :509). (And Kanner also quotes a study which found that among six thousand patients admitted to the Boston Psychopathic Hospital in the years 1923, 1924 and 1924 there were only four cases of manic-depression under the age of 16, and in all four the disorder developed after the 14th year of life. *ibid.*: 506). In other words, although in the 19th century, children often found themselves in conditions that can only be described as horrendous, they did not break down, in the manner that adults would. To put it simply, children do not go mad. It would take someone with knowledge of child psychology to explain why this is so, but the idea of a nine year old paranoiac sounds like an aberration, and this is why a child suicide always strikes as something absolutely shocking. But if the mad child is absent, than how did today's child psychiatry emerge?

PART 2 – 1902-1935

The turn of the century saw a change in psychiatry's conceptual outlook. In 1883, Emil Kraepelin published *Compendium der Psychiatrie* (English translation *Lectures on Clinical Psychiatry*). In this work Kraepelin began to outline a new classification of mental illness. He divided mental illnesses into two groups, in the first he grouped various forms of 'dementia praecox' (later renamed by Bleuler

'schizophrenia'), in the second manic-depressive psychoses. The book went through numerous editions, the last one appearing in 1927, and throughout the century it remained a blueprint for psychiatry's taxonomical efforts. The beginning of the 20th century also saw the wane of the influence of Morel's theory of degeneration. The influence of psychoanalysis and phenomenology brought in a new sophistication in thinking about mental illness. However, as far as practice went no noticeable change took place; hospitals continued to grow in size and what is important in the context of this article, children remained outside psychiatry's sphere of activities. Nevertheless, reports on difficult children started appearing, penned by those who had a great deal to do with them – paediatricians.

The nervous child

The first significant development of the 20th century that draws one's attention is a 1902 report of twenty children with a cluster of symptoms that was called 'defect of moral control'. Although this communication did not exert immediate discernable influence, it is nevertheless very significant as it points to the new developments concerning childhood and mental well being.

1. This report, which was published in the *Lancet*, came from a King's College and Great Ormond Street hospitals paediatrician George F. Still and it was presented in a series of three lectures to the Royal College of Physicians in London.

2. This was the first diagnosis to specifically describe a condition found in children, in other words, it was not an extension of already existing terminology developed in adult psychiatry onto children, like in 'childhood mania', for example.

3. Although Still named this new disorder 'defect of moral control', couched his rhetoric in the language of degeneration, and like a criminal anthropologist sought 'stigmata', the concept was new as he argued that the reason for a defect of moral control lie in a disturbed 'cognitive relation to environment' (Still 1902: 1008), which in time will transmute into 'attention deficit' and a disorder known as AD/HD (Attention Deficit/Hyperactivity Disorder).[7] This had nothing to do with earlier

[7] Still's report is mentioned in a number of historical accounts (Stone 1973, Wardle 1991, Neve and Turner 2002). But that his description of children with 'defect of moral control' was in fact the first clinical description of the 'attention deficit disorder' seems to have been first noted in a recent article (2007) by Mayes and Rafalovich. One should, however, point out that Still's long presentation includes such children amongst a number of other afflictions that he describes under the term 'defect of moral control'. Children suffering from psychological disturbances after various bouts of illness are mentioned, as well as a child that seemed to suffer from a severe form of

theories about masturbatory activities, childhood sexuality (which was already on the agenda for at least two decades by the time Freud announced his theory), or 'instinctual insanity'; it is an entirely new clinical finding.

4. Still considered this condition to be distinct from idiocy or child insanity (which, like his predecessors, he considered extremely rare). It is not necessarily a chronic condition; it can come and go, '[there are] cases in which periods of defective moral control alternate with periods in which no such defect is present' (ibid,: 1163), and this makes it also distinct from another childhood condition that was receiving much attention, feeble-mindedness.

5. The fact that 'attention' or, in Still's language, poor 'cognitive relation to environment' has become the focus for a diagnosis, points towards a new social context in which the child's comportment will be judged – the classroom, as by this time the education system had become compulsory and universal. Still evokes the importance of this context as some of these children have an 'abnormal incapacity for sustained attention' (*ibid.*: 1166) and he suggests that there might be a need to separate children suffering from 'defect of moral control' from others in a classroom (*ibid.*: 1167).

Although the Attention Deficit Disorder has acquired immense currency (and has been since used in adult psychiatry), this only came later. What is important is that we see emerge a new type of a difficult child, one that is neither an idiot nor insane, it will be called the 'nervous child'. Mention of such a child appeared earlier, notably, in the text of Charles West, the eminent paediatrician based in the Great Ormond Street hospital. He first mentions such a child in his *Lectures on the Diseases of Infancy and Childhood* (1848), but in subsequent editions, he did not develop the theme. In time, the most famous became the 1919 text 'A Nervous Child' by Hector Charles Cameron, which went into a few editions. What were the types of afflictions typical of a nervous child? We find problems with appetite, disturbed sleep, bed wetting, bad habits, phobias, night terrors, nervous vomiting, and other signs of nervousness (twitching of facial muscles, air swallowing, over excitability, constipation, etc.). Cameron concluded the book with a chapter 'A nervous child and school'; in 1933, he extended the chapter into a full-length study, of the same title (Cameron 1933).

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dyslexia, for example. Nevertheless, it seems to this author that Mayes and Rafalovich are right, as the first description of AD/HD, if still only embryonic, can be teased out of Still's account particularly that at one point he does indeed concentrate on the problem of these children's disturbed capacity for any sustained attention (Still 1902: 1166).

Psychometric scales

In view of the fact that education became compulsory it is not surprising that this began to pose a problem in schools, as all children, whatever their aptitude, were compelled to sit in the classroom; the problems they posed soon became of paramount importance. In 1905, following a request of the French government, two psychologists Alfred Binet and his student Theodore Simon introduced a method of diagnosing different degrees of mental retardation (Binet and Simon 1905). This was a method consisting of a series of tasks that children were asked to perform that allowed a precise assessment of the child's development. Within the next few years this was extended to measure 'normal' children and this gave educators indicators of what could be expected of children at a specific age. Binet and Simon's work was swiftly translated into English (in the US), it was modified to become the Stamford – Binet scale; it became widely used and has been ever since developed and improved. The idea of being able to test and determine aptitude psychometrically goes back to Francis Galton's eugenics, but these scales were a new development that could only be achieved in the context of the new educational system. The introduction of the first psychometric scales was the beginning of a new science of children. Now they could be classified, segregated, it was known what could be expected of children at different stages of their development; and failure to achieve this, either because of mental retardation or some other problems, could be objectively measured.

Child Guidance

A nervous child was recognised and in detail described, new methods of measuring the child's development made the diagnoses more precise; and, before long, a need to help those children arose. As a consequence of this need, one can see the emergence of the Child Guidance Clinics. Historians' accounts (Thom 2000, Wardle 2000) suggest that this was a complex affair. Nevertheless, some basic themes can be discerned.

The beginnings take us to the US, where the Child Guidance movement grew out of the Mental Hygiene movement. The driving force behind the Mental Hygiene movement was Clifford W. Beers, who in his 1908 autobiographical work *A Mind That Found Itself* describes the kind of treatment he received in the mental hospital after suffering a breakdown (Beers 1913). In the following years he devoted his energy to establishing outpatient facilities for dealing with people with mental

problems. The Clifford Beers Clinic in New Haven was set up in 1913. Beers had the cooperation of the doyen of American psychiatry at the time, Adolf Meyer, who by then began to espouse a psychosocial model of mental illness and believed there was need for interventions in the community. These structures were used when, in the early twenties, first child guidance clinics were set up (Deutsch 1947).

Britain did not have the equivalent of the Mental Hygiene Movement (although, in time, the movement acquired a truly international dimension) but the inspiration, and most importantly funding, for the first Child Guidance Clinics came from the US. There were, however, two significant differences. The American initiative was closely linked, at least at the beginning, with juvenile courts, which was not the case in England (Thom 1992: 209) and, secondly, the involvement of psychiatrists was less prominent, there was no equivalent of Adolf Meyer in England, the driving force behind the Child Guidance Movement was the psychologist Cyril Burt. The first Clinic opened in 1927. One important development that took place, both in the US and Britain, was the emergence of social work, which was to play an integral part in these new settings. In the US, social work training began at the beginning of the century, in England the first course in social work was set up at the London School of Economics in 1929 (Wardle 1991: 56). But what seems most important is that, all historians' accounts underline the multidisciplinary character of the Child Guidance Clinics as it was an effort that involved paediatricians, health workers, educationalists, social workers, psychoanalysts, psychologists as well as psychiatrists.

Leo Kanner

What is notable is that none of the developments discussed above (the emergence of a 'nervous child', psychometric scales, Child Guidance Clinics) had any connection with psychiatric thought of the previous century. Furthermore, it is not clear whether these developments can be described as psychiatry, at any rate, in the first third of the century, the concept of a 'child psychiatrist' did not even exist. So a question remains. How did all the activities of paediatricians, health workers, educationalists, social workers, psychoanalysts, psychologists that were involved in the Child Guidance Clinics, and which took place outside the psychiatric context, become 'child psychiatry'? To all intents and purposes, the term was introduced by

Leo Kanner in his 1935 text, which carries that very title 'child psychiatry'. (According to Kanner, the term was used earlier only once.)[8]

Now, unlike the texts of Crichton Browne, Maudsley and others of the preceding century, which did not bear on the developments described above, Kanner's *Child Psychiatry* is a veritable event, the consequences of which are difficult to overestimate.

It is clear that Kanner had great knowledge of children, probably the first psychiatrist with such deep clinical experience of youngsters. And this is evident in his remarkable monograph. It runs to something like 250 thousand words and consists of 44 chapters. It is also preceded by two prefaces; the first, coming from Adolf Meyer, the second, coming from Edward A. Park, head of the psychiatry department at Johns Hopkins University. Kanner belonged to the cream of psychiatrists of his generation and in his work he displays extraordinary erudition. This publication could have not gone unnoticed; it became the standard text on the subject for the next three decades, its dominance only waning with the publication of Michael Rutter and Eric Taylor's *Child and Adolescent Psychiatry* in 1976.

There are a number of features that make this work stand out. First of all, the various problems concerning children that were reported first by paediatricians and which led to the emergence of the concept of a 'nervous child' are repeated. We find in Kanner's text everything – temper tantrums, nail-biting, problems with appetite, stuttering, antisocial behaviour, sleep disturbances, enuresis, migraine and they are all recast in psychiatric terms. One can see how different it is when we compare it with Cameron's *The Nervous Child*. Cameron was a paediatrician from Guy's Hospital, and his writings are devoid of any psychiatric thought (and he was a little apprehensive as far as the then emerging psychoanalytical theories were concerned); they come across as observations and advice of a good, sensible and sensitive doctor. Kanner's work is a *tour de force*, he almost overwhelms with his psychiatric erudition, (not all of it necessarily relevant to the subject). One way of putting it is that Kanner's 'child psychiatry' is no more than an annexation of the territory identified by the Child Guidance movement.

But since it is the territory of Child Guidance that is recast in psychiatric terms it is a psychiatry that shows a pronounced difference from adult psychiatry. This is not just the question of the age of patients. Although, as it has been demonstrated, psychiatry evolved at the beginning of the 19th century out of the confinement of the insane, that is, its origins lie in social control concerns rather than medical thought,

[8] Subsequently it has been shown that the term 'child psychiatry' has been used more than once (Harms 1962) but this does not change the picture significantly as it did not have any currency before Kanner's time.

and despite the frequent arguments (often crude) that mental illness is a social construct, it still remains incontestable that insanity exists (not even the likes of Foucault, Laing or Basaglia contested this). This, one could say, legitimises the psychiatric enterprise. And, since madness exists, we owe to psychiatrists sophisticated and often sensitive clinical descriptions of various mental conditions. Child psychiatry deals with restlessness, nervousness, bed wetting, disruptive behaviour, and the like, but its relation with insanity is tenuous, if there is any at all. Kanner does have a chapter on mental conditions that would be in earlier days referred to as insanity – under the title 'Major Psychoses' – but it is just one, penultimate chapter, out of the 44 that make up the book. The discussion is very thorough indeed, only that it is based exclusively on writings about madness in adults (Kraepelin, Bleurer, Schneider, etc.) and he confirms the observations of his predecessors that insanity in children under the age of 15 is 'exceedingly rare'. (Interestingly, he deleted this sentence in subsequent editions without, however, presenting a sufficient number of cases to invalidate this observation.)

So why did Kanner write the chapter? (In Cameron's *The Nervous Child* we do not find any mention of children suffering from what could be considered insanity.) Had Kanner not included the chapter, we would have a strange text that pertains to psychiatry but that has no relation to insanity. And while psychiatry as a whole can live without mad children, child psychiatry cannot. There might have been some sightings of a mad child, like that of a very rare animal, but it remains an imaginary child, a *possible* one, not one that is seen in the clinic.

For psychiatry to take over the clientele of the Child Guidance Movement one further profound change was necessary. Up to the time of Kanner, the practice of the psychiatrists was to treat those deemed by the community insane and, once this was confirmed by the GPs (in England), they were delivered by this community to the mental hospital. The psychiatrists' activities were practically entirely confined to the asylum, where they often resided. While they remained in the asylum *child* psychiatry could not have developed as the community was not inclined to deliver children to the asylums' gates, even more so with the emergence of the Child Guidance movement, which developed necessary services. Psychiatrists had to change their old habits; their sphere of activities had to be re-defined. Rather than being asylum based, this new psychiatry will be based in the school (and in the US also the juvenile court); it will also enter the family. In this sense, Kanner's text is quite revolutionary as it must be the first that carries the word 'psychiatry' in its title and which has nothing to do with the mental hospital.

Did the fact that psychiatry took over the problem of difficult children make a difference? Yes, it seems clear it did. It led to a medicalisation of children's troubles; the enthusiasm to try drugs on children would perhaps be less pronounced; first trials on amphetamines on children were carried out in 1937, and it comes as no surprise to learn that this happened within the context of a hospital clinic, the Boston 'neuropathological unit for children', to be precise, and the current trend to prescribe medication to children (mostly Ritalin) has grown to quite alarming proportions. Furthermore, only after children were brought into the psychiatric domain, will we start seeing separate wards for children set up in hospitals, something unthinkable in the 19th century, which is when, it is thought, psychiatry was at its most excessive.

Concluding remarks

If this account is just, than what strikes about it is the almost complete discontinuity between what we find regarding children and psychiatry in the 19th century and developments in the 20th. We can summarise by pointing out the elements that did not exist in the previous century. First, there is a new setting – the school, and it is in this setting that children became a concern. Second, we find a clinical description of a new type of troubled child – the 'nervous child'. And third, a new system of measurement and evaluation of the child's development, the psychometric tests, is put into place. These led to the emergence of the Child Guidance movement and the setting up of first clinics, which specifically dealt with children. The other set of changes pertains to psychiatry. First, in order to bring children into its orbit psychiatry had to forgo its link with insanity; second, in order to become involved with children, psychiatrists had to leave the walls of the asylum.

Perhaps the matter becomes clearer if we try to look at the problem by studying it backwards, beginning with the present day. We would begin by noticing that there are two parallel services for children, one designated as 'child psychiatry' the other as 'child guidance' (the emergence of child psychiatry did not lead to the disappearance of Child Guidance Clinics). We would then look, for example, at the latest edition of the *Diagnostic and Statistical Manual of Mental Disorders*, DSM-IV-TR, to see what kinds of ailments are discussed in this psychiatry textbook. We would arrive at a conclusion that there is a considerable overlap between the services provided by Child Guidance Clinics and child psychiatrists. Moving back in time we would find the publication of Rutter and Taylor's *Child and Adolescent Psychiatry* in 1976 and then Leo Kanner's 1935 *Child Psychiatry*. At this point the 'child

psychiatry' thread would break. However, one could continue with the Child Guidance Movement story for another two decades (if the American developments are taken into account as well as the earlier Mental Hygiene movement). This would bring to an end the Child Guidance Movement story. The last element that would remain would be the clinical descriptions of 'nervous children' that paediatricians began to report at the beginning of the century. At this point, all the threads that make it possible to retrace these developments are broken; there does not seem to be any passage from these developments to the writings of the nineteenth century psychiatrists. The two main reasons why one cannot link the twentieth century developments with the earlier period have been spelt out earlier. First, the type of child that came to the attention of health workers was not yet identified; second, at the time, the psychiatrists' activities were confined to the asylums, as the notion of community work was not yet formulated.

However, a clarification as to why the school has such prominence in this account is necessary as one could raise objections to this. It could be pointed out that the Child Guidance Movement in the US was at first linked with juvenile courts, not with schools; and furthermore one notes that the Child Guidance Clinics saw a great number of children that presented symptoms such as delinquency or the frequent cases of bed wetting, which would not seem to be related to the school, at least not directly. First of all, one should note that the question of the problem of difficult children in the classroom is posed from the beginning by paediatricians who first reported cases of 'nervous children' (at least in English literature). Furthermore, although the American developments began in juvenile courts, soon we see the question of schoolchildren coming to the foreground in America as well. However, it is not just the frequency with which the problem of difficult children in schools crops up in various contexts, the prominence given to the school follows a precept worked out by French philosophers of science, beginning with Gaston Bachelard, which states that for knowledge to be constructed (for knowledge, in this line of reasoning, is a construct not a find) earlier knowledge is necessary, for example, without the tensor calculus the General Theory of Relativity could not have been formulated. However, as we have seen, prior psychiatric knowledge was not the condition for the development of child psychiatry (even if its emergence could not have happened without the earlier appearance on the social landscape of psychiatry as such). Therefore, a further argument of Bachelard has to be considered. It states that for knowledge to be constructed a suitable setting is also necessary.[9] To make advances

[9] What Bachelard meant by the 'setting' needs perhaps calls for some explaining. Bachelard's analyses are confined to 'hard sciences', mathematics, physics and chemistry. In this context he coined the term 'phénoménotechnique', by which he meant the complicated apparatus needed

in nuclear physics, for example, it was necessary to begin building particle accelerators; chemistry was always linked with the laboratory, and likewise in social sciences a setting is needed in order to arrive at knowledge of social phenomena. In this context one can say that psychiatric knowledge could have not been developed without the asylum, within which it was constructed, similarly, psychoanalytical knowledge could not have been arrived at without the couch; and, since we have seen that it was not the asylum that provided the setting for the development of child psychiatry, the context in which the 'nervous child' became apparent – and a problem – had to be identified. While the classroom was not the only place where we find such children, there were also juvenile courts, children's hospitals; nevertheless, a new science of children, which plays an integral part in child psychiatry, could not have emerged without the classroom. In the 19th century some children received no schooling, either because they were in some ways retarded, or because they already began to work and what they earned was crucial for their families survival, or simply because they were tearaway kids. Once education became compulsory, the classroom became a context that no child could escape, and it is in this context that the extent of the problem of the 'nervous child' became apparent, and in this context it could be compared to a 'normal' child. It is interesting to see, and it seems no coincidence, that the time George Still was making his first clinical observations of children with disturbed attention, which he presented in his 1902 lectures, was the time 100% education was finally achieved. (Although the first Education Act goes back to 1870, it took a few more Acts and a great deal of government effort and money to make universal education reality, this only happened at the turn of the century.)

However, this has to be qualified. This is not an argument that the educational system 'fabricated' nervous children, there could well have been children in the previous century that would have fitted that description; rather, the argument is that there did not exist the context in which this problem could have become apparent and also that there was no social necessity for identifying nervous children.

Finally, some remarks of a historiographical nature are necessary. When dealing with the history of child psychiatry we find two types of accounts. On the one hand, some historians speak of child psychiatry in the 19th century, as does von Gontard, which has been chosen as an exemplar of this view; on the other hand, we

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to perform scientific experiments (the particle accelerators are a good example of these). But this idea has been taken up by others, Georges Canguilhem and Michel Foucault, amongst others, to apply it to a wider range of scientific investigations, medicine and social sciences. In time, the term 'dispositif' has become the most widely used, but the discussion of the term and the philosophical specificity that it has acquired cannot be discussed within the scope of this article.

find accounts that do not make any reference to the 19th century, Kanner belongs to the latter. (Another example would be a recently published article about the development of Child Psychiatry in Norway, which makes no reference to the nineteenth century, Ludvigsen and Seip 2009.) This author read Kanner's account when already familiar with von Gontard's text and the first reaction was that Kanner wilfully ignored developments that preceded his time, and that he was making false claims about the novelty of child psychiatry; after all, attempts by historians to reconstruct some form of child psychiatry in the 19th century could not have been based on nothing.

Much depends on what status one accords to the findings in the 19th century. 'Many authors have shown that, for most European countries, the origins of child psychiatry can be traced back to the 19th century' stated von Gontard. In this context, 'origins' presumably means 'beginnings' (as it is difficult to see how it could mean 'cause'). However, locating the first reports of insane children does not necessarily mean that activities that could be called 'child psychiatry' began with these reports. This is because psychiatry is not constituted by an odd case description; psychiatry is a social phenomenon, which reflects the society's perception that a collective effort to deal with those who are deemed insane is necessary, psychiatry deals in large numbers. This was so from the very beginning; when Philippe Pinel was appointed at the Salpêtrière, which is considered one of the founding moments of psychiatry as we know it, he had some three thousand patients in his charge; England at the beginning of the 19th century already had around five thousand inmates in various establishments. While clinical descriptions of various conditions are indispensable for the development of the science of psychiatry, statistics are also an integral part of it (and one notes that the full title of the DSM-IV-TR, mentioned earlier, includes the word 'statistical'). The difference between Maudsley's classification of child insanities, in which he proposed seven types, and Kraepelin's division of mental illnesses into two great categories, is that Maudsley based his classification on less than two dozen reported cases, while Kraepelin had at his disposal notes on hundreds and hundreds of patients that passed through his clinic.

However, if one assumes that child psychiatry already existed in the 19th century, albeit without being formalised, there will be a temptation to produce an exhaustive account of *everything* that touches on children and psychiatry; *every* utterance coming from psychiatrists will be referred to and every reported case mentioned as adding to the body of knowledge on the subject. In such a narrative remarks about general conceptions of childhood also seem to be *de rigeur*. (Philippe Ariès's *Centuries of Childhood* is most often quoted. This is indeed a remarkable text, only that it is not clear what relevance it has to psychiatric practices in relation to

children.) In all, one sometimes gets the impression that we are confronting what one could call the method of 'hopeful accumulation', that is, if one can accumulate enough material on the subject than, somehow, an imaginary social phenomenon (in this case child psychiatry in the 19th century) will become reality. After all, the title of von Gontard's article, 'The Development of Child Psychiatry in 19th Century Britain', already announces the existence of this phenomenon. One is tempted to refer to Marc Bloch, as he expressed dislike of exhaustive research typical of some historical productions, he wrote, 'polymathy can well assume the form either of recreation or of mania, but it cannot pass for one of the proper tasks of the intellect […] history will rightfully claim its place among those sciences truly worthy of endeavour only in proportion as it promises us, not simply a disjointed and, you might say, a nearly infinite enumeration, but a rational classification and progressive intelligibility' (Bloch 1992: 9).

A disjointed and almost infinite enumeration is what characterises von Gontard's account. But there is more, one needs to understand why there were so many misrepresentations. For example, the term 'moral insanity' was not the most commonly used diagnosis as he states, research suggests that it was used by those who wrote about childhood insanity, but as a diagnosis, not at all; there is no evidence of any increase in child admissions towards the end of the nineteenth century; and, in general, the frequent usage of terms like 'most common', 'often' creates the impression of abundance of children in psychiatric care is deeply misleading and can lead a researcher astray.[10]

The eagerness to demonstrate the existence of child psychiatry in the nineteenth century would seem to mirror the conviction that there were insane children in the nineteenth century that we can see in the writings of some of the psychiatrists of the period. '[N]o age is exempt from attacks of Insanity' assure the readers Bucknill and Tuke. We also see this in Crichton Browne's essay and in Maudsley's writings. It is interesting to note that this conviction emerges only in the second half of the century. This could well have had something to do with the theory of degeneration, which was introduced in 1857 by Morel. This swept like quick fire throughout Europe. The theory stated that all afflictions such as alcoholism, prostitution, all sorts of deviancy as well as mental illness run through degenerate

[10] Such a presentation can be very misleading indeed. Some time ago, at Goldsmiths, a mature student prepared a proposal for a PhD thesis entitled 'Children and Insanity in the Nineteenth Century'. The proposal was based on secondary material and the application for funding was successful. Sadly, soon after, the student went down with cancer and passed away. This author was involved in helping put the project together and afterwards decided to pick up the research. It took well over a year to realise that funding was given for a proposal that was effectively a wild goose chase.

families. Soon, some psychiatrists held that children were just as prone to madness as adults; and, although the mad child remained an elusive being, entire chapters and the first monographs on child insanity began to appear. Psychiatrists have often been guilty of writing fiction, which is reason why historians should sift through the material with a critical eye.

But one does not want to end on a too negative note. There are a number of accounts that comprehensively enumerate nineteenth century developments that are of great help to any researcher on the subject and, which, unlike von Gontard's article, do not make statements that turn out to be inaccurate and misleading. If this account has taken a different approach, it is because as the research evolved, its focus changed. At the outset, the intention was to deepen our understanding of the developments in the 19th century. Since first reports on child insanity go so far back, finding a way of fleshing out the *de facto* existing child psychiatry in that period seemed to be desirable; even more so when one notes that in general histories of psychiatry or paediatrics the question of children and mental illness is never aired; a wider study of marginal children and their relation to psychiatry seemed necessary. But in time, it became clear that the existing historians' accounts give a rich and detailed enough picture and that any further research would only make the absence of mad children more striking and make it clearer that 20th century child psychiatry did not evolve out of the earlier practices. Consequently, focus of this work changed and ended up being governed by the question that is posed at the beginning of this article: Why is it that despite the fact that first reports of mad children go back to the beginning of the 19th century, child psychiatry only emerged about a century and a half later? It seemed that an approach that alerts to the differences between the two centuries, to the shifts in perceptions and changes in attitude towards children as well as changes in psychiatric practice that took place might go some way towards explaining the almost 150 year 'delay'. Whether this explanation is satisfactory it remains for the reader to decide.

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Visualizing Abduction

Fernando Soler Toscano
(Grupo de Lógica, Lenguaje e Información, Universidad de Sevilla)
fsoler@us.es

1 Introduction

1.1 What is abduction?

Traditionally, Logic has focus on *deduction*. Its main objective has been to clarify when a given inference is *valid*, that is, when the conclusion is *entailed* by the premises. One of the pillars of this enterprise is the Aristotelian *Analytics*, where the syllogism is taken as the paradigm for reasoning. The modern notion of *logical consequence*, though is defined in a more abstract and formalised way, ratifies the privileged position of deduction as the centre of the logical universe.

But there exist other kinds of inference. The philosopher C. S. Peirce (1839 - 1914), distinguishes three kinds of reasoning: *deduction*, *induction* and *abduction*. Peirce, contrary to the logical tradition, considers that the most interesting kind of inference is abduction. Peirce characterizes abduction in this schematic way:

> The surprising fact, F, is observed;
> but if H were true, F would be a matter of course.
> Hence, there is reason to suspect that H is true (CP 5.189, 1903).

So abduction is the inference which formulates a hypothesis H that explains a surprising fact F. If we turn back to Aristotle, in the *Prior Analytics* he establishes not only the categorical syllogism, but also other inference patterns which change the order of the propositions. Those patterns, though non deductively valid, are very important in human reasoning. One of them is the *apagoge*. An example of Aristotle:

> Animals without bile live many years.
> But the man, the horse and the mule live many years.
> Hence, the man, the horse and the mule do not have bile.

The similarity between the Aristotelian *apagoge* and the Peircean *abduction* is not a matter of chance. In fact, Peirce himself admits the borrowing of concepts from Aristotle. But for Peirce, abduction is not only a kind of reasoning among others. Peirce thinks that abduction is the most interesting kind of inference because it is the only way of introducing new ideas (EP 2:216, 1903).

Thanks to the Peircean works, abductive reasoning has become a central topic not only in Logic, but also in other disciplines as Linguistics, Epistemology, Artificial Intelligence and, of course, Philosophy of Science.

1.2 Is abduction the logic of discovery?

Philosophy of Science is, exactly, the discipline in which the importance of abduction is more discussed. It is usual the distinction between the *context of discovery* and the *context of justification*. The former is understood as the set of processes which lead to the formulation of a new theory, while the latter refers to the methods used in science to confront the hypothesis with the empirical evidence. P. Lipton (1991) defends that scientific discovery is achieved as an *inference to the best explanation*, so *explanatory reasoning*, and hence abduction, play a central role in the context of discovery. But other authors are critical with these ideas.

S. Paavola (2004) collects the arguments that are commonly given against the characterisation of abduction as a *logic of (scientific) discovery*. The first of them is that in the Peircean formulation of abduction – given above – the only requirement for H is that it would make F true. Authors like P. Achinstein argue that this is too permissive, because it does not exclude some improbable or wild hypotheses if they would explain F. Other criticisms insist in the idea that abduction cannot be understood as a logic of discovery because the explanatory hypothesis H is already included in the premises, because prior to infer H we need to know that H would explain F. Then, as scientific discovery introduces new ideas in science, it cannot be the result of an abductive inference.

To defend abduction as a logic of discovery, S. Paavola follows a distinction borrowed by J. Hintikka from game theory. It is the distinction between *definitory* and *strategic* rules. The former settle the legal moves in a game, whereas the latter are used to decide which is the most suitable rule among all the possible. Logic has been traditionally devoted to definitory rules of the calculi and has barely paid attention to the strategic rules which are able to lead to a good proof, in a strategic sense that goes beyond classical soundness.

In line with this, J. Hintikka (1998) takes from T. Kapitan four theses which sum up the main characteristics of abduction:

Inferential Thesis. Abduction is, or includes, an inferential process or processes.

Thesis of Purpose. The purpose of "scientific" abduction is both (i) to generate new hypotheses and (ii) to select hypotheses for further examination; hence, a central aim of scientific abduction is to "recommend a course of action".

Comprehension Thesis. Scientific abduction includes *all* the operations whereby theories are engendered.

Autonomy Thesis. Abduction is, or embodies, reasoning that is distinct from, and irreducible to, either deduction or induction.

We will come back to Kapitan theses. For now, it is enough to remark that there are reasons to accept abduction as a logic of discovery. But then, abduction must include some kind of strategic rules and satisfy the above theses which set up abduction as an autonomous inferential process, irreducible to deduction.

2 Abduction by dualizing deduction

2.1 Logic-based abduction

To introduce the formal definitions concerning abduction, let us consider that $\mathcal{L}$ is a propositional language with the habitual connectives, and let $\vDash$ be defined as the classical logical consequence relation. We use capital Greek letters for sets of formulas and small Greek letters to denote formulas.

Definition 1 (Abductive problem). *Given* $\Theta \subset \mathcal{L}$ *and* $\phi \in \mathcal{L}$, *we say* (Θ, ϕ) *is an abductive problem iff (if and only if):*

$$\Theta \nvDash \phi \tag{1}$$

$$\Theta \nvDash \neg\phi \tag{2}$$

Definition 2 (Abductive solution). *Given the abductive problem* (Θ, ϕ), $\alpha \in \mathcal{L}$ *is an abductive solution for it iff:*

$$\Theta, \alpha \vDash \phi \tag{3}$$

$$\Theta, \alpha \nvDash \bot \tag{4}$$

$$\alpha \nvDash \phi \tag{5}$$

According to definition 1, an *abductive problem* appears whenever there is a formula ϕ such that neither it (1) nor its negation (2) can be derived by only the

background theory Θ. Then, α is an *abductive solution* if it extends the theory in a way such that now $\Theta \cup \{\alpha\}$ entail ϕ (3). Some authors demand only this condition, but following A. Aliseda (2006) we have included in definition 2 requirements (4) and (5) to ensure that α is not a trivial explanation. That is, α is consistent with the theory and it does not entail ϕ by itself without the theory, so it is an explanation *within* the theory. With these strong requirements, abduction becomes an interesting non-monotonic inference very different from deduction. Additional conditions may be added. In fact, we will later concentrate on *conjunctive minimal explanations*, conjunctions of literals such that no proper subset of them is an abductive explanation.

To mechanise the generation of abductive solutions, many calculi have been proposed. Most of them make *abductive uses* of *deductive calculi* by exploiting the equivalence of (3) with:

- $\Theta, \neg\phi \vDash \neg\alpha$. This is done by most on the logic-based approaches coming from Artificial Intelligence (Kakas *et al.* 1998). Using the resolution calculus, the clausal form of $\Theta \cup \{\neg\phi\}$ is obtained and then resolution is applied. Any dead end of the resolution tree can be taken as the negation of an abductive solution.
- $\Theta, \neg\phi, \alpha \vDash \bot$. This is what the semantic tableaux approach does (Mayer *et al.*, 1993). The tableaux of $\Theta \cup \{\neg\phi\}$ is obtained and then a formula α which closes all the open branches is searched.

In both approaches $\neg\phi$, the negation of what is intended for explain, is in the starting point of the abductive search. So the abductive process starts by negating the empirical evidence which tries to explain. This is somehow similar to *reductio ad absurdum*, because the explanation α becomes an extension of Θ that makes impossible $\neg\phi$.

Proceeding in this way has moved logical abduction further away from the great expectations coming from Philosophy of Science. It is hardly believable that a logic of discovery proceeds by negating exactly what is trying to explain. It is not possible to take the above procedures as a logical model either of scientific or commonsense reasoning.

2.2 The δ-resolution calculus

Definitions 1 and 2 restrict the scope of abductive reasoning. They do not seem to be appropriate to include «*all* the operations whereby theories are engendered», as Kapitan's *comprehension thesis* requires. Anyway, though reductive, those definitions can be understood as a scale model of (scientific) explanation. They comprise the

common features of any explanatory process. But to increase their interest a proper abductive calculus should be formulated. That is, a calculus which neither proceeds in an *indirect* way or assimilates explanation to *reductio ad absurdum*.

That is what we have done with the δ-resolution calculus. It is a reformulation of resolution which turns it an abductive calculus which generates explanations in a *direct* way. It works by using the equivalence between (3) and $\alpha \models \Theta \rightarrow \phi$, where Θ denotes the conjunction of its formulas. Now, the observation ϕ is not negated, and we obtain directly abductive explanations, not their negations. Let us see an informal sketch of the process applied to an example of Kakas *et al*. (1998). Let $rained$, $sprinkler$, $grass$ and $shoes$ represent, respectively "rained last night", "sprinkler was on", "grass is wet" and "shoes are wet". Then, the theory is:

$$rained \rightarrow grass$$
$$sprinkler \rightarrow grass$$
$$grass \rightarrow shoes$$

We want the theory to explain that "shoes are wet", that is:

$$(rained \rightarrow grass) \wedge (sprinkler \rightarrow grass) \wedge (grass \rightarrow shoes) \rightarrow shoes \qquad (6)$$

If this is a valid formula, then the theory itself explains that the shoes are wet. Otherwise, the theory needs an additional support, that is, an abductive explanation. When is (6) true? A conditional is true when the antecedent is false or the consequent is true:

$$\neg((rained \rightarrow grass) \wedge (sprinkler \rightarrow grass) \wedge (grass \rightarrow shoes)) \vee shoes \qquad (7)$$

This sets the two possible extremes of an abductive process. We can refuse the theory if the observation contradicts it, or we can add the observation to our knowledge base, if there is no possible explanation within the theory. But **Error! Reference source not found.** is equivalent to:

$$(rained \wedge \neg grass) \vee (sprinkler \wedge \neg grass) \vee (grass \wedge \neg shoes) \vee shoes \qquad (7)$$

Any disjointed term in (7) is a formula which supports (6), by either contradicting the theory or assuming the observation. But, is there any intermediate alternative? Of course, these are the abductive explanations. For example, both $(grass \wedge \neg shoes)$ and $shoes$ support (6). So, also $grass$ because, whenever it is true, one of $(grass \wedge \neg shoes)$ or $shoes$ is too, as a trivial semantic reasoning shows. So $grass$ is a possible explanation. It is not the best, as we can continue the abductive process, to explain why the grass is wet. From just obtained $grass$ and

$(rained \wedge \neg grass)$ we get $rained$. Also, $grass$ and $(sprinkler \wedge \neg grass)$ produce $sprinkler$. The three obtained explanations, $grass$, $rained$ and $sprinkler$ are abductive solutions.

The previous example shows that δ-resolution is in fact a dual version of resolution[1]. In the following, we introduce the most important definitions and results concerning propositional δ-resolution. Formal proofs can be found in (Soler-Toscano *et al*., 2006) and an extension to predicate logic in (Soler-Toscano *et al*., 2009).

Definition 3. *A δ-clause Σ is a finite set of literals of $\mathcal{L}$. Given a boolean valuation v, $v \vDash \Sigma$ iff v satisfies all the literals of Σ. The empty δ-clause, $\Diamond$, is universally valid.*

Definition 4. *A δ-clausal form A is a finite set of δ-clauses. Given a boolean valuation v, $v \vDash A$ iff v satisfies at least one δ-clause of A. The empty δ-clausal form is not satisfiable.*

It is possible to translate any formula to an equivalent δ-clausal form by obtaining its disjunctive normal form. The following definition introduces two additional restrictions in the requirements of definition 2. We select only minimal conjunctions of literals.

Definition 5 (Set of abductive δ-clauses). *Given the abductive problem (Θ, ϕ), the set of abductive δ-clauses $\mathcal{A}bd(\Theta, \phi)$ contains every δ-clause Σ such that:*

- The conjunction of the literals of Σ is an abductive solution for (Θ, ϕ).
- There is no $\Sigma' \subsetneq \Sigma$ such that $\Theta, \Sigma' \vDash \phi$.

Definition 6 (δ-resolution rule). *Given two δ-clauses $\Sigma_1 \cup \{\lambda\}$ and $\Sigma_2 \cup \{\neg\lambda\}$, the δ-resolution rule produces their δ-resolvent $\Sigma_1 \cup \Sigma_2$:*

$$\frac{\Sigma_1 \cup \{\lambda\} \qquad \Sigma_2 \cup \{\neg\lambda\}}{\Sigma_1 \cup \Sigma_2}$$

Though this rule is presented with the same format that the standard resolution one, they are different since now we are working with δ-clauses. In the standard

[1] Dual versions of the resolution calculus are introduced in (Eder 1991) and (Ligeza 1993). We introduced the abductive possibilities of dual resolution in (Soler-Toscano *et al*. 2006).

resolution calculus (Robinson, 1965), every obtained clause is a logical consequence of the original set. Now, any δ-clausal form which contains $\Sigma_1 \cup \{\lambda\}$ and $\Sigma_2 \cup \{\neg\lambda\}$ is a logical consequence of $\Sigma_1 \cup \Sigma_2$, because any valuation v which satisfies $\Sigma_1 \cup \Sigma_2$ satisfies λ or $\neg\lambda$, so v satisfies $\Sigma_1 \cup \{\lambda\}$ or $\Sigma_2 \cup \{\neg\lambda\}$. Then v satisfies any δ-clausal form with $\Sigma_1 \cup \{\lambda\}$ and $\Sigma_2 \cup \{\neg\lambda\}$.

Definition 7 (Proof by δ-*resolution*). *The δ-clause Σ is provable by δ-resolution from the δ-clausal form A, what we express with $A \vdash_\delta \Sigma$, iff there is a sequence of δ-clauses such that:*

• Each δ-clause in the sequence is either a member of A or a δ-resolvent of previous δ-clauses.

• Σ is the last δ-clause of the sequence.

In deductive logic, soundness and completeness results are important to prove the adequacy of a calculus. Now, these properties are related to abductive adequacy of dual resolution, that is, the δ-resolution process produces every abductive solution, and just them.

Theorem 8 (Soundness). *For every δ-clausal form A and δ-clause Σ, if $A \vdash_\delta \Sigma$, then $\Sigma \vDash A$.*

Theorem 9 (Completeness). *If A is an universally valid δ-clausal form, then $A \vdash_\delta \Diamond$.*

The following theorem proves the *abductive completeness* of the δ-resolution calculus, that is, all the δ-clauses that satisfy Definition 5 can be proved by δ-resolution.

Theorem 10 (Abductive Completeness). *Let A be the δ-clausal form of $\alpha \in \mathcal{L}_p$. Then $A \vdash_\delta \Sigma$ for each satisfiable δ-clause Σ such that:*

• $\Sigma \vDash \alpha$.

• For every $\Sigma' \subsetneq \Sigma$, $\Sigma' \nvDash \alpha$.

Definition 11 (Saturation). *Given the* δ*-clausal form* A*, the set* saturation by δ-resolution *from* A*, that we represent as* A^{δ}*, is the minimal set which contains every* δ*-clause* Σ *such that*

- Σ is satisfiable.
- $A \vdash_{\delta} \Sigma$.
- There is not $\Sigma' \subsetneq \Sigma$ such that $A \vdash_{\delta} \Sigma'$.

Given a finite set of δ-clauses A, A^{δ} can be obtained in a finite number of steps, by successive applications of the δ-resolution rule, and eliminating subsumed[2] and contradictory δ-clauses.

The following is the fundamental theorem of the δ-resolution calculus as it provides the right way for obtaining all abductive solutions by means of a δ-resolution process.

Theorem 12 (Fundamental theorem). *For a given abductive problem* $(\{\theta_1,\ldots,\theta_n\},\phi)$*, if* N_{Θ} *and* O *are respectively the* δ*-clausal forms of* $\neg(\theta_1 \wedge \ldots \wedge \theta_n)$ *and* ϕ*, then*

$$Abd(\Theta,\phi) = \left(N_{\Theta}^{\delta} \cup O^{\delta}\right)^{\delta} - \left(N_{\Theta}^{\delta} \cup O^{\delta}\right)$$

2.3 An abductive process

By using only δ-resolution operations, an abductive process can be defined, as it is implicit in Theorem 12. Given $\Theta = \{\theta_1,\ldots,\theta_n\}$ and ϕ, it follows four steps to determine whether (Θ,ϕ) is an abductive problem and, in the affirmative case, to produce all of its abductive solutions:

Step 1: Theory Analysis. Let N_{Θ} be the δ-clausal form of $\neg(\theta_1 \wedge \ldots \wedge \theta_n)$. Then:

- If N_{Θ} does not contain any satisfiable δ-clause, then Θ is universally valid, and the process stops, because in case (Θ,ϕ) is an abductive problem it cannot have abductive solutions in the sense of definition 2.

[2]The δ-clause Σ is *subsumed* by Σ' iff $\Sigma' \subsetneq \Sigma$.

- Else, N_{Θ}^{δ} is obtained, and:
  - If $\Diamond \in N_{\Theta}^{\delta}$, then Θ is not satisfiable, and the process stops, because (Θ, ϕ) cannot be an abductive problem.
  - Else,

Step 2: Observation Analysis. Let O be the δ-clausal form of ϕ. Then:

- If O does not contain any satisfiable δ-clause, then ϕ is not satisfiable, and the process stops, because (Θ, ϕ) cannot be an abductive problem.
- Else, O^{δ} is obtained, and:
  - If $\Diamond \in O^{\delta}$, then ϕ is universally valid, and the process stops (as $\Theta \models \phi$, (Θ, ϕ) is not an abductive problem).
  - Else,

Step 3: Refutation Search. If for every δ-clause $\Sigma \in O^{\delta}$ there is a $\Sigma' \subseteq \Sigma$ such that $\Sigma' \in N_{\Theta}^{\delta}$, then $\Theta \models \neg\phi$, and the process stops because the observation refutes the theory. Else,

Step 4: Explanations Search. From N_{Θ}^{δ} and O^{δ}, $(N_{\Theta}^{\delta} \cup O^{\delta})$ and then $(N_{\Theta}^{\delta} \cup O^{\delta})^{\delta}$ are obtained. Then,

- If $\Diamond \in (N_{\Theta}^{\delta} \cup O^{\delta})^{\delta}$, then $\Theta \models \phi$ and the process stops.
- Else, (Θ, ϕ) is an abductive problem. The process returns:

$$\mathcal{A}bd(\Theta, \phi) = \left(N_{\Theta}^{\delta} \cup O^{\delta}\right)^{\delta} - \left(N_{\Theta}^{\delta} \cup O^{\delta}\right)$$

Is there a *logic of abduction*? This is a recurrent question with a difficult answer. Abductive reasoning has a double character. It is a product, but also a process. Moreover, the process to obtain an explanation is maybe more interesting than the explanation itself. So, the answer cannot be affirmative if there is not something like an *abductive logic* which integrates abductive process and product. As we argued, traditional logic-based approaches obtain abductive products which fulfil definitions 1 and 2, but their processes can hardly be considered abductive, because of the abuse of deduction and *reductio ad absurdum*.

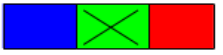
However, δ-resolution can be considered an abductive logic. Not only its products are correct (theorem 12), but also it is possible to define an abductive process which proceeds only by δ-resolution operations, as we have just shown. The steps of this process can be connected with some ideas coming from the Philosophy of Science. As we show in the next section, it is possible to visualize this process in a

way that makes it very intuitive. So δ-resolution agrees with common arguments for abductive logic.

3. Abductive diagrams

Peirce thinks that diagrammatic elements are fundamental in cognition in general and creativity in particular. So, it makes sense to look for a diagrammatic approach to abduction. In this section, we partly follow the ideas in (San Ginés, 2011). She makes an original representation of propositional logic that allows her to visualize some inference rules. The system has an usability problem that makes difficult reasoning within a two dimensional sheet of paper: it requires a temporal dimension to represent material implication by means of colour-changing squares. But if we simplify the notation and change a little the semantics of the pictures, we can take the approach as a good representation for a sound and complete abductive calculus. In the original formulation, there is a pool of formulas that is interpreted as a conjunction. Now, we take the pool as a δ-clausal form, so its elements (δ-clauses) are interpreted in a disjunction.

First, to represent some proposition we use a colour square. The colour is always the same for a given proposition. If the square is crossed out, it represents the negation of the proposition. As a δ-clause is a set of literals, we join the squares that represent its literals. For example, if the blue square represents *p*, the green is *q* and the red one *r*, the following is a δ-clause that represents $p \wedge \neg q \wedge r$:

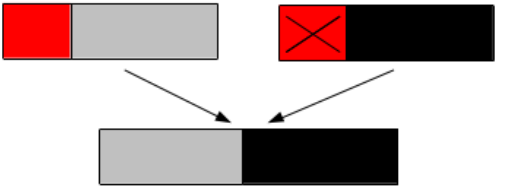


When several δ-clauses are represented in a same pool, we interpret it as a disjunction, that is, a δ-clausal form.

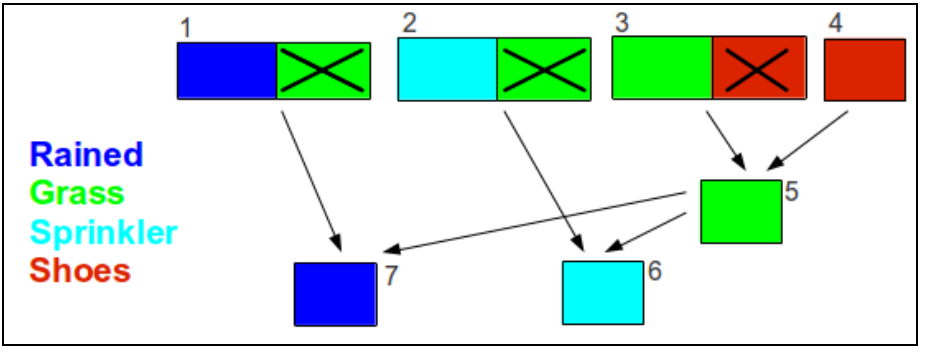
The rules of the δ-resolution calculus can be represented with these conventions. To do that, we first need a way for representing a general δ-clause. We do it with a dark rectangle, that means some irrelevant set of literals. As an example, look at the following set of δ-clauses:

The first one represents some δ-clause where a given literal is negated and the second one another δ-clause where the same literal is affirmed. The literals in the dark boxes could be related or not. Note that, as the order of the literals is not relevant, the position of the red squares is arbitrary. And also the position of the δ-clauses, of course.

With these resources we can represent the δ-resolution rule. Note that if the previous two δ-clauses are in the same pool, they constitute two different explanations for a given abductive problem: one where the red proposition is affirmed and another one where is negated. Then the δ-resolution rule allows us to propose a new possible explanation that does not use that proposition, as the following figure shows. Arrows indicate the direction of the inference. As it is an abductive inference it is the opposite direction to that of the logical consequence (look at the Soundness Theorem above).

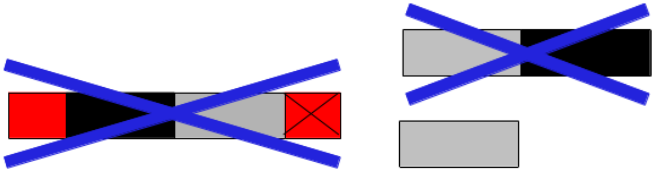


With these elements we can illustrate the δ-resolution process with an example. We start with the δ-clausal form that corresponds to the *wet shoes* example given at section 2.2. The following picture shows which colour we have chosen to represent each proposition. We have drawn explicitly a border to indicate that all δ-clauses belong to the same δ-clausal form, so they are interpreted as a disjunction and all are possible explanations for the same abductive problem.



Look that there is a natural order in the derivation of new δ-clauses. Number 6 and 7 can only be generated after 5. This corresponds to the intuitive idea that "rained last night" (number 7) or "sprinkler was on" (6) are deeper explanations for the wet shoes than just that "the grass is wet" (5), which of course is also a possible explanation, but not so elaborated. It is easy to visualize the idea of greater reasoning chains in the diagram.

An accurate representation of the abductive process in section 2.3 would require two δ-resolution processes (for the theory and the observation) that are originally separated (steps 1 and 2 of the abductive process) and then they join (steps 3 and 4). One can imagine those breeding cages where birds are originally separated at the beginning of the courtship and only later they are put together. Both stages of the process are necessary for the success. Moreover, additional rules to eliminate contradictory or subsumed δ-clauses can be represented within these diagrams:



The left picture eliminates δ-clauses that contain some literal and its complementary. In the same way, the right rule eliminates a δ-clause whenever there is another one which is a proper subset. That is, the big one becomes subsumed, as there is another simpler explanation.

With these rules, in the previous example, δ-clauses 3, 2 and 1 are eliminated, respectively, when 5, 6 and 7 are generated. The old δ-clauses become subsumed by the new ones. This represents the intuitive idea that some conflicting explanations are discarded, before solving an abductive problem, when a better hypothesis is formulated.

As we have shown, all the resources in the δ-resolution process can be represented by these diagrams. So we can use them as a sound and complete abductive calculus, which allows us to visualize many of the intuitive ideas about the formulation of hypotheses.

4. Conclusions

To conclude, let us evaluate the contributions of the δ-resolution calculus and the proposed diagrams. The four Kapitan's theses draw the objectives that any systematisation of abductive reasoning should attain. These are high objectives which operate as the horizon of abduction. So, we finish this paper revisiting them and evaluating to what extent δ-resolution satisfies them.

First, the *inferential thesis* is completely fulfilled, as δ-resolution is an inferential process, formally characterized, with important logical properties.

The *thesis of purpose* highlights the double task of abduction: the *generation* of new hypotheses and the *selection* of the best ones for further examination. Many

approaches split these processes so first generate a number of hypotheses which satisfy (3) and then select, among them, those that satisfy (4) and (5). Usually, each process is performed in a different way. However, δ-resolution integrates *generation* and *selection*, as the abductive process defined above shows.

The *comprehension thesis* is the most interesting one and, at the same time, the most difficult to satisfy. The δ-resolution calculus is far from including all the operations whereby theories are engendered. But the steps of the abductive process are related to some ideas from Philosophy of Science and Epistemology, as we have commented when showing the diagrammatic approach to abduction at section 3. Indeed, the δ-resolution rule, in its graphical representation, can be interpreted within some cognitive theories, as *Mental Models* (Johnson-Laird, 1983). Johnson-Laird's mental models can be assimilated to sets of literals. Then, if $\Sigma_1 \cup \{\lambda\}$ and $\Sigma_2 \cup \{\neg\lambda\}$ are two mental models which explain a given observation, $\Sigma_1 \cup \Sigma_2$ is another mental model, maybe a smaller and better one, which also explains the observation. Some representations of mental models agree with our diagrams. Also, we underlined in the introduction the relevance of strategic rules for abductive reasoning. That relevance is shown explicitly in the diagrammatic representation, as an intelligent ordering of the applications of the δ-resolution rule can lead to shorter paths to the best explanation.

Finally, the *autonomy thesis* requires that abduction is irreducible to deduction and induction. Abduction is frequently called retroduction or backward deduction. A duality between deduction and abduction is often suggested. In this sense δ-resolution, which is dual to a typical deductive calculus, is somehow dual to deduction. But δ-resolution is also irreducible to deduction. Classical deduction is a monotonic reasoning, while the abductive process by δ-resolution, as it fulfils definition 2, is a non-monotonic[3] inference.

[3] Details on the non-monotonicity of abduction can be found in (Aliseda, 2006).

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O problema da forma nas ciências. Ernst Cassirer e as analogias metodológicas nas ciências da cultura e na biologia

Christian Moeckel
(Universidade Humboldt de Berlim)
MoeckelC@philosophie.hu-berlin.de

1. O problema da forma nas ciências da cultura e na biologia

As seguintes considerações articulam três aspectos que caracterizam a actividade filosófica de Ernst Cassirer. O primeiro é o conceito de forma que, sendo o ponto central na sua obra filosófica, substitui o relevante conceito de função – isto é, de funcionalidade – utilizado pela matemática. O segundo aspecto abarca a tentativa de Cassirer de aliar questões epistemológicas e teórico-culturais a determinados problemas metodológicos das ciências, com o propósito fundamental de poder fundar tanto a unidade como a especificidade das ciências da natureza e da cultura. O terceiro, por último, diz respeito ao interesse pela biologia como uma ciência que, a partir do século XIX, adquire um crescente papel paradigmático; interesse esse que percorre toda a obra do filósofo.

Para a apresentação da sua teoria da linguagem como uma forma simbólica da cultura, como uma realização simbólica, Cassirer escolhe por vezes o procedimento metódico dos factos e conhecimentos da patologia da linguagem como indício empírico das suas principais teses filosóficas. A ideia básica deste procedimento consiste, como Cassirer revelou em 1927 numa conferência sobre a Linguagem, o Pensamento e a Percepção, na convicção de que as perturbações patológicas da linguagem não só indicam uma perturbação da faculdade simbólica, mas também o seu estudo e a sua ponderação podem esclarecer a própria função simbólica da linguagem e a sua conexão com "uma função basilar comum ao espírito [...], que é designada por nós como a função simbólica enquanto tal." Para uma tal aclaração, assim acrescenta Cassirer, "não só a patologia da linguagem, mas também a biologia

e a psicologia do desenvolvimento põem à disposição vários materiais", ou seja, contêm "muitas indicações".[1] Este procedimento metódico, aqui indicado, com o objectivo de elucidar e sublinhar o vínculo à patologia da linguagem, à psicologia do desenvolvimento e à biologia, é usado por Cassirer também nos casos relativos às questões da filosofia da cultura e das ciências da cultura, formando, no entanto, a biologia e a doutrina biológica da evolução o campo de referência mais explorado. Isso manifesta-se, em primeiro lugar, no texto das lições apresentadas em Gotemburgo (1939) sobre *Os problemas da filosofia da cultura* e nos seus anexos (folhas sobre a objectivação) – patentes no quinto volume das obras póstumas (ECN5)[2] –, bem como nos esboços preparados para o manuscrito das lições, os quais foram, há pouco tempo, publicados no quarto volume das obras póstumas (ECN4).[3] Muitos dos conhecimentos provenientes destas lições e destes esboços farão parte dos estudos reunidos na obra *Da lógica das ciências da cultura* (LKW).[4]

Com o escopo de fundar as ciências da cultura segundo uma autonomia em relação às ciências da natureza nos conceitos de forma e de estilo, nos conceitos de vida e de fenómenos da expressão,[5] Cassirer procura nos textos já referidos – e tendo sempre como objectivo, nessa exacta medida, delinear conceitos específicos e autónomos para as ciências da cultura – estabelecer analogias metodológicas (semelhanças) relativas ao problema da forma entre as ciências da cultura e a biologia. Ele faz questão de afirmar lapidarmente que nas ciências da cultura "nunca se pode colocar o ›problema da causalidade‹ [...] desprendido do problema da forma", pois o primeiro só poderá ser ultrapassado "única e exclusivamente pelo retorno ao problema da forma". Além disso, Cassirer põe entre parêntesis o seguinte: "o mesmo é válido para a biologia, pois, nela, a ›entelequia‹ continua a ser considerada como a verdadeira ›causa actuante‹".[6] Que esta explicação não encerra a condição de tese

[1] Ernst Cassirer: Über Sprache, Denken und Wahrnehmung. (1927) In: Symbolische Prägnanz, Ausdrucksphänomen und ›Wiener Kreis‹. Hrsg. von Christian Möckel. (Nachgelassene Manuskripte und Texte. Hrsg. von Klaus Christian Köhnke, John M. Krois † und Oswald Schwemmer. Bd. 4.) Hamburg 2011 (ECN4), 310.

[2] Ernst Cassirer: Probleme der Kulturphilosophie. (1939) In: Ders.: Kulturphilosophie. Vorlesungen und Vorträge 1929-1941. Hrsg. von Rüdiger Kramme †. (Nachgelassene Manuskripte und Texte. Bd. 5) Hamburg 2004 (ECN5), 29-200.

[3] Ernst Cassirer: Ausdrucksphänomen und ›Wiener Kreis‹ (1935/36). In: ECN4, 151-215.

[4] Ernst Cassirer: Zur Logik der Kulturwissenschaften. Fünf Studien. (1942) In: Ders.: Aufsätze und kleine Schriften 1941-1946. In: Gesammelte Werke. Hamburger Ausgabe. Hrsg. von Birgit Recki. Band 24. Hamburg 2007 (ECW24), 357-486.

[5] Christian Möckel: Die Kulturwissenschaften und ihr ›Lebensgrund‹. Zu Ernst Cassirers Beitrag zur Theorie der Kulturwissenschaften. In: Reto Luzius Fetz / Sebastian Ullrich (Hg.): Lebendige Form. Zur Metaphysik des Symbolischen in Ernst Cassirers »Nachgelassenen Manuskripten und Texten«. (Cassirer-Forschungen Bd. 13), ,Hamburg 2008, 179-195.

[6] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 190f.

fortuita, assim é provado pelas múltiplas referências – reunidas e classificadas, em seguida, por nós – às analogias nas ciências da cultura e na ciência descritiva da biologia. Quase todas as analogias encontram-se no já mencionado curso das lições de Gotemburgo, de 1939, e outras podem igualmente ser encontradas no quarto volume da obra *O problema do conhecimento*.[7] Em relação ao problema da forma, Cassirer coloca, por conseguinte, dentro da sua concepção das ›Duas Culturas‹ na ciência (Oswald Schwemmer) e apesar de toda a distinção e delimitação, as ciências da cultura e as ciências da natureza (a Biologia) numa relação de proximidade muito mais estreita do que possa parecer à primeira vista.

Contudo, a questão das similitudes científico-metódicas, nomeadamente epistemológicas, e a questão das particularidades diferenciadas entre a matemática (ciências matemáticas), as ciências da vida natural (biologia) e as ciências da vida espiritual são já abordadas por Cassirer quer na sua obra *Leibniz* (1902), quer nos dois primeiros volumes da obra *O problema do conhecimento* (1906/07). Tal abordagem é, neste sentido, animada e influenciada pelo conceito de organismo na filosofia de Leibniz, na *Crítica da Faculdade do Juízo* e nos escritos científicos de Goethe acerca da natureza. Cassirer assinala de forma positiva na filosofia de Leibniz a transição metodicamente eficaz – executada com o auxílio do conceito de mónada – da mecânica abstracta para a "área da concepção orgânica da natureza". Esta transição, formulada segundo uma lógica monista, quer dizer, sem o apoio de uma ›força de vida‹ indiferente ao sistema conceptual, mas antes pela introdução da contingência irracional no sistema, culmina num conceito de organismo vivo que "preserva ainda hoje a sua acuidade científica".[8] No entanto, ele vê no facto de, na filosofia de Aristóteles, a lógica perder para a biologia a função superior do conhecimento, um retrocesso face ao idealismo lógico de Platão, o qual, em mil e novecentos e dois, ainda exerce alguma autoridade sobre o marburguiano Cassirer.

A colocação desta questão também leva Cassirer ao problema se e até que ponto os instrumentos epistemológico-metodológicos de um grupo de ciências podem ser transferidos e aplicados, de forma fundamentada e sem simplificações, a outras actividades científicas. Se é verdade que Cassirer não gosta de adoptar sem qualquer restrição a aplicação, propagada por Goethe, do princípio da vida à natureza não-orgânica, também é verdade, por outro lado, que ele apõe conscientemente o conceito

[7] Ernst Cassirer: Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit. IV. Bd.: Von Hegels Tod bis zur Gegenwart (1832.1932). In: Ders.: Gesammelte Werke. Hamburger Ausgabe. Bd. 5). Hamburg 2000 (ECW5).
[8] Ernst Cassirer: Leibniz' System in seinen wissenschaftlichen Grundlagen. (1902) In: Ders.: Gesammelte Werke. Hamburger Ausgabe. Bd. 1). Hamburg 1998 (ECW1), 9. Kap. Das Problem des Individuums in der Biologie - Der Organismus, 358-378, hier: 367.

de organismo, segundo a tradição kantiana, à caracterização da razão.[9] O problema metodológico da extensibilidade do princípio orgânico da vida ao conhecimento – e, consequentemente, à razão, que passa, deste modo, a ser compreendida como uma "unidade orgânica" – é mencionado inúmeras vezes por Cassirer na sua explanação do idealismo alemão pós-kantiano.[10] O conceito de organismo, desenvolvido pelo romantismo na viragem do século XVIII para o século XIX e que, ao estar baseado no conceito geral de forma, permite um paralelismo entre vida e espírito, conserva, no século XIX, uma posição central no sistema das ciências do espírito. Contudo, como Cassirer afirma em mil e novecentos e vinte e três, no que diz respeito ao seu sentido e à sua orientação, tal conceito sofre uma transformação profunda, especialmente devido à primazia que o "conceito biológico de desenvolvimento vai auferir na ciência moderna da natureza".[11] O conceito de organismo vivo, que começou por ser um conceito estritamente filosófico, acaba por se transformar, com a biologia do século XIX, num conceito propriamente científico.

Em seguida devem ser apresentados os resultados da análise das teses de Cassirer sobre as semelhanças e analogias entre as ciências da cultura e as ciências da natureza. Em primeiro lugar, devem ser expressas e tematizadas as analogias da forma mais relevantes destacadas por Cassirer. Com isso, pretende-se também saber até que ponto o problema da forma é colocado de forma distinta em ambas as áreas de objectos, isto é, saber qual é o traço singular do problema da forma na biologia. Em segundo lugar, deve-se esboçar, considerando as explicações anteriores, a concepção cassireriana da biologia como uma ciência moderna da natureza. Depois segue-se a tentativa de discutir e explicar alguns aspectos – e as questões a eles relativas – das analogias, mais concretamente as semelhanças do problema da forma nas ciências da cultura e na biologia científica, bem como alguns aspectos da concepção de biologia que está em jogo. Isso implica, entre outras coisas, o significado da afirmação já aqui citada, nomeadamente aquela que se refere à relação metódica entre ambas – "o mesmo é válido para a biologia" –, e que sugere, por sua vez, uma certa ordem hierárquica das ciências. Além disso, temos de mencionar a tese de Cassirer – à primeira vista incómoda – segundo a qual não só as ciências da cultura mas sim todas as ciências modernas da natureza, como a física, a psicologia e a biologia, possuem o

[9] Ernst Cassirer: Kants Leben und Lehre. In: Ders.: Gesammelte Werke. Hamburger Ausgabe. Bd. 8). Hamburg 2001 (ECW8), 346; Ernst Cassirer: Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit. (1920) Bd. III. In: Gesammelte Werke. Hamburger Ausgabe. Bd. 4). Hamburg 2000 (ECW4,) 296, 303.
[10] Ernst Cassirer: Das Erkenntnisproblem. Bd. III. In: ECW4, 153, 230, 363.
[11] Ernst Cassirer: Philosophie der symbolischen Formen. Erster Teil: Die Sprache. (1923) In: Gesammelte Werke. Hamburger Ausgabe. Bd. 11. Hamburg 2001. (ECW11), 107.

seu próprio problema da forma.[12] Parece que esta ideia está em condições de superar não só a distinção fundamental entre as ciências da causalidade e as ciências da forma (*Gestalt*)[13], mas parece também que a biologia perde de novo, no seio das ciências da natureza, o seu proeminente carácter particular, procedente do seu objecto, da vida, do organismo vivo, da auto-organização orgânica.

2. Sete analogias da forma e algumas diferenças

Uma explicação e justificação da possibilidade das analogias da forma, especialmente aquelas entre a biologia e as ciências da cultura, são dadas por Cassirer mais tarde, recorrendo o filósofo alemão ao axioma da hierarquia das áreas de objectos científicas, que pressupõe a ideia de que os níveis mais elevados possibilitam as leis a as estruturas dos níveis mais baixos, acrescentando os primeiros, para além disso, outras dimensões peculiares aos segundos.[14] Este facto parece legitimar, até a um certo ponto, a transferência dos conceitos característicos dos níveis mais baixos para os níveis mais elevados. A distinção feita por Cassirer entre o conceito ›geral‹ de forma e o conceito ›particular‹ da forma de uma determinada área de objectos, entre o princípio da formação (princípio da superioridade e da subordinação, entre outros) e os conceitos particulares da forma de outras áreas específicas, parece perfazer tal legitimidade.[15] Contudo, esta concepção de uma hierarquia – mormente da sua emergência – submete, na visão de Cassirer, a enumeração das analogias, bem como a própria aplicabilidade dos conceitos de forma da biologia aos conceitos de forma da cultura, a um procedimento metódico do ›*como se*‹ (›*als-ob*‹); segundo esta concepção, "as ›formas‹ da cultura são ›organismos‹, mas apenas no sentido de um *como se*".[16]

As indicações explícitas das analogias da forma, contidas nas lições sobre a filosofia da cultura de 1939 e na obra *O problema do conhecimento* (vol. 4), escrita mais ou menos na mesma altura, podem ser ordenadas em sete aspectos: a compreensão e a descrição; a permanência e a capacidade de transformação (metamorfose); o carácter individual da pregnância; a morfologia e a génese; a síntese

[12] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24: 454f.
[13] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 92, 100.
[14] Ebd., 63; Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 286. Deveras interessante é o facto de esta ideia cassireriana estar também presente na Ontologia Nova de Nicolai Hartmann. Vide, por exemplo, Nicolai Hartmann: Neue Wege der Ontologie. Teildruck aus: Systematische Philosophie. (1942) 2ª edição. Stuttgart 1949, 231ff.
[15] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 58f.
[16] Ebd., 127.

do múltiplo; a indeduzibilidade, por exemplo, do problema da causalidade; e, por último, a linguagem emocional. Desta tentativa de submeter todas as indicações a uma ordenação, resultam os seguintes aspectos que permitem, ao mesmo tempo, vislumbrar a perspectiva como Cassirer vê a especificidade do conceito de forma na biologia em comparação com o conceito de forma nas ciências da cultura:

Compreensão e descrição: O que se compreende, em vez de se explicar através de uma redução a leis quantitativas, é, em primeiro lugar, o entendimento filosófico-cultural que, pressupondo ou implicando o conceito de forma, se apresenta como um procedimento fundado na descrição de estruturas e totalidades. Se a percepção expressiva, enquanto pedra de toque da particularidade dos objectos culturais, exige o conceito de forma para se afirmar como fundamento lógico da compreensão, então "o mundo dos objectos da cultura é, nesta perspectiva lógica, paralelo ao dos conceitos biológicos."[17] Também o compreender biológico se cumpre já como um descrever das formas e estruturas. Nestes casos, a descrição do organismo singular (isto é, do órgão singular) deve pressupor o geral, o todo, a estrutura integral da forma animal correspondente.[18] "O conhecimento de uma forma singular [da vida – C. M.] pressupõe [...] o conhecimento do mundo das formas na sua totalidade".[19] Por outras palavras, a forma genérica – quer dizer, o "plano de construção", a "forma de vida" – representa as leis gerais da forma que determinam rigorosamente, no reino animal, a forma do organismo singular.[20]

Permanência e capacidade de transformação: "O *Analogon*, tendo levado, sempre de novo, a uma comparação dos objectos da cultura com os objectos da natureza orgânica", é visto por Cassirer, em primeiro lugar, segundo a característica da "estabilidade/consistência" (*Bestand*) dos fenómenos da cultura, ou, por outras palavras, na particularidade destes se encontrarem em constante mudança e, apesar disso, permanecerem intactos, o que se manifesta nos seus sucessivos efeitos e transformações. "Esta capacidade de permanência da forma e do desenvolvimento da forma" é, segundo Cassirer, uma capacidade inerente a ambas as classes de objectos.[21] Se Cassirer sublinha esta particularidade comum, então ele possui, como principal argumento, o facto de a capacidade da forma para a permanência e para a transformação, designada por ele como "a sua ›vida‹ específica", continuar a ser conservada nas configurações tanto culturais como biológicas, apesar de todas as propensões desfavoráveis. O mesmo é dizer, a metamorfose, isto é, a ideia de

[17] Ebd., 101f.
[18] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 151.
[19] Ebd., 152.
[20] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 135.
[21] Ebd., 127.

transformação da forma e de desenvolvimento de um arquétipo, revela, por conseguinte, "a afinidade [das configurações culturais – C. M.] com o mundo do orgânico".[22]

Aquilo que aqui está subentendido é, com efeito, a capacidade de "deixar brotar de si" uma plenitude de novas configurações; configurações estas que se afectam mutuamente. As ›formas de vida‹ orgânicas, ou as ›naturezas orgânicas‹, não constituem, deste modo, "relações basilares estáveis", mas antes podem estar associadas a um determinado modelo ou ›protótipo‹;[23] elas aparecem, para a "intuição empírica", como que "formadas por um protótipo", que, apesar da variabilidade e permeabilidade das suas partes, se transforma e desenvolve como um todo indivisível.[24] Neste sentido, as configurações (*Gestalten*) da cultura comportam-se como configurações orgânicas, sem, contudo, se reduzirem umas às outras, mais precisamente: elas comportam-se ›como se‹ fossem idênticas. A suposição de Cassirer de que os conceitos de forma da teoria biológica da metamorfose conservam a sua validade metódica a partir dos níveis objectivos superiores da ciência – pelo menos, a partir dos das ciências da cultura e da história –, é sustentada igualmente por uma anotação sobre Jakob Burckhardt. Este vislumbrava, nos mundos plásticos e figurativos da arte, da poesia, da linguagem etc., uma metamorfose, um ›devir (*Werden*) no ser‹, uma "transformação figurativa", os quais devem, por sua vez, ser transmitidos por qualquer historiografia da cultura.[25]

Carácter individual da pregnância: A mutabilidade metamorfósica**,** ou a ›pregnância‹ da forma, explicada por Cassirer nas formas espírito-culturais, indica que "as formas ›estáveis‹ gerais [...] são sempre remetidas para este âmago [...] do configurar, do transformar",[26] como já se pode deduzir das formas biológicas. Neste sentido, a forma biológica é também uma "›forma de pregnância‹ [...], que se desenvolve vigorosamente"[27], é apenas uma forma única, que "se transforma e desenvolve permanentemente".[28] Aqui, contudo, Cassirer também indica as diferenças principais relativas ao carácter individual da transformação da forma por parte do homem que actua culturalmente. Na natureza orgânica, no reino dos animais, não se pode falar de um "trabalho dos indivíduos [...] que se exprimem através destas formas [de pregnância – C. M.]",[29] e que, pelo uso e repetição, uma nova vida ganham.[30] "A

[22] Ebd., 127.
[23] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 160f.
[24] Ebd., 167.
[25] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 323.
[26] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 139.
[27] Ebd., 128.
[28] Ebd., 134.
[29] Ebd., 134.

regeneração por apropriação individual e por formação individual" existe exclusivamente para e através do homem da cultura,[31] já que este processo implica a transmissão hereditária das características adquiridas,[32] o que significa, pelo menos nos tempos de Cassirer, uma barreira intransponível para o conceito de forma da biologia.[33] O animal, como exemplar da espécie, constava unicamente nas leis da forma gerais, que seriam prescritas pela espécie de modo rígido e obrigatório.

Morfologia e génese: Cassirer descobre mais uma analogia metódica na dimensão morfológica de ambas as classes de objectos. O filósofo alemão elege a morfologia como a teoria da ›génese ideal‹ da forma intuitiva (Goethe), distinguindo-a, nessa medida, da explicação causal ligada à sucessão histórica dos fenómenos da vida.[34] A ›génese ideal‹ da ›forma‹ morfológica, que o observador encontra na biologia moderna e que não põe em jogo a "questão da sucessão histórica dos fenómenos da vida", possui a sua correspondência no ›ponto de vista genético‹.[35] Cassirer tinha reclamado este ponto de vista, perante os fenómenos de cultura, para o problema de forma – como se pode ver, por exemplo, nas lições sobre os problemas fundamentais da filosofia da cultura (1929)[36] –, porque todo o fenómeno cultural devia ser compreendido como um ›devir para o sentido‹.[37] Neste aspecto, esta concepção ou interpretação despega-se do conceito de forma aristotélico, que destina as formas do devir natural aos "fundamentos imutáveis do devir".[38]

Os conceitos morfológicos da forma, como eles são conhecidos e usados nas ciências da cultura, parecem ser "indispensáveis já na biologia".[39] Contudo, também a analogia da morfologia tem de tomar em conta as diferenças entre a história natural e a história cultural. A morfologia, mais precisamente a forma morfológica da espécie, é compreendida pelos vistos por Cassirer como a teoria da construção funcional, ou seja, das funções, da estruturação do organismo típico da espécie a partir das funções

[30] Ebd., 131.
[31] Ebd., 135.
[32] Ebd., 140.
[33] Pesquisas biológicas actuais parecem, contudo, dar provas de uma transmissão hereditária de determinadas experiências de vida, até mesmo entre as plantas. Assim, as biólogas Galloway (University of Virginia) e Etterson (University of Minnesota) provaram, em experiências de laboratório e de campo em torno das campânulas americanas, que a história familiar das plantas, transmitida através da informação contida nas sementes, determina a duração de vida da futura geração de plantas. Laura F. Galloway / Julie R. Etterson: Transgenerational Plasticity Is Adaptive in the Wild. In: Science. Vol. 318. No. 5853, 1134-1136.
[34] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECW5, 148, 173.
[35] Ebd., 172f.
[36] Ernst Cassirer: Grundprobleme der Kulturphilosophie. (1929) In: ECN5, 21f.
[37] Ebd., 12.
[38] Ebd., 21f.
[39] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 157.

›incorporadas‹ nos órgãos. As "leis [morfológicas – C. M.] das formas gerais" da espécie animal determinam o "›plano de construção‹ corporal", isto é, os órgãos e as funções de cada animal ligam-no com o seu campo perceptual (*Merkwelt*) e o seu meio ambiente (*Umwelt*) correspondentes. O animal singular exprime, como exemplar da sua espécie, a forma morfológica da espécie "e o "›plano de construção‹ prescrito por esta forma"; e, enquanto exemplar singular da sua espécie, ele não pode nem mudar, nem ampliar ou abandonar o seu plano de construção.[40] Quanto à sua "estrutura morfológico-física", quer dizer, quanto à sua "forma física", o indivíduo humano singular também tira daí a estrutura que já está incluída nela, também tira daí a imutabilidade através do particular. Esta rigidez e determinação da estrutura morfológico-física é válida, como Cassirer crê, igualmente para o "mundo [cultural] das formas [simbólicas – C. M.]", as quais não são meramente engendradas pelo indivíduo, mas este está já imerso nelas.[41] Porém, o homem, o indivíduo, muda estas formas espirituais gerais através do uso. Isso é válido, em primeiro lugar, para os actos dos ›grandes‹ indivíduos: "O grande político é aquele que é capaz de modificar igualmente a forma do estado".[42] O grande indivíduo surge, deste modo, nunca como um simples caso da espécie humana, porque ele anima, transforma e regenera a forma cultural da espécie, bem como as suas formas de cultura particulares. A forma cultural, assim como as formas linguísticas ou as formas artísticas etc., é interpretada por Cassirer não como uma forma em si, como forma imóvel, mas antes como uma forma da qual nós podemos mostrar ou representar um protótipo, um arqui-fenómeno, embora nós cheguemos sempre a conhecê-la exclusivamente como uma forma concreta, modificada, regenerada e enriquecida.

Contudo, uma certa modificação da forma morfológica deve existir também no mundo orgânico; caso contrário, Cassirer não poderia falar de uma analogia com o mundo da cultura. Referindo-se à doutrina de Lamarck, nas lições sobre os problemas da filosofia da cultura, o filósofo alemão diz-nos que a transformação da forma através do uso "possui o seu *analogon* já no mundo físico [isto é, orgânico – C. M.], onde é aí propriamente a função que muda, transforma e desenvolve a forma morfológica" da espécie ou do género.[43] Como já se tinha sugerido anteriormente sobre a transformação da forma espiritual geral através do seu uso, aqui encontramos a mesma relação analógica que une as funções simbólicas da actividade cultural às funções orgânicas do organismo biológico. Na minha opinião, esta analogia deve ser compreendida pelo facto de o "animal singular exercitar uma certa competência" para

[40] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 135.
[41] Ebd., 135f.
[42] Ebd., 191.
[43] Ebd., 136.

"transformar, sob condições desfavoráveis, o órgão [da função – C. M.] através dessa prática".[44] Todavia, a capacidade de uma transmissão hereditária destas mudanças morfológicas do órgão aos fenómenos subsequentes – isto é, ao género – não gerava qualquer consenso absoluto no discurso da biologia.

A reconciliação teórica necessária da "estabilidade inalterável da forma biológica" com a "mutabilidade, com o desenvolvimento da forma" distingue-se aparentemente da concordância correspondente no mundo da cultura, mais concretamente nos domínios espirituais em que, devido ao dinamismo da função, ocorre uma transformação do órgão, levando este último, por sua vez, a assumir novas funções.[45] Do facto de a "relação entre movimento e repouso [...], que impera na natureza orgânica", se diferenciar "da relação que nós encontramos nas configurações culturais", é mencionado também por Cassirer na sua obra *Lógica das ciências da cultura*.[46] É aí, mais uma vez, que ele sublinha que, na forma natural (*Naturform*), as modificações operadas pelos indivíduos não entram na vida do género, como tende a suceder, pelo contrário, na forma cultural (*Kulturform*): "O ›espírito‹ tem [aqui – C. M.] realizado tudo o que era vedado à ›vida‹."[47] Além disso, ou mesmo por causa disso, a humanidade, opondo-se à animalidade, havia criado "em todas as suas formas de cultura [...] um novo [segundo – C. M.] corpo, que a todos pertence".[48]

Síntese do múltiplo: Referindo-se ao modo peculiar como os conceitos de estilo e de forma são aplicados na história da arte – e distinguindo-os dos conceitos de lei –, à maneira como ambos tornam "visível um geral [da forma – C. M.] no particular",[49] como eles originam, de uma maneira específica, a ›unidade sintética do múltiplo‹,[50] Cassirer estabelece mais uma afinidade, estreitamente ligada com aquelas que já foram indicadas, entre ambas as áreas de criação de conceitos e do conhecimento científico. A relação "entre o geral e o particular" não deve ser interpretada, como Goethe tinha mostrado, "[como] a relação de uma subsunção lógica", mas sim como "a relação de representação ideal ou ›simbólica‹",[51] como uma relação que é realizada e mediada pelos conceitos de forma (*Gestalt*). "Já a biologia teórica", especialmente o vitalismo biológico, conhecia, "além dos conceitos de lei", os conceitos de forma que revelavam uma relação característica entre o geral e o particular.[52] Com isso, a

[44] Ebd., 136.
[45] Ebd., 137.
[46] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 484.
[47] Ebd., 485.
[48] Ebd., 486.
[49] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 133.
[50] Ebd., 165.
[51] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5:, 169.
[52] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 133.

›forma‹ (*Gestalt*) na investigação cultural é algo tão objectivo como a ›lei‹ (*Gesetz*) no conhecimento da natureza. A forma é "uma ›forma‹ igual às formas da natureza, [ela] possui uma determinação e uma estrutura objectivas".[53] Noutro trecho das lições também encontramos a seguinte asserção: "A maneira, o modo através do qual a ›unidade sintética do múltiplo‹ é consumada" parece ser, "na Física, o instrumento da ›lei‹, na Biologia e nas ciências da cultura, [pelo contrário – C. M.], o [instrumento – C. M.] da forma (›*Gestalt*‹)".[54] Porque "já na Biologia" a forma, e, por isso, a configuração, é, além de lei, um conceito *sui generis*. Resta indicar ainda uma pequena, mas importante, diferença de significado de ambas as citações relativas ao conceito de lei: no primeiro caso, a ›lei‹ e a ›forma‹ (isto é, a ›forma natural‹) não se encontram aparentemente em oposição; no segundo caso, porém, há uma oposição explícita.

Indeduzibilidade e problema da causalidade: Os problemas da forma – relativos aos conceitos de forma –, nas ciências da cultura e na biologia, têm, para Cassirer, o carácter de problemas puramente metódicos ou conceptuais; não sendo, com efeito, problemas de cariz metafísico,[55] eles devem ser compreendidos como irreduzíveis e indeduzivéis arqui-fenómenos, porque não podem ser explicados por causas estranhas à forma, ou seja, por meras causas materiais. Darwin, por exemplo, havia considerado a forma – embora ele sublinhasse a importância do conceito de forma para o orgânico – como um "simples produto contingente", como um "produto das forças puramente mecânicas", e tinha, desta maneira, negado a sua autonomia e indeduzibilidade.[56] Cassirer vê esta autonomia, pelo contrário, presente no conceito de ›plano de construção‹, introduzido por Uexküll, e que vai ser interpretado por ele como conceito de estrutura e de forma. A defesa desta autonomia é perceptível, em primeiro lugar, quando Uexküll fala de uma "›relação imaterial das partes materialmente dadas de um corpo‹"[57] ou de "uma ordem não-material, de uma regra da vida [...] que dá primeiramente à matéria a sua estrutura (*Gefüge*)".[58] Porém, embora Cassirer exclua, quer nas ciências da cultura quer na biologia, a pesquisa causal da origem do conceito de totalidade ou de estrutura, ele assevera que ambas as ciências não podem existir sem o problema da causalidade, sendo que, nas ciências da cultura, e como aliás já tínhamos visto, este "só pode ser resolvido pelo regresso ao problema da forma (algo que é igualmente válido para a Biologia [...])".[59] Contudo,

[53] Ebd., 143.
[54] Ebd., 165.
[55] Ebd., 95.
[56] Ebd., 61f.
[57] Ebd., 162.
[58] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 235.
[59] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 190f.

aqui coloca-se a questão se Cassirer fala de forma inequívoca de causalidade, ou se ele pretende, desta vez, aludir a uma causalidade produzida por cientistas [da natureza] sob condições laboratoriais, ou, então, a uma causalidade definida como problema estrutural, o que seria aparentemente válido para a asserção anteriormente citada.

Linguagem emocional: Por fim, Cassirer fala, na sua obra *Essay on Man*, e ainda noutro sentido, das analogias entre o reino cultural e o reino animal, e, deste modo, indirectamente das analogias entre as ciências da cultura e a biologia, nomeadamente quando distingue os "modes of symbolic behavior".[60] Assim, e apesar de todas as diferenças, podem-se encontrar "analogies [...] to emotional language" do ser humano já "in the animal world".[61] Até o animal goza de uma linguagem subjectiva emocional, embora não conheça a linguagem objectiva, afirmativa e proposicional de que dispõe exclusivamente o ser humano.[62] O animal dispõe de uma linguagem de *sinais*, mas não de uma linguagem de *símbolos*, que é reservada ao homem,[63] pois o primeiro está condicionado pelos "limits of his biological needs and his practical interests",[64] limites esses que o segundo ultrapassa através da simbólica criada por ele mesmo.[65]

3. A Biologia como ciência moderna da forma

A Biologia, como salienta Cassirer, sofreu, nos séculos XIX e XX, um desenvolvimento considerável. Um dos pressupostos desta prosperidade consiste na concepção da autonomia ou particularidade do seu objecto, do organismo e da vida orgânica; concepção essa que se incrementa a partir da obra kantiana *A crítica da faculdade de juízo* (1790). Tal concepção atribuiu ao organismo e à vida orgânica características teleológicas, assim como distinguiu ambos principalmente da natureza inorgânica e da sua ciência fundamental, isto é, da física. Com isso, o monismo do método positivista, como era defendido, embora de forma diferente, por Aristóteles e Descartes, foi posto em dúvida.[66] Cassirer crê que, no seu tempo, este processo

[60] Ernst Cassirer: An Essay on Man. (1944) In: ECW23, 32.
[61] Ebd., 34.
[62] Ebd., 34f.
[63] Ebd., 36f.
[64] Ebd., 47.
[65] Ernst Cassirer: Ziele und Wege der Wirklichkeitserkenntnis. Hrsg. von Klaus Christin Köhnke und John Michael Krois. (Nachgelassene Manuskripte und Texte. Bd. 2) Hamburg 1999 (ECN2), 86.
[66] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 31, 33; Ders.: Das Erkenntnisproblem Bd. IV. In: ECW 5, 137f.

repetia-se continuadamente – e sem cessar – nas ciências da cultura. Ele tende sempre a dar relevância como, na biologia e nas ciências da cultura, se estabelece uma metodologia particular que assume uma distância em relação aos métodos predominantemente científico-matemáticos, nomeadamente os métodos científico-causais aplicados pela física e utilizados também por biólogos, com convicções monísticas e mecânico-materialistas, para a explicação do próprio objecto (o organismo).

A exploração dos escritos e textos de Cassirer, já mencionados no parágrafo anterior, permite-nos salientar, mais uma vez, pelo menos sete aspectos desta renovação metodológica e apresentá-los sob a seguinte ordem: Morfologia e metamorfose, teoria darwinista da evolução, animismo/vitalismo, método descritivo, método analítico-causal, ciência como problema da forma, conceito de forma e conceito de lei.

Morfologia e metamorfose: Por um lado, no desenvolvimento da biologia e do seu método a partir de Kant, a classificação do conjunto da vida (Linné) é substituído já pela transferência do "objecto capital das investigações biológicas para a morfologia" (Cuvier), quer dizer, para a ›génese ideal‹.[67] Por outro lado, um pensamento biológico guiado pela "teoria da metamorfose de Goethe"[68] substitui, na opinião de Cassirer, a questão metafísica e, por extensão, a improfícua questão "da essência e da origem da vida". Este novo pensamento põe as "formas de vida [particulares – C. M.]" numa ordem sistemática, numa totalidade, e é através desta ordem que serão contempladas conjuntamente; este pensamento utiliza o conceito de tipo**,** o qual permite, por sua vez, encontrar no mundo orgânico as "configurações da vida".[69] A biologia moderna, ao descobrir pela primeira vez "o significado elementar do factor [da] ›forma‹",[70] afasta-se do conhecimento metafísico, ou seja, do conhecimento – explicado causalmente – da origem, porque estas ›questões da origem‹ "conduzem a biologia [...] a ›aporias‹ obscuras".[71] Esta alternativa metodológica à questão da forma descritiva e à questão metafísica da origem é encontrada por Cassirer não só na biologia, mas também, do mesmo modo, nas ciências da cultura: os objectos culturais também podem ser compreendidos como um conjunto de formas ou ser estudados a partir da sua origem (›porquê‹, ›de onde‹).[72]

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[67] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 148.
[68] Christian Möckel: Formenschau, Formenwandel und Formenlehre. Zu Goethes Morphologie- und Metamorphosenlehre. In: Goethe-Jahrbuch. (Goethe-Gesellschaft in Japan) Bd. 52 / Tokyo 2010, 45-73.
[69] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 149.
[70] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 94.
[71] Ebd., 189.
[72] Ebd., 96.

Teoria darwinista da evolução: A teoria darwinista da evolução significa, na visão de Cassirer, um passo importante, porque ela supera, com a sua hipótese de um "continuous and uninterepted stream of life"[73], todos os "arbitrary limits between the different forms of organic life" resultantes da classificação científica.[74] No entanto, esta teoria usa as mutações contingentes na vida de cada organismo com o objectivo de explicar a transformação "from the simplest forms of life [...] to the highest forms"[75]. Além disso, ela explica "the phenomenon of life", "the structure of organic nature",[76] a partir das causas materiais contingentes, ou seja, a partir das causas mecânicas. Com a teoria darwinista, Cassirer vê agora o historicismo do século XIX (Hegel) a "infiltrar-se também na Biologia"[77] e a estabelecer-se como pesquisa histórica dos seres vivos.[78] Daí surgiu igualmente a necessidade de aclarar a aplicabilidade do método descritivo (o histórico) e do método analítico-causal (o racional) aos processos da vida.[79] Cassirer nutre simpatia pela tentativa de Goethe de excluir o "espírito da simples análise" – como método de trabalho – da Biologia, porque esta exclusão prepara e confirma o reconhecimento da ›autonomia do orgânico‹ (vitalismo).[80]

Animismo/Vitalismo: Analisando a transformação da biologia como uma ciência que, começando por tratar o organismo quase como um mecanismo físico, passa agora a dar importância à percepção da expressão, aos fenómenos expressivos que remetem para a ›vida‹ e a ›vitalidade‹, Cassirer atribui, nos seus textos, um papel decisivo à corrente do vitalismo e à sua reiteração moderna nos estudos de Jakob von Uexküll .[81] Por isso, ele vai dar uma grande atenção ao debate entre os vitalistas que defendiam o particular e o específico do orgânico ("força da vida") em detrimento do inorgânico[82] e os seus adversários, que apenas estavam interessados, segundo o seu princípio monista, em "dissolver o organismo simplesmente num sistema de forças em movimento" (modelo este, aliás, que já era aplicado ao mundo da natureza inorgânica).[83] O novo vitalismo (Uexküll) tenta defender a "autonomia da vida" através do conceito de forma, que é asseverado como uma alternativa às concepções

[73] Ernst Cassirer: An Essay on Man. In: ECW23, 25.
[74] Ebd., 25.
[75] Ebd., 24.
[76] Ebd., 23.
[77] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 197.
[78] Ebd., 200.
[79] Ebd., 210f.
[80] Ebd., 214, 217.
[81] Ebd., 150; Ders.: Zur Logik der Kulturwissenschaften. In: ECW24, 404.
[82] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 218.
[83] Ebd., 220.

mecanicistas.[84] O conceito de forma, que, norteando-se pela totalidade, vigorava agora na biologia vitalista do primeiro terço do século XX, tinha substituído, na opinião de Cassirer, o antigo conceito teleológico de finalidade. Resta observar que o conceito do todo é utilizado muitas vezes por Cassirer no sentido leibniziano, isto é, como algo que é mais do que a soma das suas partes.[85] Ele adiciona a este a ideia de representação: cada momento temporal representa, por exemplo, a sua totalidade, inclui esta – ou seja, o sentido – imediatamente em si; e por isso esta pregnância do sentido deve ser reconhecida como um ›arqui-fenómeno‹.[86] Para Cassirer, a totalidade denota, em muitos casos, um todo significativo que se manifesta num fragmento significativo particular.

Método descritivo: O método que melhor se aplica às investigações da biologia – o método da pura descrição dos processos orgânicos – orienta-se, na opinião de Cassirer, pelo conceito de totalidade, e não pelo conceito psicológico e teleológico de finalidade (*Ungerer*).[87] Cassirer anuncia, entre outros, Ludwig von Bertalanffy como um representante importante desta Biologia puramente descritiva.[88] Uexküll aplicava também o método puramente descritivo aos fenómenos orgânicos; método esse que, aparentemente, é preferido pelo próprio Cassirer.[89] E é o seu método preferido porque leva inevitavelmente ao problema da forma, ao conceito de forma no seu significado fulcral para a Biologia, visto que o método mecânico-materialista considerava somente a "soma das partes" em detrimento da forma. Numa ›ciência da forma‹, não orientada pelas causas mas antes pelas estruturas, os fenómenos singulares serão submetidos à ordem estabelecida pelas formas.[90] A ›análise da forma‹, ao contrário da análise causal da lei, é interpretada por Cassirer como um "sentido específico do saber", quer dizer, da formação dos conceitos e do conhecimento.[91]

Método analítico-causal: No entanto, Cassirer não interpreta só a questão do mecanismo, que já tinha sido criticada pelo vitalismo, como uma posição unilateral

[84] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 378; Ders.: An Essay on Man. In: ECW23, 28f.
[85] Ernst Cassirer: Leibniz' System. In: ECW1, 115ff., 361.
[86] Ernst Cassirer: Praesentation und Repraesentation. (1927) In: ECN4, 4.
[87] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 247.
[88] Ebd., 250; Ders.: Zur Logik der Kulturwissenschaften. In: ECW24, 452f. Sobre a recepção de Bertalanffy pelos filósofos, especialmente por Cassirer, vide também: David Pouvreau † and Manfred Drack: On the history of Ludwig von Bertalanffy's ›General Systfemology‹, and on its relationship to cybernetics. In: International Journal of General Systems. Vol 36, No. 3, June 2007, 281-337, hier 294ff.
[89] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 49.
[90] Ebd., 160f.
[91] Ebd., 94.

ligada a uma Biologia mecânico-materialista[92] e à "teoria mecanicista (*Maschinentheorie*) da vida"[93], e que não estava, por causa disso, em condições de resolver o "enigma da vida". Pelo contrário, ele interpreta também o próprio vitalismo biológico[94], principalmente quando este insiste, como é defendido por Hans Driesch ou Eduard von Hartmann, no conceito das ›forças de vida‹ – conceito que Cassirer reprova, dada a sua dimensão metafísica conduzir, de igual modo, a uma posição unilateral.[95] A Biologia moderna havia evitado ambos os extremos, e aproximava-se, cada vez mais, "de um sentido puramente metódico do problema [da vida]". Ela já não tentava deduzir as formas de vida orgânicas a partir das forças puramente mecânicas, mas insiste antes no facto de que as formas de vida orgânicas "não podem ser descritas totalmente através de conceitos puramente causais. E, para poder corroborar isso, ela recorreu à categoria da ›totalidade‹".[96] Por outras palavras, a biologia moderna possui a consciência de que os conceitos de forma, na sua qualidade de conceitos *sui generis*, "não podem ser reduzidos integralmente a ›leis‹ (a forças causais)",[97] ou seja, só algumas das suas dimensões podem ser parcialmente remetidas para a explicação causal. Mas isso significa, mais uma vez, que o método analítico-causal da explicação não é excluído totalmente. Pelo contrário, os limites da capacidade de explicação tornam-se, com isso, visíveis e definidos, contribuindo, nessa exacta medida, para que a ›consideração do todo‹ (›holismo‹[98]), intrínseca aos conceitos de forma e de estilo, obtenha o seu espaço próprio.[99]

Além destes conceitos, também os de leis causais – mais concretamente, o característico conceito de lei na física – desempenham, nos processos orgânicos, um papel insofismável. Cassirer, que aqui se alicerça em Moritz Schlick e na interpretação do conhecimento como um ›reencontrar‹ (*Wiederfinden*), um ›reconhecer‹ (*Wiedererkennen*)[100], vê nos conceitos de lei a "forma ›exacta‹ de um ›reencontrar‹"[101], uma forma cujos fenómenos físicos determinados por nós – isto é, pelos físicos –, assim como as suas condições de produção, podem ser analisados. Ele

[92] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 46f.
[93] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 452.
[94] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 238, 242.
[95] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 452.
[96] Ebd., 452.
[97] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 165.
[98] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 453 Anm. 1.
[99] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 167f.
[100] Christian Möckel: Moritz Schlick und Ernst Cassirers Auseinandersetzung mit dem ›Wiener Kreis‹. In: F.O. Engler / M. Iven (Hrsg.): Moritz Schlick - Ursprünge und Entwicklungen seines Denkens. (Schlickiana Bd. 5) Berlin 2010, 207-224.
[101] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 167.

vê exactamente nisso "a essência das experimentações físicas"[102], o que aparece confirmar a interpretação do conceito cassireriano de lei, que assenta, por sua vez, na investigação das estruturas e das configurações, abraçando, de igual modo, as condições extrínsecas possibilitadas pelos processos causais (Oswald Schwemmer). É neste sentido que podemos assimilar a seguintes afirmações de Cassirer: "A força da experimentação [física, objectivável – C. M.]", que não é aplicável aos fenómenos do mundo cultural, estende-se, pelo contrário, "largamente à biologia – é deveras evidente que as constatações mais relevantes da biologia moderna provêm da experimentação".[103]

Com efeito, a biologia também é "determinada pelos pontos de vista causais", mas o facto de estes não poderem "ser ignorados por ela", não significa, porém, que ela "possa ser construída exclusivamente por eles".[104] A biologia é, por conseguinte, ciência da lei e ciência da forma! Nós procuramos nela, "como máxima heurística", as "leis para o processo orgânico", o que, por seu lado, só pode ser atingido a partir do conceito de forma.[105] O desenvolvimento causal pode ser analisado e conhecido "no âmbito da vida orgânica". Contudo, isto não se aplica inteiramente à origem deste âmbito a partir de um outro,[106] como não pode, por exemplo, ser deduzida a origem do âmbito da cultura da própria natureza.[107] As ciências da cultura baseiam-se, neste sentido, e apesar dos seus conceitos de forma e de configuração, igualmente no conceito de causalidade, nomeadamente naquilo que diz respeito ao problema causal (devir, história).[108]

Ciência como problema da forma: A transição metódica, efectuada pelas teorias científicas contemporâneas, para a questão da forma é estendida por Cassirer, como já foi indicado, não só às ciências da cultura e a biologia, mas também – e devido aos desenvolvimentos nas ciências da natureza em geral – à física (como teoria de campos [*Feldtheorie*]), à biologia (como teoria da evolução) e a psicologia (como psicologia da forma [*Gestalt-Psychologie*]).[109] Todas elas se afastam progressivamente do materialismo mecânico e do monismo, e introduzem metodicamente totalidades e estruturas como "algo originário, indeduzível".[110] Estas totalidades e estruturas são compreendidas como as condições de possibilidade das

[102] Ebd., 168.
[103] Ebd., 168.
[104] Ebd., 189.
[105] Ebd., 166.
[106] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 460f.
[107] Ebd., 461.
[108] Ebd., 455f.
[109] Ebd., 454f.; Ders.: Probleme der Kulturphilosophie. In: ECN5, 57.
[110] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 57.

suas partes. O conceito de estrutura impõe-se (e opõe-se), num certo sentido, ao conceito de causa como princípio dominante.[111] Segundo Cassirer, é desta maneira que se vai extinguir uma "linha divisória" entre as ciências da natureza e as ciências da cultura, o que não significa, de nenhum modo, que a biologia e as ciências da cultura não preservem as suas respectivas particularidades metódicas. Da oposição inicial entre causa e forma (estrutura) sobreveio, por conseguinte, uma correlação entre ambas; correlação essa, todavia, que inclui simultaneamente uma modificação do significado – ou uma individuação do significado – dos próprios conceitos.

Estes aspectos da correlação metódica são resumidos por Cassirer da maneira seguinte: "A lógica da investigação está agora em condições de conferir a todos estes problemas [metódicos – C. M.] o seu lugar. As análises da forma e as análises causais aparecem agora como orientações, não estando mais em conflito uma com a outra, mas sim como orientações que se completam uma a outra e que tem de relacionar-se em todo o saber."[112] A ideia de "construção" (*Aufbau*), relevada pelo próprio Cassirer, tem que ver directamente com a "surpreendente construção articulada da ciência da natureza e da ciência da cultura" decorrente do processo de objectivação gizado através dos conceitos de lei e dos conceitos de forma. Assim, o filósofo alemão tem em mente, entre outras coisas, que ambos os tipos de ciência incluem o "particular e o geral como [...] momentos correlativos", perseguindo, nessa exacta medida, um objectivo científico comum, apesar, é evidente, da diferença que reside nos seus métodos.[113] Dependendo do modo de actuação da "›unidade sintética do múltiplo‹", distinguem-se as classes objectivas do objecto físico, biológico, psíquico e espírito-cultural.[114]

Conceito de forma e conceito de lei: Estas classes ou áreas de objectos das ciências – física, biologia, psicologia e cultura (esfera ideal) – parecem formar, segundo Cassirer, um tipo de hierarquia, que faz lembrar a ›ontologia dos estágios‹ (*Stufenontologie*) de Nicolai Hartmann. Por exemplo, as transições do estágio ou da área mais baixa para a mais alta, não põem em causa as leis inferiores, não levam a nenhuma ruptura entre as leis da biologia e as da física, desencadeando, bem pelo contrário, novas leis biológicas. Aquilo que é realizado acaba por ser um salto qualitativo para um novo "tipo de problema e de objecto".[115] Deste modo, a área objectiva das ciências da cultura continua a incluir leis e estruturas da biologia ("referência à totalidade"), embora enriquecidas por um ›cunho‹ específico da cultura.

[111] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 455.
[112] Ebd., 455.
[113] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 164.
[114] Ebd., 165.
[115] Ebd., 63.

Em cada área de objectos, nomeadamente em cada ›estágio‹ da ciência, os problemas da lei e da forma encontram-se numa relação particular distinta, porque cada ciência é simultaneamente ciência da lei e ciência da forma.[116] Para a Biologia, torna-se válido que nela "continua a existir ainda a relação entre o conceito de forma e o conceito de lei, que ambos não podem ser reduzidos um ao outro – [...] mas que devem, porém, ser correlacionados permanentemente".[117] Neste sentido, um "equilíbrio entre conceitos de forma e conceitos de lei" aplica-se sobretudo à biologia descritiva.[118] A física teórica era, se continuarmos envolvidos pela argumentação de Cassirer, caracterizada pelo primado dos conceitos de lei em relação aos conceitos de forma, enquanto que, nas ciências da cultura, o "primado dos conceitos de forma" põe-se em evidência perante os conceitos de lei. O facto de que é necessário considerar os conceitos de forma como formas de pregnância, como formas vivas, contribui, por sua vez, para que as ciências da cultura e a biologia possam tecer elos de ligação entre si.

4. Considerações finais: razões do interesse pela biologia

Olhando retrospectivamente para as teses cassirerianas das analogias e das semelhanças, anteriormente apresentadas, resta-nos explicar porque é que a biologia é tão frisada, quais são as bases dos principais fundamentos que levam Cassirer a desenvolver um interesse tão veemente pela biologia científica e pela relação desta com as ciências da cultura, embora o filósofo alemão, enquanto filósofo da cultura, elabore a sua obra tardia dedicada à ›lógica das ciências de cultura‹, assim como já tinha caracterizado a sua ›Filosofia das formas simbólicas‹ como uma ›filosofia crítica da cultura‹ que tenta explicar descritivamente os objectos culturais como um conjunto de formas.[119] Como resposta provisória as estas questões, podemos chamar a atenção para três pontos de referência sistemáticos.

Esta relevância da biologia, este interesse sistemático pelas suas dimensões, tem como *primeiro* fundamento a convicção de Cassirer que, por um lado, as formas de vida superiores da cultura ultrapassam as formas de vida elementares, orgânicas, mas que, por outro lado, tem de ser inferido um tipo de ›enraizamento [*Verwurzelung*]‹ – nomeadamente um ›brotar [*Entspringen*]‹ – das formas de vida superiores da cultura da forma natural da vida orgânica, "do substrato primário e

[116] Ebd., 92.
[117] Ebd., 165.
[118] Ebd., 166.
[119] Ebd., 96.

originário da vida".[120] Neste sentido, o conceito ›vida‹ não se refere simplesmente ao organismo vegetativo com as suas leis biológicas, mas antes à função expressiva nas formas de vida superiores. Por exemplo, o homem tem de apreender "o seu mundo particular de objectos e de formas" a partir do mundo do "primado da percepção expressiva", que determina as dimensões da percepção, as capacidades cognitivas dos animais[121], bem como, enquanto "substrato elementar da existência e do comportamento", o "mundo da criança e o do homem ›primitivo‹".[122] Este desvendar do *Entspringen*, às vezes também chamado por Cassirer ›génese‹, levava a "colocar um limite (*Grenzsetzung*) entre ›vida‹ e ›espírito‹, entre o mundo das formas orgânicas e o mundo das formas culturais", e encontrava a sua explicação, por exemplo na obra de Uexküll, numa "diferença funcional", numa "mudança de função característica" de todas as determinações.[123]

Cassirer continua a dar extrema relevância à questão se temos de partir de uma continuidade entre vida biológico-vegetativa e vida do espírito. Só se pode dar uma resposta afirmativa a tal questão se interpretarmos a vida espiritual através de categorias biológicas, como fazem, por exemplo, Darwin, Nietzsche e também Spengler; ou então se a constituição da vida orgânica e vital fosse, enquanto objecto de conhecimento, sustentada em analogias e categorias do espiritual. Cassirer assume uma atitude céptica em relação à hipótese, defendida por Darwin, de que o fenómeno da expressão espiritual, constituindo o fundamento da cultura, poderia se concebido como quase uma simples extensão (ou uma simples sublimação) dos processos de expressão orgânico-vitais. Porque, neste caso, tínhamos de "compreender o processo de expressão como um processo de vida puro", e tínhamos, nessa medida, "de o descrever com categorias puramente biológicas",[124] o que implicaria, por outro lado, uma continuidade entre o âmago da vida natural e o da vida espiritual. A consequência era, desta forma, que o mundo da cultura seria integrado no mundo da natureza e "explicado segundo o mesmo princípio inerente a este"[125] – uma consequência que Cassirer pretende evitar.[126]

Além disso, aqui também é posta implícita e simultaneamente a questão se as funções da vida espirituais derivam das funções da vida orgânico-vitais, mais concretamente, se as funções vitais se transmutam em espirituais ou, pelo menos, se

[120] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 376f., 378.
[121] Ernst Cassirer: Ziele und Wege der Wirklichkeitserkenntnis. In: ECN2, 84.
[122] Ebd., 86f.
[123] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 379f., 381.
[124] Ernst Cassirer: Zur Metaphysik der symbolischen Formen. Hrsg. von John M. Krois. (Nachgelassene Manuskripte und Texte. Bd. 1) Hamburg 1995 (ECN1), 37.
[125] Ebd., 38.
[126] Ebd., 244.

as primeiras possibilitam as últimas, como Simmel sugere na sua colectânea de ensaios *Lebensanschauung* (1918).[127] Cassirer defende aqui aparentemente a ideia de que a vida espiritual significante – quer dizer, as formas simbólicas espirituais e vitais – não emerge das funções orgânico-vegetativas, assim como também não há qualquer transferência destas últimas para a vida espiritual. Porque o espírito, no seu nível mais elevado, o pensamento puro da função de significação, move-se exclusivamente no mundo normativo, num mundo ideal sem qualquer "plenitude de vida individual" e, deste modo, sem qualquer determinação da utilidade e da finalidade que caracterizam a vida não-espiritual.[128] Apesar disso, as suas formas simbólicas encontram-se, numa fase primária, confinadas, como na colectânea de Simmel, ao "âmbito da simples ›utilidade‹", isto é, o âmbito da simples luta pela existência, e elas têm de servir, nessa exacta medida, finalidades de vida que, apesar de inteiramente desconhecidas, lhes são essenciais como ponto de partida.

A relevância dada à biologia surge, no seu *segundo* fundamento, com a observação de Cassirer de que a biologia moderna, como uma ciência da vida, tem-se transformado, num sentido estritamente científico-metodológico, numa disciplina-chave: a compreensão do conceito de vida (de forma de vida) tem substituído, de certo modo, o conceito de função da matemática. Cassirer considera que, na segunda metade do século XIX e no início do século XX, a biologia não só se tem transformado numa verdadeira ciência,[129] mas também já no século XIX "the biological thought takes precedence over mathematical thought".[130] Por isso, disciplinas como a antropologia filosófica defenderam que agora elas tinham de se alicerçar na biologia (como teoria geral da evolução) e já não mais na matemática.[131] Uma expressão desta predominância do pensamento biológico era provavelmente – também nos textos de Cassirer – o uso variado de conceitos com uma intenção metódica, como, por exemplo, "inquirição orgânica" [uma observação integral – C. M.],[132] "correlação orgânica" [isto é, sistemática, estruturada],[133] "todo orgânico" [uma ordem estruturada em desenvolvimento].[134] Em muitos casos, as formas culturais são designadas como "organismos" vivos,[135] como é bem patente, por

[127] Georg Simmel: Die Wendung zur Idee. In: Ders.: Lebensanschauung. Vier metaphysische Kapitel. München und Leipzig 1918, 28-98, hier: 38.
[128] Ernst Cassirer: Zur Metaphysik der symbolischen Formen. In: ECN1, 99f.
[129] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 19.
[130] Ernst Cassirer: An Essay on Man. In: ECW23, 22.
[131] Ebd., 22f.
[132] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 188.
[133] Ernst Cassirer: Das Erkenntnisproblem Bd. IV. In: ECW5, 58.
[134] Ebd., 141.
[135] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 127.

exemplo, na expressão "organismo vivo da linguagem".[136] Porém, trata-se aqui de saber se Cassirer utiliza os conceitos conforme a acepção da Biologia ou, pelo contrário, segundo a semântica das ciências do espírito.

O interesse pela Biologia está ligado, no seu *terceiro* fundamento, às analogias da forma, às semelhanças da forma assinaladas por Cassirer, que resultam, em primeiro lugar, da incorporação irredutível da vida e do vivido em ambas as áreas de objectos e de conceitos. Aqui, quando se fala do processo da vida, é necessário também pensar na diferença da vida orgânica (relação causal [*Wirkverhältnis*]) e da vida espiritual (relação de sentido). A atenção conferida a estas analogias tem, como presume Cassirer, inicialmente a sua raiz já nas inovações metódicas observadas no desenvolvimento das ciências modernas da natureza e alimenta-se, por sua vez, da questão formulada em mil e novecentos e trinta e nove: qual é o significado que possuem os conceitos de forma e de estrutura, introduzidos nas ciências da natureza como "algo de originário, indeduzível", para "a fundamentação da filosofia da cultura e [...] para a possibilidade da ciência da cultura"?[137] Esta formulação[138] sugere um primado – pelo menos metódico – das ciências da natureza neste domínio da formação de conceitos científico-culturais. Para Cassirer, as analogias tematizadas sobre o problema da forma encerram um fundamento particular; algo que aparentemente falta na física, mas que existe especificamente tanto na Biologia ("vida orgânica") como nas ciências da cultura ("vida espiritual"), mais concretamente, tudo aquilo que se revela no "substrato primário" dos fenómenos que espelham o comportamento mundano apoiado em invariantes, em normas de respostas idênticas aos impulsos do meio ambiente.[139] Este substrato primário é localizado por Cassirer, como se sabe, na área da percepção da expressão. Ao mesmo tempo, temos de esclarecer se – e até que ponto – Cassirer emprega estes conceitos de forma, procedentes da filosofia e das ciências da cultura, com o objectivo último de os transferir e aplicar aos métodos das ciências modernas da natureza, tal como estas são por ele compreendidas.

É evidente que este interesse pela biologia entra em relação com a tentativa de Cassirer, iniciada no fim dos anos vinte, de colocar a sua filosofia teórico-simbólica da cultura e a sua antropologia filosófica numa conexão muito estreita, numa relação de fundamentação mútua. Por isso, interessa-nos também indagar se – e, mais uma vez, até que ponto – Cassirer se aproxima de uma orientação teórica que sustenta um

[136] Ernst Cassirer: Zur Logik der Kulturwissenschaften. In: ECW24, 403.
[137] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 57.
[138] Esta relação entre *ciência* da cultura e *filosofia* da cultura, ancorada, talvez, na relação entre ciência da linguagem e filosofia da linguagem, não será, contudo, por nós aprofundada.
[139] Ernst Cassirer: Ziele und Wege der Wirklichkeitserkenntnis. In: ECN2, 83.

enraizamento, uma fundação do mundo espírito-cultural no mundo natural e orgânico. Tal poderia sugerir que a normatividade biológica das formas continua, pelo menos, a actuar na vida cultural e espiritual do ser humano.[140] Trata-se, na verdade, de uma orientação teórica que adquire um genuíno interesse, principalmente quando é necessário esclarecer e determinar a relação entre a cultura e a natureza dentro da esfera do próprio ser humano, já que Cassirer também aventa de forma clara que o mundo especificamente humano "forms no exception to those biological rules which govern the life of all the other organisms".[141]

O propósito de relacionar metodicamente o cultural e o espiritual com a biologia, não está somente presente na filosofia de Cassirer. Encontramos, por exemplo, na obra *Einleitung in die Geisteswissenschaften (Introdução às ciências do espírito*) de Wilhelm Dilthey – autor que desempenha um papel estimulante para o pensamento de Cassirer e que foi, em mil e novecentos e vinte e nove, classificado pelo mesmo como de extrema relevância para a então ainda jovem disciplina da filosofia da cultura[142] – a hipótese cabalmente inesperada de que "as ciências do organismo [formam] a sua [isto é, das ciências da cultura – C. M.] base", já que as "ciências humanas, da sociedade e da história, [têm] como seu fundamento unicamente a natureza".[143] Num outro lugar, Dilthey também tematiza, como Norbert Meuter constatou correctamente, o "significado da natureza deduzida pela [...] Biologia [...] como ›fundamento‹ válido para as ciências da cultura".[144] Estas considerações encontram-se em conexão com o propósito de Dilthey em arquitectar uma antropologia filosófica que procurasse abarcar e unir o homem como um ser espiritual portador de cultura e, ao mesmo tempo, como um ser orgânico portador de um corpo.[145] Todavia, não é deveras intenção de Cassirer interpretar os conceitos biológicos de forma como conceitos culturais. Isso torna-se particularmente evidente quando ele exprime a convicção de que não se deve compreender o jogo com as analogias e as semelhanças no sentido de reconduzir (e desintegrar) as formas

[140] Christian Möckel: Kulturelle Existenz und anthropologische Konstanten. Zur philosophischen Anthropologie Ernst Cassirers. In: Zeitschrift für Kulturphilosophie. (Hamburg) 3. Jg. Heft 2/2009, 209-220.
[141] Ernst Cassirer: An Essay on Man. In: ECW23, 29.
[142] Ernst Cassirer: Probleme der Kulturphilosophie. In: ECN5, 3.
[143] Wilhelm Dilthey: Einleitung in die Geisteswissenschaften. Erster Band (1888). In: Gesammelte Schriften. Bd. 1, Leipzig und Berlin 1922, S. 19.
[144] Norbert Meuter: Anthropologie des Ausdrucks. Die Expressivität des Menschen zwischen Natur und Kultur. München 2006, 40, 47.
[145] Norbert Meuter: Anthropologie des Ausdrucks, 80f.

culturais e espirituais aos seus níveis biológicos inferiores, "apesar de [essas formas] poderem estar geneticamente em directa conexão com estes últimos".[146]

Além disso, temos de averiguar se e por que é que este processo de denominar as analogias da forma aparece fundamentado, bem como indagar a sua validade para uma inquirição filosófica dos fenómenos culturais. Dilthey, por exemplo, que ainda em mil e novecentos e dez, na obra *Aufbau der geschichtlichen Welt in den Geisteswissenschaften* (*A construção do mundo histórico nas ciências do espírito*), revela a ideia de que o mundo cultural do espírito objectivo e a estrutura da consciência hermenêutica tinham certas "semelhanças com a estrutura biológica", acaba por defender, no entanto, que tal remissão só pode conduzir a "analogias vagas".[147] E, ainda não por último, resta explicar se estas analogias metódicas são formuladas *da* biologia *para* as ciências da cultura ou, pelo contrário, no sentido inverso. O último processo – das ciências da cultura para a biologia – debilitaria sistematicamente a tese cassireriana da emergência. A observação de Cassirer, anteriormente citada, de que o reconduzir das questões causais ao problema da forma "é *já* válido para a Biologia", indica desde logo que ele pensa reencontrar certos dados científico-culturais nos dados biológicos, o que facultaria aos primeiros uma validade adicional.

Se nós apontamos tais analogias da forma, então temos também de falar das diferenças metódicas fundamentais entre o problema da forma nas ciências da cultura e na biologia (compreendida como uma ciência da natureza), porque se trata, em rigor, de ›duas culturas‹ científicas. Contudo, parece que, no que diz respeito aos conceitos, Cassirer nem sempre utiliza de uma maneira consistente nos seus textos as distinções (oposições) e as comparações (unificações) entre ambos os tipos de ciência. É provável que, em muitos casos, o filósofo utilize o termo ciência em duas acepções completamente diferentes (Ernst Wolfgang Orth): a primeira estaria relacionada directamente com a auto-consciência metódica dos cientistas ligados à investigação, que impõe uma distinção entre as ciências da natureza como ciências da causalidade e as ciências da cultura como ciências descritivas, impregnadas de um estilo individual; e a segunda acepção estaria relacionada com a compreensão metódica da variabilidade dos problemas da forma e da causalidade em ambos os tipos de ciência,

[146] Cassirer define esta formulação sobre a forma simbólica como algo de ›arqui-fenomenal‹, que não existe no reino animal: "a forma simbólica é sempre algo particular, indeduzível, *sui generis* [,] que não remete para outros níveis precedentes (biológicos), que se mantém irredutível a estes, apesar da possibilidade de estar geneticamente em conexão com os mesmos". – Ernst Cassirer: Ausdruckssphänomen und ›Wiener Kreis‹. In: ECN4, 158.

[147] Norbert Meuter: Anthropologie des Ausdrucks, 48; Wilhelm Dilthey: Der Aufbau der geschichtlichen Welt in den Geisteswissenschaften. In: Gesammelte Schriften. Bd. 7. 8. Aufl. Stuttgart/Göttingen 1992, 23.

compreensão essa, no entanto, que abraçaria o problema da forma como um objecto temático originário, presente em todos as áreas científicas.

Nas considerações de Cassirer, é possível encontrar ainda, se bem me parece, algumas interpretações sobre esta última questão, que levam a sentidos diferentes. Encontramos, em *primeiro* lugar, a oposição expressa entre ciência da lei (causalidade) da natureza (condições laboratoriais) e ciência da forma da cultura, mais concretamente, ciência do estilo (individualidade e liberdade). Em *segundo* lugar, vislumbramos a interpretação da ciência como processo da formação de invariantes a partir de um material múltiplo, podendo-se aqui distinguir duas direcções da formação de invariantes: uma que parte dos conceitos provenientes das leis da natureza, as quais obrigam a uma submissão do singular, do caso, ao geral, à lei; e a outra que parte dos conceitos de estilo e de forma, sendo, por sua vez, o singular aquele que aqui representa o geral, aquele que permite, por exemplo, a expressão simbólica do geral.

Em *terceiro* lugar, encontramos a concepção que vê a ciência (incluindo as ciências da natureza) principalmente dominada pelos problemas da forma e pelos problemas da causalidade. Neste âmbito, o problema da causalidade tem de ser provavelmente compreendido como uma investigação das condições estruturais, das configurações como relações causais, já que é também possível associá-lo ao problema da forma. Trata-se aqui – dependendo do objecto de pesquisa – de uma relação dinâmica e mutável, visto que, no que concerne à relevância do problema da forma, Cassirer sublinha diferenças, por exemplo, entre a mecânica e a física de campo (*Feldphysik*), entre a psicologia experimental a psicologia da forma (*Gestaltpsychologie*). Em *quarto* lugar, e por último, somos confrontados com uma interpretação da própria ciência da natureza (biologia) como um fenómeno cultural. O universo da natureza é concebido como espaço cultural, como universo simbólico. A diferença que existe entre ciências da natureza e ciências da cultura é realizada e designada pela cultura, pelo ser cultural que actua como cientista da natureza (Ernst Wolfgang Orth). Deste modo, o conceito de cultura encerra tanto a inclusão das ciências da natureza como a sua exclusão.

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Arguments for "Descriptivism"

Raffaela Giovagnoli
(Pontifical Lateran University)
raffa.giovagnoli@tiscali.it

The classical problem that characterizes the discussion on reference in analytic philosophy is whether when we refer to, for instance, "Aristotle" (proper name) the fact that he was "the Master of Alexander the Great" is included in the sense through which we understand the name. The "descriptive" theory of reference that, starting from Frege and Russell was reinterpreted by Searle and Strawson, contrasts the theories of "direct reference" that are grounded on the causality of external objects on mind (famous authors of this view are Kripke, Donnellan and Kaplan).

Descriptivism can be analyzed on two different levels: first, on the level of the philosophy of language, focusing on how proper names and definite descriptions function in the process of reference; second, on the level of the philosophy of mind focusing on how we grasp objects through sensorial and perceptive processes. I'll argue for descriptivism as presented by John Searle, who considers both sentences and mental states and processes in comparison with descriptivism by Frege and Russell. I'll try to clarify why, contrary to Russell (who aimed at eliminating the Fregean "senses"), we cannot avoid to consider the level of the pragmatic sense of linguistic expressions.

1. Proper Names

As it is well known, Frege introduced the thesis that proper names (names and definite description) have two components: "reference" (*Bedeutung*) and "sense" (*Sinn*). Starting from the questions: "Is identity a relation?, "Is it a relation among objects or among names or signs of objects?" we can individuate two fundamental solutions. The first solution is the one presented in the *Begriffschrift* that privileges the relation among names or signs of objects; the second is the one we find in *Sinn*

und Bedeutung that privileges the relation among objects. In this essay Frege tries to substance the semiotic analysis of the *Begriffschrift*. The sentences $a = a$ and $a = b$ seem to have different cognitive value: sentences of the first form are valid a priori, they are "analytic"; whether sentences of the second form are not always justifiable a priori. If we intend identity as a relation between what the sign "a" and "b" designate (*Bedeuten*) then $a = a$ seems not to differ from $a = b$ (under the condition that $a = b$ is true). This happens because we obtain a relation in which a thing is with itself and no other thing is with another, namely $a = b$ seems to allude to the fact that the names or signs "a" and "b" designate the same thing. In this sense, the discourse would be about the signs and a relation between signs would be stated. Consequently, we would have to deal with a relation mediated by the connection of each of the two signs with the one designated and this connection is arbitrary. On the one side, we have the possibility to freely establish to let whatever object be sign for anything, on the other we have the necessity to obtain an authentic knowledge. If "a" would distinguish itself from "b" by virtue of its form but not as sign then $a = a$ and $a = b$ would have the same cognitive value: «A difference can arise only if the difference between the signs corresponds to a difference in the mode of presentation [*Art des Gegebenseins des Bezeichneten*] of the thing designated. Let *a*, *b* and *c* be the lines connecting the vertices of a triangle with the midpoints of the opposite sides. The point of intersection of *a* and *b* is then the same as the point of intersection of *b* and *c*. So we have different designations for the same point, and these names ('point of intersection of *a* and *b*', 'point of intersection of *b* and *c*') likewise indicate the mode of presentation and hence the statement contains actual knowledge» (Frege 1892, p. 152). According to Frege, this example suggest us to intend a sign as connected in addition to what it designates (*Bedeutung*) also to the "sense" of the sign in which the content of the mode of presentation of the object is given. Thus, we will have that the meaning of the expressions 'the point of intersection of *a* and *b*" and 'the point of intersection of *b* and *c*' is the same, but not the sense. Similarly, the meaning of 'The Morning Star' and 'The Evening Star' is the same, namely 'Venus', but not the sense.

Russell underscored some difficulties in the theory of Frege. A first difficulty arises if we maintain that denotative syntagmata (for instance, "a man", "some men", or "The Morning Star") express a meaning and denote a denotation that concerns cases in which the denotation is absent. If, for example, we consider the assertion "The actual king of France is bald" it does not seem to be an assertion on the complex meaning of "the actual king of France", but an assertion about the real man denoted by the meaning. So, the assertion "The actual king of France is bald" that has the same form of the previous one, would be on the syntagma "the king of France", to

have therefore a meaning but not a denotation. This let us suppose that "the king of France is bald" is a nonsense; according to Russell, the proposition is false.

A second difficulty is bound to the consideration of the denotation of a denoting syntagma C as opposed to its meaning. For example, the first line of Gray's Elegy asserts a proposition and "The first line of Gray's Elegy" does not assert a proposition. In the second case, the quoted syntagma expresses the meaning. We have here a problem with the logical relation between meaning and denotation. In particular, it is difficult to hold the connection between meaning and denotation and at the same time to avoid that they identify each other (for the meaning can be grasped only through a denotative syntagma). The denotative syntagma C would have either a meaning and a denotation. But if we want "the meaning of the first line of Gray's Elegy" we obtain "the meaning of 'the curfew tolls the knell of parting day'" that does not correspond to "The meaning of 'the first line of the Gray's Elegy". If we want the desiderated meaning we must not talk of "the meaning of C" but of "the meaning of 'C'", that corresponds to the one of "C". Things are the same as regards the denotation as "the denotation of C" does not mean the desiderated denotation but an arbitrary one. Thus, if C = "The first line of Gray's Elegy" and the denotation of C = "The curfew tolls the knell of parting day" we do not obtain the denotation we wanted of "The first line of Gray's Elegy". Russell's argumentation brings to the conception according to which a denotative syntagma is *essentially part of a sentence* and, differently from the most part of the words, it has no sense by itself. If we would like to know, for example, that Scott was the author of Waverley we must exclude that the substitution of "The author of Waverley" with Scott and thus that Scott = Scott could normally be of some interest for us. Consequently, "Scott was the author of Waverley" (being Scott identical with the author of Waverley) becomes "One and only one entity wrote Waverley and Scott was identical with this entity". If x is whatever entity and "C" a denotative syntagma, giving that the proposition "x is identical with C" is true, then x is the denotation of "C". Scott is the denotation of "the author of Waverley". The "C" under quotation is the simple syntagma and nothing else that we could identify as the *meaning*.

2. Searle's Descriptivism

Searle moves from the thesis that proper names have a sense and this is demonstrated by an analysis of the identity's relation (Searle 1958). Tullius = Tullius and Tullius = Cicero are analytic but the fact that in order to express them we must use determinate words is not trivial. Following Wittgenstein, to explain the use of a

name according to the characteristic of the object to which it refers does not mean to formulate the adequate linguistic rules: rules do not possess a descriptive content. The fact that some propositions are synthetic is however clear if, for example, someone maintains that Shakespeare is Bacon and clearly this is not a matter of the language he/she uses. Searle aims at showing how analytic and synthetic propositions are possible by means of the connection between proper names and their referents; the argumentation makes clear why a proper name has a sense. Differently from Russell, reference is not bound only to the assertion hence to the question of the existence of a certain entity. In this sense, the propositional content is common to a wide variety of illocutive acts like questions, commands, promises, etc (see Searle 1969). To grasp the identity of an object we must distinguish proper names such as "Aristotle", definite descriptions and demonstratives. Demonstratives require special conditions for the emission of the expression, definite descriptions specify certain characteristics of the objects, proper names do not specify them so, for instance, "Scott" refers to the same entity to which "The author of Waverley" refers, but in the last case some characteristics are specified (Searle 1958). Strawson (1950) showed that the referential use of proper names and definite descriptions presupposes the existence of one and the same object to which they refer and ordinary language has specific expressions that allow the speakers to identity the objects. How does reference occur? It seems important that though proper names do not normally assert or describe characteristics of the objects, still the referential use presupposes that the object possesses certain characteristics. This view can be compatible with Strawson's thesis that proper names and descriptions differ only in the degree of "descriptive meaning". At the end, descriptions seem to be relevant for the referential use because they plausibly depend on the context of expressions (speaker's intention, special and temporal coordinates, situation, identity of the speaker, etc.). This becomes clear in the Searlean thesis that referential uses of a proper name such as "Aristotle" presuppose the existence of an object of which a sufficient number of assertions is true. To referentially use a proper name means to presuppose the truth of some descriptive assertions univocally referential, but these "presuppositions" are not normally formulated or precisely indicated. Proper names have thus the function of "hooks" to which we hang descriptions. These hooks cannot be rigid and specific otherwise they would be redundant as abbreviations of necessary and sufficient criteria of the referential use, criteria that the language has not jet established (a part from, for instance, religious vocabulary). Turning back to the initial example, "Tullius = Cicero" can be considered as an analytic assertion because to each name the same descriptive presuppositions are associated; this does not imply that if they reveal

themselves false the same sentence cannot be used to perform a synthetic assertion (and so to announce a very important discovery).

3. Problems with Descriptivism

The originality and actuality of the Serlean account of proper names resides also in the relationship between philosophy and neuroscience that represents the *leitmotiv* of his work until our days and focuses on the functioning of brain processes in experience and perception. The background of perceptual, conceptual and linguistic capacities and abilities gives us an interesting view on the role of the intentional content for reference. As it is well known, several criticisms have been raised against Searle's "internalism" that gave rise to a lively and interesting debate (Lepore & Van Gulik 1991). The debates Searle conduced with Kripke, Donnellan and Putnam are useful to show how in the process of reference is always active a certain individual description. For instance, the use of the proper name "Madagascar" shows that it is difficult to isolate the causal chain that relate it to the originally reference to the African land. Originally, "Madagascar" was the name of a part of Africa. Marco Polo while satisfying the Kripke's requirement to use the name with the same reference used by the "man from whom he learned it" referred to an island in front of the African coasts (the one to which we refer as "Madagascar"). Therefore, it seems that the use of the name "Madagascar" satisfies the causal chain that goes back to the African land but not the reference to it.

Recently, some interesting criticisms have been raised against the Searlean theory of reference to overcome the primacy of the "descriptive content" (Tsohatzidis 2007). Wayne Davies (2007) presents a thoughtful analysis of Searle's descriptivism and tries to overcome several critical nodes by proposing a view of reference based on the relation between name and subject concept. The "explanation argument" maintains that a name expresses an individual concept that represents that object: «Thus "Aristotle" expresses the concept of Aristotle, which represents Aristotle. Only Aristotle satisfies that concept, so Aristotle is the referent of "Aristotle". Some would now raise a further question: *What makes it the case that the concept of Aristotle represents Aristotle?* Searle should give the same answer he gives for any other concept: the Intentional content of a mental state is an intrinsic property. For a concept to represent Aristotle is for it to *be* the concept of Aristotle. We can no more explain why the concept of Aristotle represents Aristotle than we can explain why the concept of red represents red» (Davis 2007, p. 113).

Davis individuates three forms of descriptivism in Searle's account of reference that do not allow us to establish the relationship between a name and an individual concept.

The first is represented by "The disjunctive definite description theory" that rests on the fact that when we use a name we presuppose the truth of a sufficient number of descriptions. Consequently, the disjunction of seemingly logically independent propositions is analytic even though no one of them is. Davies quotes the following argument from Searle: «A classical scholar might discover that Aristotle never tutored Alexander or that he did not write the Metaphysics; but if the classical scholar claimed to discover that Aristotle wrote none of the works attributed to him, never had anything to do with Plato or Alexander, never went near Athens, and was not even a philosopher but was in fact an obscure Venetian fishmonger of the late Renaissance, then the "discovery" would become a bad joke» (Searle 1967, p. 490).

Against the disjunctive definite description theory Davis notices that we have much evidence that Aristotle was a Greek or that he had something to do with Plato. Therefore, he proposes the alternative hypothesis that the scholar was using the name "Aristotle" to refer to someone other than Aristotle; another is that he was playing some kind of hoax. Actually, we can find a relationship between the name and "sortals" or "categories" in Searle's text *Intentionality* (on this topic see also Geach 1980, Cocchiarella 1984). But even in this case Davis underscores that the sortals Searle proposes are too specific. It is plausible that Aristotle was a "man", but it is logically possible that he was a god or a Martian or "an exceptionally sophisticated machine planted on earth by gods or Martians".

The second is represented by "The second-order definite description theory". This "cluster theory" is present in Searle's earlier work: «The individual satisfying a sufficient number of the descriptive contents users of "Aristotle" believe to be established facts about Aristotle» (Searle 1967, p. 490; Searle 1958, pp. 171-173). The objection to this option recalls the Kripkean one and says that the description is so vague that it makes impossible to establish if it is true of Aristotle. Actually, it is not an intentional content the typical sense of Aristotle would ever express.

The third is represented by "the variable definite description theory" that rests on the thesis that a name does not express a particular intentional content; rather it is used to express different descriptive contents on different occasion, all of which refer to the same object. According to Davies, this move does not convey the "sense" of a name and, in doing so, it does not account for the marked difference in meaning between "Cary Grant is Cary Grant" and "Cary Grant is Archibald Leach". Moreover, it does not provide any criterion for the use of expressions like "Santa

Claus does not exist" which does not pick out the "right object" as Santa Claus does not exist. Davies recalls the Kripkean criticism to Searle according to which it loses the rigidity required for proper names. For instance: (1) "Aristotle" refers to x in the possible world w iff x is Aristotle in w' is true for every world w' accessible from w but (2) "Aristotle" refers to x iff x is the author of *De Anima* in w is false for any possible world w' accessible from w in which Aristotle is not the author of *De Anima*.

We face another problem with modal sentences like (3) NP is NP in every possible world. In the case of descriptions, the first "NP" typically expresses a truth if it is inside and a falsehood if it is put aside. But if name are rigid designators this structural ambiguity does not affect the truth-value of the resulting statement in both interpretations. Consequently, (4) Aristotle is Aristotle in every possible word is true whether we say that the person who is Aristotle in any possible world is Aristotle in that world, or is saying that Aristotle is such that he is Aristotle in every possible world. The problem with the variable description theory is that (4) should have the semantic properties of a sentence like "The author of *De Anima* is the author of *De Anima* in every possible world" but in fact it is true for one interpretation and false for the other.

Descriptivism has been criticized also from the perspective of the philosophy of perception. For instance, Kent Bach has argued against Searle that he explains awareness of the object only "under a description" and a description is no substitute for awareness: one is aware of the object as just the "cause" of the experience. According to Bach (2007), we must give up the supposition that the content of perceptual experience determines its condition of satisfaction and he proposes an indexical view of experience. In his view an experience is "token-reflexive" not because it refers to itself but because, being perceptual, its content is context-sensitive. The condition of satisfaction is experience-relative. To experience a certain event or state of affairs is to experience it from a certain point of view. A person experiences it in a space-temporal relation to her point of view. But though she experiences it in an experience-relative way, she does not experience it under an experience-relative description. It is by having the experience at a certain time and place, with a certain orientation that the time and the place of the event or state of affairs experienced can be represented as being when and where it is. Robin Jeshon (2007) specifies the relevance of the spatio-temporal location through visual experience in the process of reference. She maintains therefore that Searle's intentionalistic account is too poor to specify the truth-conditions of indexicals. The originality of Jeshon's solution of the problem of particularity is the consideration of a rich body of recent experimental research to demonstrate that subjects are able to

consciously track the successive locations of multiple moving objects under considerations that rule out the possibility of a conceptual encoding of their changing location properties.

I think that we could profit from the distinction between "unconscious visual experience", which provides awareness of the object, and that actually cannot be satisfied and genuine "perception" that is conscious but not indexical in the sense proposed by Bach and Jeshon. Rather, using a phenomenological jargon, it is a "reflex" of the position of our body in the world. In Searle's words:

It is fundamental to the conceptual apparatus that we have evolved, and that is above all encoded in our language, that we take our physical situation, the situation of our bodies in space and time and the causal relations between our bodies and the rest of the world as a kind of fulcrum for conceptualizing our relations with the world.

We can therefore give a brief description of the relationship between perception and indexicality. Clearly there are linguistic expressions that are indexicals and indicate relations in which the objects stand to the very utterance of the expression itself. For instance, "yesterday" refers to the day before the utterance or "I" refers to the person making the utterance. Searle isolates four types of lexicalized indexical relations: spatial, temporal, utterance-directional ("I" or "we" and "you") and discoursal (like "latter" or "former"). It is easy to grasp the self-referentiality or self-reflexivity of the indexical "I" or of the visual experience. "I" in virtue of its meaning refers to the person who uttered it and if I see an object in front of me then the visual experience of the object itself figures in the conditions of satisfaction. Namely, one's visual perception is successful only if the features and presence of the object caused that very visual experience. The main difference between the two sorts of cases is that the perceptual cases are "causally" self-referential and the "indexical" cases are not.

Conclusion

On my view, the term "conceptualizing" in the last quotation does not allude to the use of concepts as Davis would prefer or as Jeshon would rather rule out. In this context, "conceptualizing" means to give a "sense" to our linguistic expressions, a sense that is strictly connected with our perceptual apparatus.

Contrary to Russell, all we can do is to give a description of the sense of our linguistic expression in the process of reference. What is relevant is to extend the Fregean account of reference that distinguishes between proper names and objects. Reference has the function to identify objects and to communicate true facts about

those objects to our interlocutors. This is the reason why Frege maintains that referring expressions have a sense. Referring expressions possess a "meaning", namely a "descriptive content" that allows the speaker to succeed in referring when he utters it. Successful reference is communication of facts (true propositions) about objects. This move presents a step beyond Frege's descriptivism because in order to give weight to propositions we must distinguish them from the sense. The sense of a referring expression is given by the descriptive general terms entailed by that expression but the sense is often not sufficient to communicate a proposition. Consequently, it is the utterance of the expression "in a certain context" (namely a pragmatic context) that communicates a proposition. For example, the expression "the dog" has the descriptive content entailed by the simple term "dog"; this very content is not sufficient for a successful reference which also requires the communication or the possibility to communicate a uniquely existential proposition (or fact, e.g. "There is one and only one dog barking on the right of the speaker and it is in the field of vision of both speaker and hearer". The classical formalization $\exists x\, Fx$ could be used to mean that "the predicate F has at least one instance" instead of "Some object is F". The meaning of this option does not establish a correspondence between the original proposition and its revised existential formulation; rather it says that the circumstances in which the one is true are identical with the circumstances in which the other is true. This thesis does not entail that the speech act of reference cannot fail like for instance when there is a discrepancy between the referent and the expression used for referring. But it is worthy to remember Frege's famous slogan "Nur im Zusammenhang eines Satzes bedeuten die Wörter etwas".

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Recensões / Book Reviews

Bachelard

by Vincent Bontems. Paris: Belles Lettres, 2010, 244 pp. ISBN 978-2-251-76068-1.

Gaston Bachelard (1884-1962) is without any doubt one of the most original and influential philosophers that worked in 20th century France. In more than 20 works spanning over three decades Bachelard expounded an original view of scientific epistemology, wrote about the psychic value of the poetic image and presented an intriguing meditation on the nature of time. This diversity of themes has led to quite different commentaries, of which there was abundance in the first years after his death, almost 50 years ago. However, in the last two decades or so interest in this stimulating thinker seemed to have declined, it is, therefore, always welcome to see new commentaries appear, and when one comes from competent hands like those of Vincent Bontems this is more so indeed.

Bontems's book, which is published by *Les Belles Lettres* in a collection *Figures de savoir*, is ostensibly a general presentation, as are other titles from the same collection. However, although the basic contours of Bachelard's thought (epistemological rupture, opposition between concept and image, epistemological obstacle, etc.) are duly presented, Bontems offers an analysis which is of a finesse that surpasses a mere introductory presentation. The structure of the book already bears this out. The basic text consists of three chapters. The first two deal with Bachelard's epistemological views and the import they have on philosophical thought. The chapters are titled 'Une épistémologie *trans*historique' and 'La relativité philosophique' and these titles already indicate the direction of Bontems's commentary. The earlier part of the first chapter deals with Bachelard's conception of history of science, however, already in the Introduction, Bontems signals one of the fundamental insights of Bachelardian thought: 'Le mathématiquement pensable *induit* le physiquement possible' (22). This is indeed a key concept in Bachelard's understanding of rationality, or more precisely 'surrationality', and Bontems fully explores the role mathematical thought plays in Bachelard's analyses of scientific thought. This is perhaps the most valuable part of the book. Bachelard is not an easy philosopher to engage with, he requires a good understanding of some of the problems emerging from contemporary physics as well as a wider philosophical context. Bachelard himself compounded the difficulty in that he did not situate

himself in the philosophical tradition; if anything, he often positioned himself against this tradition. Bontems seizes well this dimension of Bachelard's reflections on philosophy, which he points out is the consequence of the 'conflit entre les découvertes de la science et la stabilité des certitudes métaphysiques' (100). Because of this conflict Bachelard argued that philosophy should renew itself by adopting the openness of scientific rationality; there is a need for a philosophical relativity akin to relativity in the physical world. However, this does not mean the abandonment of realism, rather: 'la relativité philosophique aboutit alors à une réorganisation ontologique et non à l'abandon définitif du réalisme' (113). Bontems´s presentation of Bachelard´s position regarding the relation between scientific thought and philosophy is insightful and one reads it with great profit.

The third chapter 'Au rythme de nuit' deals with Bachelard´s writings on the poetic imagination. Bontems follows the customary division of the distinction between the 'diurne' and ´nocturne´, already signalled by Bachelard, ('on ne dort pas avec des équations dans la tête'), the first referring to scientific activity the second to poetic creativity. This strong separation between the scientific concept and poetic image has elicited different reactions from different commentators. Bruno Latour went as far as to declare Bachelard 'schizophrenic', others, on the other hand, have sought a certain unity between these two poles. The second approach is obviously potentially more fruitful and this is the one that Bontems adopts. He observes that certain concepts used in epistemological writings also appear in the works on the imaginary, such as the operator (*operateur*) or induction, and he points out a certain consistency that runs through Bachelard's thinking about the imaginary ('l'induction, les groupes de transformation formelle, l'ambivalence dynamique, la covariance du rêveur et de sa rêverie', 164). But unlike the epistemological writings that require very rigorous analyses, the writings on poetic imagination also permit a more free associative reading. After presenting the basic contours of Bachelard's thought (the elementary images, dynamic imagination, material imagination) Bontems concentrates on two images and their value for the nocturnal activity. The first is the house, of which Bachelard wrote at most length in *La Poétique de l'espace* but of which there are scattered remarks in a number of other works. The other image is wine, and Bontems evidently takes great pleasure in enumerating the frequent references to it, often nostalgic ('J'ai bu le vin du Rhin et les vins de Moselle avec, je pense, le sens délicat des hommages qu'ils peuvent recevoir d'un Champenois'). The final pages of the chapter are devoted to one of Bachelard's most complex thoughts, his 'metaphysical, writings' *L'Intuition de l'instant* and *La Dialectique de la durée.* In these works Bachelard presents a meditation on temporal discontinuity, which is pitched against Henri Bergson's notion of duration. Borrowing from an obscure

Portuguese philosopher Lucio Alberto Pinheiro dos Santos the notion of rhythmanalysis he argues that rather than viewing time as a continuous uninterrupted flow, it should be regarded as first and foremost bundles of instants that are held together through rhythms, vibrations. Bontems is not very convinced by Bachelard's arguments for temporal discontinuity but perhaps they need further exploration as they might hold the key to the understanding of the réorganisation ontologique that Bontem refers to. The chapter closes with the quote from a short 1939 text 'Instant poétique et l'instant métaphysique' in which Bachelard exhorts philosophers to meditate in order to break the attachment to duration. Bontems points out that such a call from a contemporary philosopher is most unusual and one could add that it has a very Buddhist feel about it (although Bachelard does not allude to Oriental thought).

Bontems completes the work with a chapter carrying the title *Le Bachelardisme*. It is difficult to see whether there really can be such a thing as 'Bachelardism' but the chapter is a useful survey of the influence that Bachelard exerted. It comes as no surprise to learn that in the Anglo-Saxon countries his influence is practically non-existent. One hopes this will change.

One of the virtues of the book is its succinctness. Bontems manages to convey in less than two pages (61-2) why the mechanics of Paul Dirac made an impression on Bachelard; in a matter of a few sentences (65) we learn about the essential differences (which are not clear to everyone) between Thomas Kuhn's concept of scientific revolutions and the Bachelardian 'ruptures' in scientific developments; although the differences between images of the house in Bachelard's poetics and Heidegger's mystico-réactionnare evocations of *Heimat* take a little longer to spelt out (148-54). Bontems's *Bachelard* is rich in detail, thoughtful and a very valuable contribution to our understanding of this eminent philosopher.

(Book review by Zbigniew Kotowicz)

The Scientific Revolution

by Steven Shapin. Illinois: University of Chicago Press, 1996, 232 pp. ISBN 978-0226750217.
(Trad. port. de Ricardo Afonso Roque. Lisboa: Difel, 1999, 230 pp. ISBN 978-972-2904520.)

O livro de Steven Shapin tem um objectivo polémico. Sob o título "A Revolução Científica"[1], Shapin constrói um discurso que visa defender a tese de que a revolução científica não existiu. Esta é aliás a afirmação com que o livro começa: "A revolução científica não existiu e este livro é acerca disso" (SR: 23).

Steven Shapin vem contestar aquilo que alguns historiadores da ciência não hesitaram em considerar como um processo decisivo da história da ciência, um acontecimento que, desde os finais do séculos XVI até ao século XVIII, teria transformado "de forma fundamental e irrevogável o conhecimento do mundo natural" (SR: 23). Algo que, nas palavras de Koyré, constituiria "a mais profunda revolução alcançada e experimentada pela mente humana desde a Grécia Antiga" (Koyré, cit in SR, 23).

A referência a Koyré não é ocasional. Segundo Shapin, Koyré teria sido o grande iniciador desta linha interpretativa e aquele que, em grande medida, teria sido responsável pela constituição da expressão "revolução científica" como um adquirido da pesquisa histórica. Na expressão "revolução científica" está aliás consagrada uma completa transformação semântica do termo "revolução" que, do seu sentido clássico enquanto movimento circular (por exemplo, Copérnico falava das *revoluções* dos planetas à volta do Sol), passa a designar, sob influência do Iluminismo[2], uma "reorganização radical e irrevogável" (SR: 24) num tempo linear, triunfantemente progressivo[3]. Koyré teria sido então o primeiro de uma série da historiadores da

[1] Steven Shapin, *The Scientific Revolution*, Chicago / Illinois: The University of Chicago Press, 1996, trad. port. de Ricardo Afonso Roque, Lisboa: Difel, 1999, doravante designado por SR.

[2] Segundo Margaret J. Osler (edr.) (2000), *Rethinking the Scientific Revolution*, Cambridge: Cambridge University Press, a revolução científica é um produto do espírito iluminista. Ela teria sido "construída no século XVIII quando os filósofos naturais seleccionaram a física e a matemática de Newton e ignoraram as suas investigações alquímicas e teológicas" (op. cit: 5).

[3] Noel Parker, numa obra intitulada *Revolutions and History*, 1999 (trad. port. de M.de Fátima St. Aubyn, Lisboa: Temas e Debates, 2001), que tem por objecto o estudo do fenómeno histórico da revolução, caracteriza-a por um conjunto de 5 determinações: orientação global de mudança, ruptura relativamente à ordem preexistente, impacto e transformações profundas que dela decorrem, sentimento de liberdade face à autoridade existente, presença de agentes específicos que têm a capacidade de produzir efeitos (cf. op. cit, em especial, pp. 36-49 e 71-81).

ciência que utilizam esta transformação semântica do conceito de revolução e, a partir dela, constituem uma forte tradição interpretativa da ciência moderna que dá origem a uma série de estudos históricos sobre a dita "Revolução Científica"

Ora, segundo Shapin, os *desenvolvimentos recentes da história da ciência* têm permitido questionar de diversos modos a noção de "revolução científica". Vários factores concorrem para este questionamento. Por um lado, a atenção dada pela actual história da ciência à diversidade das práticas que visam alargar o conhecimento do mundo, permitiu contestar a existência de um método científico universal e mesmo discutir a efectividade, no século XVII, de uma entidade suficientemente coerente para merecer a designação de "ciência" (cf. SR: 25). Por outro lado, o conhecimento mais aprofundado da ciência medieval e renascentista permitiu esbater o carácter revolucionário de algumas das explicações e teorizações da ciência moderna face à ciência medieval mostrando, ao invés, a existência de insuspeitas antecipações e continuidades. Em terceiro lugar, a maior atenção dada pela história da ciência aos contextos sociais, culturais e humanos fez diminuir a importância atribuída pela história da ciência tradicional às transformações conceptuais autónomas, isto é, ao que seria o aparecimento súbito de novas de ideias ou "mentalidades incorpóreas" (SR: 26). Para além dos conceitos, há que olhar as práticas de construção desses conceitos assim como as figuras, as pessoas, os agentes desses desenvolvimentos.

Segundo o autor, a forte tradição historiográfica que defende a ideia de Revolução Científica tem por base *dois equívocos fundamentais*: em primeiro lugar, o levar a sério o facto de muitos dos protagonistas seiscentistas afirmarem o seu estatuto de "modernos" e manifestarem de forma violenta a sua oposição aos "antigos" modos de pensar e fazer ciência (cf. SR: 27). Ora, uma análise histórica mais aprofundada e atenta às práticas efectivas permite mostrar que os praticantes da ciência moderna, para lá do que diziam, estavam mais próximos dos antigos do que eles próprios pensavam. O segundo equívoco diz respeito ao facto de, segundo Shapin, a historia da ciência que defende a ideia de Revolução Científica estar ainda dominada por uma orientação para o presente na base da qual faz sentido valorizar tudo o que sejam episódios revolucionários que permitiram à ciência chegar àquilo que ela é no presente (cf. SR: 28). Ora, a historiografia recente, mais lúcida quanto aos perigos da projecção retrospectiva do presente no passado, permite uma objectividade maior face ao valor que atribuímos às inovações dos "nossos antepassados" e uma consciência mais aguda das redefinições constantes a que as suas ideias tiveram que ser sujeitas para chegarem até nós.

Shapin inscreve o seu livro naquilo que entende ser uma historiografia actualizada que tenha por base "os mais recentes estudos históricos, sociológicos e

filosóficos relacionados com a Revolução Científica" (SR: 29). Nesse sentido, ele mesmo explicita os *quatro princípios historiográficos que orientam o seu trabalho* e que proponho designar por:

1. uma *perspectiva claramente contextualista*: "Eu assumo que a ciência é uma actividade (...) cujo entendimento implica relacioná-la com os contextos onde decorre" (SR: 30).

2. uma *perspectiva sociológica alargada* que encara a ciência no seu aspecto colectivo e sociológico visando "expor a elaboração e a posse do conhecimento como processos sociais" (SR: 30), isto é, que pretende ultrapassar a oposição existente entre aqueles que se dedicam ao estudo dos "factores intelectuais da ciência" (ideias, conceitos, métodos) e aqueles que sobretudo trabalham os "factores sociais" da ciência (formas de organização, influências políticas e económicas, fins sociais da ciência). Como Shapin escreve: "para muitos historiadores, assim como para mim, esta parece ser uma distinção despropositada" (SR: 30), ou seja, para Shapin há que valorizar tanto uns aspectos como outros, considerando-os ambos como processos sociais.

3. uma *perspectiva sociológica interna* que recusa identificar aquilo que a ciência tem de sociológico com o que é exterior à ciência e, pelo contrário, defende que "o interior do laboratório do cientista e o desenvolvimento interno do conhecimento científico encerram tanto de sociedade quanto o seu exterior" (SR: 31).

4. uma *perspectiva anti-essencialista* que considera não existir uma "essência" da ciência do século XVII ou "uma essência da revolução científica" ou sequer uma "história única e coerente capaz de apreender todos os aspectos da ciência".

Para além destas posições historiográficas "assumidas" (SR: 30) que Shapin enumera de 1 a 4, existe ainda uma outra posição historiográfica que me parece muito significativa. Shapin apresenta-a como decorrente do ponto anterior. Mas, em meu entender, ela deve ser pensada como um traço independente tendo em vista a sua importância para a compreensão da tendência historiográfica fundamental em que Shapin se enquadra.

Refiro-me ao *pluralismo de inspiração pragmatista* que leva Shapin a declarar que "a história definitiva e exaustiva não existe" (SR: 31) porque toda a história supõe uma operação de "selecção" realizada pelo historiador em função dos seus "interesses", razão pela qual é, portanto, "legítimo contar uma multiplicidade de histórias" (SR: 31). A partir deste momento, Shapin não poderia, naturalmente, senão reconhecer que o que o seu livro faz é apenas contar mais uma história no interior de uma multiplicidade de histórias. Shapin está consciente da fragilidade a que, por essa razão, se condena: "fico satisfeito por admitir o carácter selectivo e parcial deste

relato da revolução científica" (SR: 32). Como decidir então que a revolução científica existe ou não existe? À luz desta concepção pluralista, qualquer das versões teria a sua legitimidade enquanto uma das múltiplas histórias possíveis.

Estamos pois perante uma afirmação pela qual Shapin esvazia grande parte do conteúdo polémico que o seu livro se propõe desenvolver. No entanto, a verdade é que Shapin não faz aquilo que declara fazer. O livro de Shapin não se apresenta como apenas mais uma história da ciência do século XVII. Pelo contrário, Shapin pretende mostrar que "a revolução científica não existiu", ou seja, visa refutar as histórias da ciência que afirmam a existência da dita revolução.

E, para isso, Shapin vai elaborar uma longa *demonstração* da não-existência da Revolução Científica lançando mão de *diversos tipos de argumentos* que proponho agrupar da seguinte forma:

1. *Argumentos generalistas* – Shapin recusa o privilégio dado pela história tradicional da revolução científica à astronomia e à física matemática, disciplinas às quais a revolução científica tendia a ser reduzida. Nesta perspectiva, escreve: "a matematização do estudo do movimento e a destruição do cosmos aristotélico eram o facto 'realmente novo' e 'realmente importante' do século XVII" (SR: 32). Pelo contrário, Shapin propõe dedicar uma "atenção intermitente a um largo espectro de ciências" (SR: 32). Compreende-se a eficácia do argumento: na medida em que presta atenção a outros domínios da actividade científica para além da astronomia e da física matemática, Shapin desvaloriza, ou pelo menos relativiza, as transformações operadas na astronomia e na física matemática, transformações essas nas quais os defensores da revolução científica tendiam a colocar a pedra de toque (ou a essência) da revolução operada pela ciência moderna[4].

2. *Argumentos anti-uniformistas* – Shapin defende com grande vigor a não unanimidade de crenças, posições teóricas e programas metodológicos vigentes no século XVII entre os filósofos naturais. Como escreve: "nem toda a filosofia natural do século XVII era mecanicista ou experimental e, entre as versões que abraçavam o mecanicismo e a experimentação, disputava-se o âmbito preciso de cada corrente de conhecimento" (SR: 32). O cerne do argumento é claro: na medida em que se assiste a uma diversidade das crenças, variedade dos métodos ou mesmo heterogeneidade das concepções teóricas em presença, Shapin esbate a unicidade e a coerência do que poderia ser a revolução científica dos tempos modernos.

3. *Argumentos culturalistas* baseados no reconhecimento da variedade de práticas, figuras e personagens que protagonizam de forma efectiva e muito

[4] Não é esta no entanto a posição de Margaret J. Osler (2000) que considera que as histórias tradicionais da revolução científica se ocupam, não apenas da astronomia e da física matemática, mas também da anatomia, da fisiologia e da química (cf. op. cit: 3).

diversificados os desenvolvimentos da ciência moderna. Trata-se agora da pluralidade, não das aproximações teóricas ou metodológicas características da filosofia natural do século XVII, mas das práticas e personagens efectivas que dela foram responsáveis. Mais uma vez, o argumento passa por dissolver a "essência" da revolução científica numa pluralidade de procedimentos particulares de observação e experiência e de modos institucionais de validação e comunicação das inovações e resultados produzidos.

4. *Argumentos continuístas* que têm por base o reconhecimento das linhas de continuidade conceptuais entre a ciência moderna e a ciência anterior.

Na sua interligação, este conjunto de argumentos permite a Shapin olhar de modo diferente "*o que* era conhecido sobre o mundo natural nos tempos modernos, *como* era assegurado esse conhecimento e *que propósitos* ele satisfazia" (SR: 33). Assim se explica a estrutura triádica do livro: *o quê, como e porquê.*

O que é curioso é que, no *final da introdução*, isto é, exactamente antes de entrar na apresentação dos três capítulos em que o livro está organizado, Shapin tenha necessidade de fornecer a seguinte explicação: "Afirmei que não existia nenhuma essência da revolução científica – nalguns momentos, porém, critérios pragmáticos induzem-me por vezes a oferecer uma descrição artificialmente coerente das transformações específicas do conhecimento natural" (SR: 33).

Shapin vai mais longe e oferece mesmo uma síntese das "mudanças ocorridas no conhecimento do mundo natural e nos meios de o certificar" (SR: 33) a partir de quatro determinações: 1) o desenvolvimento de uma perspectiva mecanicista que se traduz na "crescente utilização de metáforas mecânicas para interpretar os fenómenos naturais" (*ibid.*); 2) a aproximação que na ciência moderna se verifica a um conhecimento de tipo impessoal, donde decorre "o alargamento da separação entre os sujeitos humanos e os objectos naturais do conhecimento" (*ibid.*); 3) a constituição de uma nova metodologia de investigação, que passa pela "proposta de desenvolver regras de método, formuladas explicitamente, de forma a disciplinar a produção do conhecimento (*ibid.*)"; 4) a colocação da ciência ao serviço da humanidade, "a aspiração a utilizar conhecimento natural para fins morais, sociais e políticos" (*ibid.*).

Só com muito boa vontade podemos aceitar que estes quatro elementos não definem o perfil do que seria a "essência" da revolução científica que Shapin diz não ter existido. É como se Shapin estivesse a prevenir o leitor para as subsequentes contradições (ou pelo menos, ambiguidades) do seu próprio livro.

Acompanhemos os desenvolvimentos do livro de Shapin. Aí encontraremos exemplos dessas contradições ou, pelo menos, do carácter afinal conciliador da tese de Shapin.

Capítulo I: O que era conhecido?

O capítulo I examina alguns dos tópicos clássicos da história tradicional sobre a revolução científica. E, de forma surpreendente, Shapin não se coíbe de assinalar as rupturas produzidas pela ciência moderna, de mostrar os desafios por ela colocados à filosofia natural aristotélica:

– desafios face à estrutura dualista do mundo natural herdada de Aristóteles: "As opiniões de Galileu sobre as manchas solares, acompanhadas de um conjunto de outras observações e teorizações, questionaram profundamente uma distinção fundamental do aristotelismo: a separação entre física celeste e física terrestre" (SR: 38);

– desafios ao modelo antropocêntrico do cosmos Ptolemaico: "este antropocentrismo foi atacado em pontos fundamentais pelos filósofos naturais do século XVII e finais do século XVI que aceitaram e desenvolveram as concepções de Copérnico" (SR: 45);

– desafios à concepção grega de um universo finito: "só nos séculos XVI e XVII as dimensões estabelecidas da experiência comum foram desafiadas seriamente pela infinitude cósmica" (SR: 46);

– desafios às concepções animistas e teleológicas características da física aristotélica: "os novos filósofos da natureza de Seiscentos reconheceram nestes elementos teleológicos e animistas o sinal do carácter absurdo e ininteligível da física tradicional do movimento. Aquilo que, durante séculos, permitiu à física dominar o senso comum era agora visto como um sinal da sua inadequação" (SR: 50);

– desafios à concepção herdada dos gregos de uma ordem divina da natureza orientada por causas finais, em favor do estabelecimento de uma filosofia mecanicista que pensa o mundo natural "como se fosse uma grande peça de um mecanismo do relógio" (Boyle, cit. in SR: 53). Shapin mosta como, sob o modelo do artefacto, sob a metáfora do relógio, isto é, da máquina isenta de qualquer tipo finalidade oculta, governada unicamente por causas materiais acessíveis ao inteligência humano: "todas as descrições mecanicistas do século XVII se opunham à tradição que atribuía à natureza e aos seus componentes e capacidade de promover objectivos, ter intenções, ou ser sensível" (SR: 57). Contrariamente, "tudo o que integrava o mundo natural viria a ser explicado por referência às propriedades irredutíveis da matéria e aos seus estados de movimento" (SR: 64);

– desafios ao valor da experiência vulgar e do senso comum e afirmação da importância epistemológica da observação e da matemática como estrutura da realidade, linguagem do mundo e instrumento do seu conhecimento abstracto e

unificado: “Os filósofos naturais modernos, e não só os da variante mecanicista e corpuscular, concordavam, em geral, que a matemática era a forma de conhecimento mais exacta e por isso uma das mais importantes” (SR: 75).

Verificamos pois que, ao longo de todo o capítulo, Shapin sublinha as novidades introduzidas pela ciência moderna. Para além de salientar que “todos os dias surgiam novos fenómenos sobre os quais os velhos textos nada diziam” (SR: 40), de chamar a atenção para a importância dos novos tipos de observação instrumental entretanto produzida (telescópios, microscópios, relógios, barómetros), de destacar a importância da constituição de novos modos de produção artificial de experiência, Shapin não se coíbe de usar expressões como “nova maneira de pensar o mundo” (SR: 39), “novo modelo físico do cosmos” (SR: 43), “nova astronomia” (SR: 45), “nova física” (SR: 45), “novos filósofos da natureza” (SR: 50), “nova prática” (SR: 61), e mesmo “nova ciência” (SR: 79). Referindo-se à lei da gravitação universal de Newton e à capacidade de matematização que ela traduz, Shapin escreve mesmo: “Assim se dava um gigantesco passo em frente no movimento de homogeneização e objectivação do mundo natural” (SR: 78)

Cabe assim perguntar: em que é que a história da ciência moderna contada por Shapin se afasta da história tradicional para a qual a Revolução Científica é um conceito pertinente? De que modo é que, neste primeiro capítulo, isto é, ao nível daquilo que era conhecido, Shapin se opõe à tese da existência da revolução científica?

Aparentemente nada. No entanto, se olharmos com cuidado, podemos perceber que os dispositivos polémicos do livro estão presentes e se vão desenvolvendo, embora de forma pouco explícita. De acordo com o programa argumentativo acima referido, a estratégia de Shapin consiste em, sempre que possível, reconhecer linhas de continuidade entre a ciência moderna e a ciência anterior. Por exemplo, relativamente à matematização que a ciência moderna leva a cabo, e na qual, em grande parte, a história defensora da revolução científica faz residir o “auge da revolução científica” (SR: 77), Shapin não se esquece de referir que “os filósofos naturais modernos viravam-se para Pitágoras e, em especial, para Platão, de forma a legitimar um tratamento matemático do mundo” (SR: 75). Nesse mesmo sentido, Shapin sublinha a permanência de teses tradicionais no espírito de muitos filósofos naturais (por exemplo, a permanência da tese da repulsa da natureza pelo vácuo (cf. SR: 57- 60)).

O aspecto mais determinante é contudo aquele que leva Shapin a realçar a não-unanimidade de crenças e posições teóricas existente entre os praticantes da ciência moderna (por exemplo, a ambiguidade de posições relativamente ao

naturalismo renascentista e à tese da passividade da matéria (cf. SR: 61-63)), a salientar "as importantes divergências de opinião" que separam as diversas figuras e personagens que protagonizam a ciência moderna (por exemplo, a diversidade de posições sobre o estatuto da matemática no estudo dos fenómenos naturais (cf. SR: 75-77), ou mesmo a denunciar a existência, numa mesma personagem, de contradições internas. O caso de Newton e da sua posição relativamente à causalidade física é a este título exemplar. De tal modo que Shapin pergunta se as realizações de Newton devem ser encaradas "como epítome da filosofia mecanicista ou como a sua subversão mediante a reintrodução de qualidades ocultas, ou ainda como a criação de uma nova prática a apreciar segundo novos padrões filosóficos" (SR: 80).

Em qualquer caso, o principal argumento de Shapin contra a revolução científica consiste, não em negar a novidade dos seus adquiridos, mas em fazer explodir a unicidade e coerência das novas concepções e programas teóricos propostas pela ciência moderna. Ou seja, o que Shapin está a dizer é que, ainda que tenham ocorrido mudanças significativas *no que era conhecido* acerca do mundo, ainda que se tenha dado "um gigantesco passo em frente" (SR: 78) no conhecimento do mundo natural, não houve revolução científica porque os resultados obtidos não eram uniformemente respeitados nem eram isentos de variações significativas (argumento 2, acima designado por *anti-uniformista*).

O capítulo II: Como era feito o conhecimento?

"Nada marcou tanto a 'nova ciência' do século XVII quanto a insistência dos seus proponentes em proclamar a sua novidade" (SR: 81). É com esta afirmação que Shapin inicia o segundo capítulo do livro. O objectivo é chamar a atenção para o facto de, sob a oposição entre os "auto-denominados modernos" e a "poderosa comunidade dos Seiscentistas antigos" (SR: 81), ser necessário reconhecer a existência de fortes linhas de continuidade. Denunciando a retórica do "ímpeto modernizador" (SR: 82) presente, tanto no projecto de Bacon como no de Descartes de "reconstrução total das ciências, das artes e de todo o conhecimento humano" (SR: 82), Shapin afirma com vigor a sua profissão de fé continuista: "Nenhuma casa se constrói a partir de materiais virgens, segundo um projecto totalmente diferente das antigas estruturas (...), nenhum corpo de cultura é capaz de rejeitar por completo o seu passado. Não é assim que funciona a mudança histórica. A maioria das revoluções provoca menos mudanças fulminantes do que apregoa" (SR: 82).

Este é um ponto fundamental da argumentação de Shapin (argumento 4, acima designado por *continuista*). O autor ilustra-o através de alguns exemplos: a "nova" astronomia de Copérnico não rejeitou a tese da circularidade das órbitas dos planetas de Aristóteles (cf. SR: 82); os astrónomos do século XVII "dependiam em última instância dos dados de observação compilados pelos antigos" (SR: 83); "o próprio Copérnico afirmava que o heliocentrismo era uma visão antiga" (SR: 83); a "nova" anatomia de Vesalius retomava muito da "velha" medicina grega (Cf. SR: 83); Newton considerava que "a sua tarefa implicava a recuperação da desaparecida sabedoria dos antigos" (SR: 90), etc.

A conclusão de Shapin não é, no entanto, tão radicalmente continuista como se poderia pensar. Pelo contrário, é bastante conciliadora. Num livro que tinha por objectivo mostrar que "a revolução científica não existiu", Shapin acaba por concluir: "a revolução científica foi algo de significativamente novo – mas só até certo ponto" (SR: 83). Ou seja, houve aspectos novos mas houve também linhas de continuidade.

O aspecto que Shapin convoca para ilustrar esta conclusão é interessante. De acordo com a retórica dos "modernos", a "nova" ciência distinguia-se da "antiga" porque deixa de se fundar na autoridade dos livros e passa a construir-se com base na "autoridade da razão individual e no testemunho da realidade natural" (SR: 83), isto é, na razão e na experiência. No entanto, como mostra Shapin, as continuidades são mais fortes do que poderia parecer. São elas que fazem com que os "modernos" se não tenham autonomizado completamente da figura do livro. Assim se explica a importância que é dada pelos "novos" à metáfora da Natureza enquanto Livro. Dos livros dos homens, passa-se agora ao grande Livro da Natureza. Ou seja, Shapin assinala elementos de continuidade: a ciência continua a ser feita a partir dos livros. Mas há também elementos de novidade: o livro da Natureza não foi escrito pelos homens. Está escrito em linguagem matemática (Galileu), repleto de hieróglifos (Boyle), que cabe ao homem decifrar em função, não da autoridade dos antigos livros, mas da autoridade da sua razão e do testemunho dos seus sentidos. As grandes novidades são epistemológicas: a matemática é a chave do entendimento do mundo e é na razão individual e na experiência directa dos sentidos que a ciência deve buscar o seu fundamento.

No entanto, como também mostra Shapin, "a retórica deste individualismo empirista não era nem pacífica, nem óbvia" (SR: 87). Assim, o episódio do professor de Pisa que se recusou a espreitar pela Luneta de Galileu não deveria ser liminarmente "condenado como absurdo", como faz a história da ciência que defende a existência da revolução científica. Essa condenação é vítima de um presentismo inaceitável que leva a interpretar o episódio referido como sinal de uma incapacidade grosseira, quase ridícula, da ciência "antiga" face à abertura, à novidade e ao

progresso da ciência "nova". Ora, pelo contrário, Shapin defende que esse episódio deve ser entendido como uma legítima recusa dos sábios antigos face a um novo procedimento instrumental de investigação que, por muito boas razões, lhes aparecia como precário e não merecedor de qualquer confiança. Por outras palavras, uma historiografia não presentista tem de compreender que "nós pertencemos a uma cultura que já garantiu a confiança nestes instrumentos (quando utilizados da devida maneira), que já decidiu por nós qual o género de fenómenos cuja existência é autentica nos domínios do muito distante e do muito pequeno, uma cultura que forneceu estruturas de autoridade dentro das quais podemos ir aprendendo o que ver (e o que não ver)" (SR: 88).

Mais uma vez, fica clara a posição conciliadora de Shapin relativamente ao carácter revolucionário das novas práticas de investigação instrumental características da ciência moderna, aspecto este tão reclamado pelos defensores da revolução científica: "embora seja correcto dizer que a experiência celeste mediada por instrumentos desempenhou um papel importante na avaliação das teorias astronómicas, é essencial compreender o quão precária poderia ser essa experiência e o imenso trabalho necessário para a constituir como digna de confiança" (SR: 88).

Nesse sentido, isto é, como forma de sublinhar a íntima articulação entre o "velho" e o "novo" que está incorporada na ciência moderna, Shapin convoca ainda dois argumentos de tipo contextual (argumentos 3/ acima designados por culturalistas), mostrando de que modo a prática renascentista e humanista de regresso às fontes da cultura clássica e, portanto, de análise directa dos textos antigos terá facilitado a prática da nova ciência observacional (cf. SR: 90). Prática essa que, segundo Shapin, terá ainda sido reforçada pela doutrina reformista do livre exame e consequente leitura individual da Bíblia, e ainda pelo alargamento da experiência proporcionado pelas descobertas (cf. SR: 92).

Ao longo do capítulo 2, Shapin oferece outros exemplos da sua posição conciliadora relativamente à existência (ou não-existência) da revolução científica. É essa posição conciliadora que o leva a sublinhar:

– a complexidade das múltiplas posições existentes nos tempos modernos quanto ao valor da experiência enquanto alicerce do conhecimento científico, de uma experiência espontânea ou directa dos sentidos face "aquilo que acontece no mundo" (SR: 97), a uma experiência artificialmente produzida, intencionalmente orientada e que, em alguns casos, supunha mesmo a realização de "experiências imaginadas" (SR: 96);

– a diversidade de perspectivas relativamente ao progressivo desenvolvimento de uma prática de experimentação instrumental, com tudo o que isso implica de

invenção, de adaptação e aplicação de "novos" instrumentos e de discussão sobre o valor das observações assim produzidas;

– a multiplicidade de pontos de vista relativamente ao lento apurar de práticas de realização de protocolos experimentais que visavam observar "fenómenos que não poderiam ser observados (ou pelo menos não o seriam com facilidade) no curso normal da natureza"(SR: 98)

Em cada caso, Shapin vai mostrando como esses exemplos de novidades produzidas pela ciência moderna admitiam uma diversidade de posições (argumento 2, acima designado por *não-uniformista*), e como, na complexidade das suas formulações, esses exemplos também dão conta de continuidades relativamente à filosofia natural aristotélica (argumento 4, *continuista*). Como Shapin escreve, "não é verdade que a filosofia natural do século XVII não possuísse recursos para se entender com a nova experiência fornecida pela experimentação artificial ou pelos instrumentos científicos. Também não é verdade que os quadros aristotélicos de pensamento tenham definhado mal aparecerem as alternativas modernas" (SR: 98)[5].

A mesma situação se verifica, segundo Shapin, relativamente ao estabelecimento de novas metodologias. Embora haja unanimidade quanto à importância do método na constituição da "nova" ciência, há grandes divergências quanto à natureza desse método. Na verdade, uma coisa é o empirismo estrito das tábuas de Bacon que sobretudo visam o registo adequado e minucioso dos factos com vista a permitir uma indução o mais rigorosa possível (cf. SR: 101-104); outra coisa é o experimentalismo de Boyle e Hooke que preconizava a "correcção das enfermidades sensórias" (SR: 105) pelo efeito de uma razão capaz de "guiar os sentidos pelo conhecimento" (SR: 106). Razão apoiada na utilização de instrumentos que permitiam estudar fenómenos "a qualquer altura, em face de quaisquer observadores, sem ter de esperar que os fenómenos ocorressem de forma natural" (SR: 109) e que ampliavam de tal modo as capacidades humanas que tornariam possível ver, estudar, quantificar o invisível (por exemplo, o vácuo, ou a pressão atmosférica)[6]; outra coisa ainda é o matematicismo de Newton. Para lá da descrição minuciosa da natureza, do "registo permanente" (SR: 119) dos fenómenos naturais e

[5] Shapin dá dois exemplos fortes: o papel dos jesuítas na elaboração de "técnicas sociais e linguísticas que conferiam à experiência particular a aura de certeza que a prática filosófica aristotélica julgava necessária" (SR: 98) e a articulação necessária entre o experimentalismo da Royal Society e a tese Aristotélica da identidade entre Natureza e Arte. Como escrever Shapin, "as manipulações experimentais envolvendo máquinas não podiam substituir o modo como as coisas eram na natureza, a não ser que se aceitasse existir uma semelhança básica entre os produtos da natureza e os produtos inventados pelos humanos" (SR: 108).

[6] Como Shapin refere, a bomba de Boyle tornava "acessíveis e manifestos os efeitos invisíveis do ar, efeitos que normalmente não seriam sensíveis" (SR: 109).

artificiais característicos do programa experimental de Boyle, a teoria de Newton arriscava inferir e formular matematicamente uma estrutura causal invisível para os factos manifestos. Como escreve Shapin, "a uma concepção de ciência teoricamente cautelosa e baseada na experiência (a de Boyle) justapunha-se aqui uma outra (a de Newton), a qual utilizava os instrumentos matemáticos, bem como os experimentais, para proclamar uma certeza teórica" (SR: 127).

Mais uma vez, é o argumento 2, acima designado como *não-uniformista* que está aqui a funcionar. Relativamente ao modo *como era feito o conhecimento*, a revolução científica não existiu porque, embora houvesse unanimidade quanto à importância da experiência e do método na constituição da "nova" ciência, havia grandes divergências quer quanto à natureza, quer quanto ao estatuto dessa "nova" experiência e desse "novo" método.

O capítulo III: Para que servia o conhecimento?

Finalmente, o capítulo III tem por objectivo o estudo das transformações operadas ao nível dos objectivos atribuídos à ciência no século XVII. Qual a sua utilidade? A que fins se destinava? Para que se pensava que esse conhecimento era necessário?

A primeira observação de Shapin diz respeito à necessidade de se ultrapassar a mera declaração geral de intenções segundo a qual a revolução científica tinha por objectivo alargar o conhecimento do mundo. De acordo com os seus princípios historiográficos contextualistas e sociológicos, Shapin pretende encontrar as práticas efectivas por detrás das declarações gerais. Como escreve: "é necessário partir em busca dos propósitos que distinguiam cada tipo de prática e das várias situações nas quais se davam a entender" (SR: 129).

Os argumentos apresentados por Shapin para explicar os novos objectivos atribuídos à ciência na Idade Moderna, nomeadamente, a sua maior proximidade ao poder do Estado, são sobretudo de ordem contextual, histórica e política. Shapin argumenta que, face à ruptura da ordem feudal, a emergência dos estados-nação é acompanhada pelo desenvolvimento de "relações cortesãs de mecenato" (SR: 135). Os príncipes percebem que a protecção de matemáticos, astrónomos, médicos, filósofos, etc., lhes dava notoriedade cultural. Além disso, compreendem que a ciência lhes podia trazer vantagens militares e económicas, por exemplo, aplicações da astronomia nas artes da navegação ou aplicações militares da balística e da

metalurgia (cf. SR: 136)[7]. Paralelamente, Shapin defende que as academias científicas, que começaram a florescer por toda a Europa em meados do século XVII, umas sob protecção de mecenas, outras com apoio estatal (Academia da Paris), se constituem como instituições de elite, compostas por "indivíduos de posição social elevada" (SR: 142)[8] e, portanto, como poderosos veículos da relevância social e política que o conhecimento natural vai progressivamente adquirir.

No que diz respeito às relações entre a ciência e a religião, Shapin defende que, para lá de alguns episódios circunstanciais (entre os quais se conta a condenação de Galileu), em geral, a filosofia natural, sobretudo nos países protestantes, se apresentava como capaz de reforçar fé e fortalecer a religião. Como escreve, "Galileu sustentava que o Livro da Natureza escrito por Deus deveria partilhar pelo menos do mesmo estatuto de fonte de verdade atribuído às escrituras e que, em consequência, se deveria reconhecer aos filósofos naturais um estatuto pelo menos equivalente ao dos exegetas dos livros divinos" (SR: 145). Um significado similar deve, segundo Shapin, ser atribuído às palavras de Bacon quando este afirma que "a seguir à Palavra de Deus, a filosofia natural é, ao mesmo tempo, a mais eficaz medicina contra a superstição e o mais autorizado alimento da fé" (cit in SR: 147). De modo paralelo, a filosofia mecanicista, longe de esvaziar o lugar de Deus, levava a considerar que "a natureza exibia provas sólidas de ter sido projectada de forma engenhosa" (SR: 150). O caso da física celeste de Newton é apresentado por Shapin como eloquente: "no cerne do sistema newtoniano residia uma concepção voluntarista da actividade de Deus na Natureza" (SR: 159).

Para além desta utilidade da ciência para "restaurar um cristianismo puro e primitivo" (SR: 147) e fortalecer a fé cristã, Shapin sublinha ainda o facto de os praticantes da ciência moderna estarem animados de efectivos propósitos práticos: controle da natureza, utilidade social, fertilidade tecnológica. Recorrendo a um argumento de tipo 1, acima designado por *generalista*, isto é, não relativo unicamente à história da astronomia e da física matemática, Shapin oferece o exemplo de Descartes cuja medicina mecanicista visava explicitamente: "libertar-nos de uma infinidade de doenças do corpo e da mente" (cit in SR: 148). Sublinha no entanto que, para lá dos propósitos utilitaristas dos filósofos naturais, a ciência moderna "não se traduzia, automaticamente, em consequências económicas substanciais" (SR: 149).

[7] Esta ligação da Ciência ao Estado teria sido consagrada por Francis Bacon na sua proclamação célebre segundo a qual "o conhecimento e o poder humanos são uma e a mesma coisa" (cit in SR: 139).

[8] Como escreve Shapin; "a empresa do conhecimento natural atraía os *gentleman* de vocação cívica e ajustava-se-lhes" (SR: 142).

O que, a seu ver, não significa que as ligações entre ciência e poder se não tenham reforçado durante a modernidade.

Este 3.º capítulo, relativo ao *para que servia o conhecimento* é claramente o menos conclusivo. É difícil perceber onde quer chegar Shapin. Usando sobretudo argumentos contextuais e sociológicos, Shapin mostra que, para lá de objectivos gerais de alargamento do conhecimento, a ciência moderna desenvolveu um conjunto de propósitos específicos (políticos, militares) que consagram a sua aproximação ao poder do Estado. Mostra também que a filosofia natural mecanicista, ao invés de se constituir como oposição à religião, se pensou sempre como capaz de reforçar a fé e fortalecer a crença na acção de Deus sobre a Natureza. Shapin defende ainda que, ao contrário dos sábios medievais e dos eruditos professores das universidades, os praticantes da ciência moderna estavam animados de efectivos propósitos práticos de domínio da natureza, de utilidade social, de produção técnica.

Será que estes traços fazem parte da revolução científica que Shapin diz não ter existido? Nada é explicitamente dito sobre o assunto. No entanto, a jeito de conclusão, Shapin convoca o tema da despersonalização da natureza e do conhecimento. E, depois de mostrar que a concepção de ciência como conhecimento desinteressado se desenvolveu no século XVII, afirma que este aspecto constitui um dos principais motivos que leva a história defensora da revolução científica a declarar que é nesta época que o mundo moderno tem o seu começo (cf. SR: 168). Ou seja, o carácter desinteressado da ciência moderna é considerado decisivo pelos defensores da revolução científica. Porém, ao contrário daqueles que defendem a revolução científica e que vêem nela o começo de uma concepção desinteressada do conhecimento, Shapin defende que o conhecimento natural da modernidade, não só era interessado, comprometido com o poder político, com a religião e com o utilitarismo da vida social em que se inscrevia, como era assegurado por um conjunto de procedimentos metodológicos precisos mediante os quais os praticantes da ciência moderna aprenderam a produzir conhecimento objectivo, isto é, esvaziado das paixões e dos interesses particulares dos seus praticantes (cf. SR: 167-168).

Na conclusão, ou melhor, nas páginas finais do capítulo III com que o livro se encerra, Shapin salvaguarda o facto de o seu livro não ser uma crítica da ciência mas de "certas histórias sobre a ciência, bem difundidas, que é frequente ouvirmos contar" (SR: 170). Porém, *na introdução*, depois de ter afirmado o "carácter selectivo e parcial" (SR: 32) da sua interpretação da revolução científica, ou melhor, da sua não existência, e de, nesse sentido, ter afirmado a impossibilidade em que se encontra o historiador de ultrapassar os seus próprios interesses e de deixar de marcar

subjectivamente as histórias que conta sobre o passado[9], Shapin termina manifestando o desejo que está subjacente ao seu livro: "fazer ressuscitar a história" (SR: 35).

Estamos perante mais uma contradição, ou pelo menos, mais uma das ambiguidades que habitam este livro. Se elas são a marca dos inegáveis limites da obra, são porventura também sinal da sua originalidade.

(Recensão feita por Patrícia Medeiros
patpombo@yahoo.com)

[9] "Aquilo que seleccionamos representa inevitavelmente os nossos interesses mesmo que, a todo o momento, aspiremos a contar exactamente como foi. Isso quer dizer que há sempre algo de 'nós' nas histórias que contamos sobre o passado. É esta a condição do historiador e não faz sentido pensar que existe algum método, por mais bem intencionado que seja, que nos possa dela libertar" (SR: 31).

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