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Membranes Surfaces Boundaries

Interstices in the History of Science,
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Introduction – "Membranes, Surfaces, Boundaries"

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Surfaces, Membranes, and Boundaries, one should think, belong to the more intimately familiar things. Not unlikely that you will be reading this text on a screen composed of liquid crystals or sheets made up of cellulose (familiar things indeed); venture outside, and most likely your gaze will not penetrate much further than a great many façades, Schaufenster, and streets paved with greyish asphalt. Take any object, in fact – it is arguably only in virtue of its surface – reflecting, vibrating, separating, or transducing – that it will make itself noticeable in the first place. "From the phenomenon of plant osmosis to the ordinary straining of breakfast coffee," as the allegedly first text book on industrial filtration noted in 1923 (somewhat nonplussed), surfaces are active and surrounding us "constantly on every side, although [they are] seldom noticed or appreciated as such" (Bryden and Dickey 1923).

No doubt about it: the world, more often than not, is and has been appreciated in its compactness, as stuff, things, and objects; far less so, in its interstices. Science, technology and culture are permeated and traversed by boundary phenomena, of course. From the materialities of life itself – whether cellular membranes, skin, immune-systems or ecological habitats – to surface, separation and purification processes in chemistry and industry to the making, processing and exhibition of photographs and films, things coalesce(d) at surfaces. They are palpable as well in the history of geography and politics, of urban and private spaces, of literature, art, psychology and the self; or again, as interfaces in contemporary media theory (and Western lives). They are, so much seems true, "constantly on every side".

But is there anything that unites these – evidently most diverse and slightly peculiar – "things", family resemblance and metaphoric resonances apart? Or, more interesting perhaps (and certainly less metaphysical) – profound semblance or not, to what kind of questioning, to what kind of novel historical perspectives might this special class of phenomena lead us? Such, at any rate, were the questions intensely occupying the minds of a small group of scholars – and a few artists and a (considerable) number of exhibition-goers, too – on three consecutive days in October 2010. They had come together in Berlin following our invitation to think about their work in relation to this very theme, Membranes, Surfaces and Boundaries; and in particular, to think about this as a matter of materiality, intersections and sites of exchange (and hence, of non-disciplinary histories).

For, clearly enough, there is nothing especially abstract or metaphoric about these seldom noticed and appreciated phenomena as such. Nor do most of them share the intellectual and aesthetic sex-appeal of those sleek and shiny interfaces constantly surrounding us (First-Worlders) in particular (just think of, say, "industrial filters" – rather than "video"): surfaces, membranes, boundaries and the like tend to exhibit quite definite levels of concreteness, exerting their omnipresent, indeed mundane agency in virtue of their specific, material compositions and constitutions. At the same time, few concepts would seem to display similar degrees of flexibility and mobility, forever resurfacing, as it were, in other sites and contexts. Consider films – in defiance of common expectations, this was and is a topic rarely concerned with "the cinema" only, as the British colloid chemist William Bate Hardy once noted, but with the exceedingly complex and in any case essential "films spread over the surface of each living cell" and 'those thin films of matter, familiar to all in the form of soap bubbles or lubricating films of oil' (Hardy 1936).

Surfaces, then, perhaps tend to be both: abstract and concrete, thing and non-thing, real and imagined, visible and invisible, spectacular and most prosaic; more interestingly even, or so it would appear on somewhat closer inspection, they in this way tend to be ontologically elusive, not unambiguously belonging to any one particular domain. Certainly no single discipline, field of knowledge or locale, as Hardy above reminds us, could make claims to being the sole purveyor of "films". Much the same, of course, is true of those innumerable membranes, surfaces and boundaries – solid, porous, virtual and otherwise – that connect, separate and delineate things as well as places and people. And yet, very little historical and theoretical work has been done in terms of thinking, or at least bringing these various surface phenomena together, notwithstanding widely floated notions such as boundary objects or liminality, hailing from science studies and elsewhere; or, their occasional and often enlightening treatment by, say, historians of culture and art; or again, the growing literature in the history of science on things and their materiality.[1] Or that such was regrettably the case, that surface phenomena never quite received the kind of sustained attention they perhaps deserved, was the impression the three of us had when the idea for the workshop Membranes, Surfaces and Boundaries – documented in this preprint – first flickered up sometime in early 2009. It then seemed to us (and indeed it turned out to be) an immensely rich and promising sphere of historical phenomena poised to make a great diversity of scholars speak to one another – on the level of things and phenomena, rather than theory.

Our small venture into this challengingly heterogeneous topic began when Mathias Grote presented his research on membrane biology in the 1970s at the Max-Planck-Institute in June 2009. Laura Otis, one of those present, was naturally inclined to the topic, having written a well-known book on Membranes already some ten years ago. Then, she had analysed their role as metaphors of in- and exclusion in 19th century science and literature. Another attendant, Max Stadler, also was deeply immersed in matters of surfaces at the time, then being in the final stages of his PhD thesis on the sciences of the cell in the first half of the 20th century. A rare occasion, we thought, to have three people in only one room working on such a topic, and we quickly discovered that we would be hard-pressed to name a great many more. It appeared obvious that something had to be done and that the topic, if not the general theme (whose dizzyingly multifarious dimensions were soon dawning on us), might perhaps be brought together in a workshop devoted to this ontologically rather peculiar class of objects: Surfaces.

The idea quickly found its generous and enthusiastic supporter in Hans-Jörg Rheinberger (whom we would like to thank at this point), and, despite a quickly growing wish-list of themes and a maturing little agenda, we went ahead drafting a call for papers, strategizing that this would be the most productive way to proceed in a matter in which, after all, we had only some fairly vague intuitions as to what work might be potentially most relevant or what was in fact already being done.

As to the former, our wish-list, the three of us were naturally interested in anything pertaining to the biological cell, a historiographically rather neglected object – especially when it comes to cultural histories, and especially when it comes to the 20th century. Hannah Landecker's work on tissue culture obviously marks a notable exception in this regard (Landecker 2007, 2006); and (in a tiny departure from our recruitment policy) we therefore attempted, and succeeded, to make her come all the way from Los Angeles to Berlin as our keynote speaker. Many thanks to her. Few other scholars came to mind, however. Instead, we rather soon discovered that, in fact, the cell is far from the only such neglected surface-thing. There were the odd, sometimes highly stimulating, discussions of such things as skin, windows or photographs, of course. But what, for example, about immensely important and huge fields such as electro- and surface-chemistry or chemical

1 Among the studies we found particularly suggestive/valuable in this conncetion were Landecker 2006, Schaffer 2004, Schleif 2004, Ward 2001.

engineering? Did anyone even care? Topics such as filtration, osmosis, surface catalysis or plastic wraps? Lacquers, coatings and lubricants; sedimentations, pigments, silicon wavers, mirrors, CRT screens, or the ozone layer (did it still exist)? We were not able to come up with a great many histories dealing with these topics in their own right, let alone in their at times palpable entanglements with, say, the biological sciences or laboratory practice. Or again, we found treatments of our subject within film and media studies were often oblivious of the materialities of surfaces and generally not in dialogue with respective treatments in the history of technology, industry and economy.

This in any case was the picture which gradually emerged as the common thread in our preparatory discussions, our agenda taking shape accordingly: to recalibrate the balance between the concrete and the metaphoric in matters of "surfaces" as much as that between the more fanciful and down-to-earth. At the same time, however, we very deliberately refrained from pushing too far into the direction of yet another theoretical programme or putting on the map some kind of surfacial ontology.

Among the numerous interesting proposals we received following our call for papers, some treated on the more predictable themes in this connection, others fortunately not at all. A selection had to be made, at any rate, and though we had to regretfully omit a great many promising candidates, soon a (to us) very meaningful and coherent arrangement of themes emerged almost naturally. Several papers thus proposed to deal with the material dimension of surfaces of both inscription and (less familiar) separation; in other words, with epistemically active surfaces. Others would address the materiality of boundary phenomena in the netherland of living and unliving things: undead surfaces. Still others engaged the role of surfaces in processes of perception. Last, there were those thematizing surfaces as things political: waste, skin and the hazards of handling radioactivity. Meanwhile, another idea we had begun to toy around with – to have a somewhat less learned event accompany the workshop – was coming to fruition as well, rapidly developing its own dynamics. Indeed, far beyond our initial expectations it developed from side-show to full-blown, membrane-themed exhibition (more on which below).

The rest of the story in fact is quickly told; yet before proceeding with a more detailed discussion of the papers collected in this preprint, it is high time that we thanked Antje Radeck. Our noble schemings would have gone nowhere, and the organizational efforts would never have been proceeded as smoothly without the experienced support of Antje. A very big thanks to her. We are also grateful to our colleagues Christina Brandt, Ursula Klein, and Hans-Jörg Rheinberger for agreeing to participate and offering their views on the subject matter during panel- and wrap-up discussions – it proved highly stimulating indeed. Thanks, of course, to all the participants for coming out to Berlin-Dahlem from more or less distant parts of the world and making this workshop such an all-round satisfactory affair and finally, we would like to acknowledge Angelika Irmscher for setting up the preprint volume at hand. And let us now briefly sketch out their contributions and above themes, or rather, how we see them, and the way things came together, in retrospect. The following will thus loosely stick to, and motivate, the original session headings; but it will also take into account some of those issues, resonances and questions that materialised only during the workshop.

Epistemically active surfaces

Historians of science have long been interested in the laboratory as a space of experimental practices, littered with tools, machines and instruments or populated with standardized organisms. From another angle, the laboratory appeared as governed by intricate social and pedagogical regimes, and certainly enough, as caught up in wider historical dynamics – say, the city, industrialisation, or electrification. Especially studies on inscription devices have spawned a great many follow-ups reaching well into the history of art and visual studies. Arguably though, these studies did not always very deeply engage with the material conditions and origins of the visual and semiotic practices they investigated. The materiality of the medium frequently figures in the abstract only, a generic plane, malleable by the practitioner, onto which the various pencils of nature might leave their more or less appealing traces.

Today, digital technologies have made very salient, of course, the manipulative and processual nature of images (even if they simultaneously have fuelled discourses of disembodiment and information). That said, even media such as photographs or celluloid film have rarely been studied as material (and thus, historical) surfaces in their own right, let alone the more obscure specimen, say, smoked drum paper or isinglass (more on the latter below). Nor have historians always been as attentive to the at times quite concrete resonances that existed between seemingly unrelated domains as, notably, Hannah Landecker's already mentioned work on early 20th century cellular science, the cinema and film theory has explored. Indeed, one is tempted to spin this further – the physical chemistry of cells, technical emulsions, and photochemistry, for instance, would seem to provide ample points-of-contact between histories of chemistry, the life sciences, industry and photography.

And yet, with a view on the history of science especially, it seems clear that it is not least in virtue of such palpable resonances and material correspondences that surfaces – of inscription and otherwise – are anchored in the world and rendered epistemologically active. As Chitra Ramalingam showed in her contribution (published elsewhere, see Ramalingam 2010), representations of sound, conceptions of human perception, and technologies of photography all had to come together as Victorian physicists begun to chase, and fix in time, the all too elusive sparks generated by electricity. In other words, three sensitive, material surfaces had to come together: metal plates covered in fine dust, human retinae, and surfaces covered with light-sensitive, chemical emulsions. Henning Schmidgen, though originally made to appear in the session on Un-dead Surfaces, in his paper also brings home to us the importance of such intricate – and material – entanglements in a closely related (and equally elusive) affair: the history of the graphic method and nervous excitation. Schmidgen relocates the well-known episode of Hermann von Helmholtz's mid-19th century electrophysiological measurements in Königsberg within a suggestive history of isinglass, a substance obtained from the air bladders of fish. Schmidgen's paper recovers the history of this gluelike, organic commodity exploring its common uses and deployments, which stretched well beyond the laboratory. Thus, his account transcends the usual, semiotic and aesthetic concerns of the historian of the graphic method, or indeed, those of scholars of inscription devices more generally. Kijan Espahangizi in his contribution similarly explores a rarely appreciated, yet all the more widely circulating surface material – glass (or more properly, glass containers). Glass being the ubiquitous material that it was/is, Espahangizi is naturally concerned with its manifold, scientific, industrial and technological ramifications beyond the laboratory (and hence, within). By means of a very kindred strategy to Schmidgen's, Espahangizi sketches a view on the laboratory which is decidedly non-local and, on this basis, develops a notion of "boundary-thinking" that, significantly, centres on practices of separation – thus also drawing our attention away from the usual focus on inscriptions. The latter, surfaces of inscription, for their part received a most suggestive treatment by media historian Lars Nowak, who, in his essay on the spatiality of ballistic photography, reveals this conglomerate of technologies to be a counter-intuitively multi-dimensional process of immediate, physical penetrations and material contacts. As Nowak shows, multiple layerings of surfaces – filters, layers of emulsions and gas, armour plates, protective cassettes and more – constitute the immensely rich material culture behind this martial art of making perceivable the ultra-fast.

Perceiving boundaries

If the materiality of exterior surfaces mediates the presence of electric sparks, nerve impulses or guns and shrapnel, surfaces, as the talks assembled under this heading reminded us, enter perceptual processes along different, and no less convoluted paths as well. Reflection, adsorption, resonance, transduction and the like are, as we have already hinted at, all phenomena intimately associated not only with processes of perception but with surfaces – both within and without us. Veit Erlmann, in a panoramic paper covering some three centuries, tackles what is perhaps the most iconic of these surface structures besides the retina of the eye, the basilar membrane. By charting the various turns and tides of its conceptions – oscillating between paradigms of resonance and intensity – Erlmann's aim is to illuminate the "bioculturally liminal position" the latter always occupied as a surface of contact and friction between a great diversity of scientific disciplines, epistemologies, and socio-cultural concerns. Gábor Zemplén's similarly ambitious talk (not reprinted here) likewise charted a considerable stretch of time, Zemplén exploring, for his part, the significance of boundary phenomena in the conceptualization of colours apart from the (historiographically) dominant Newtonian tradition. Alternative, so-called modificationist views of light and colour not only survived among the more practically minded users of colours, as Zemplén suggested, they fundamentally centred on the concept of, in this case, light/shadow boundaries. The virtual, dynamic mutuality of perception and boundary phenomena also moves into view in Sigrid Leyssen's contribution on Belgian psychologist Albert Michotte, thus treating on an altogether different time-frame and period, the mid-twentieth century. Michotte's well-known, illusionist investigations into the psychology of causal perception intimately entangled the problematic with questions of surface phenomena, as Leyssen shows. Specifically, Leyssen argues that within Michotte's experimentalisation of perceived causality, boundaries assumed a central, threefold role as epistemic object, interpretive tool, as well as instrument. Indeed, Michotte's persistent attempts to turn the entire problematic – causality perceived – into a measurable parameter, has made manifest not only the contingent and subject-dependent characteristics of the causal scenes that were being observed, but, as Leyssen tells us, they effectively questioned the boundaries of perception in general. Michotte's "experimental phenomenology of perception" thereby held surprises not only for the experimental subjects, but for the researchers as well.

A less immediate kind of boundary (mis)perception is at stake in Tinas Choi's paper which explores the resonances that existed between 19th century discourses on the nature of and ultimate constitution of things and matter and those of the self in Victorian fiction. Choi offers a reading of George Eliot's novel Daniel Deronda as profoundly sub-structured by, or a kind of parable on, the probabilistic, mathematical physics nascent in the Victorian age. In a world of dancing, colliding and imperctible molecules, surrounded by the elusive, interstitial ether, physicists such as James Clerk Maxwell developed novel ways of exploring the contingent and invisible. In Eliot's novel, as Choi suggestively argues, the boundaries of the individual – or better yet, the unfathomable interstice of her believes, (romantic) relations and speculations – formed an analogous, vexing world of the probable, indefinitely oscillating between putative certitude and the seemingly unknowable. Or, as Eliot herself put it, "The beginning of an acquaintance whether with persons or things is to get a definite outline for our ignorance"

Boundaries of the living: From un-dead surfaces to the politics of surfaces

Last, let us briefly explore yet another, if crucial set of surface phenomena: biological ones. Life as a bounded space interacting with the environment through dedicated surfaces is, no doubt, a concept central to the biological imagination. Not too surprisingly, no small number of the abstracts we had received in some way or another approached our workshop's theme from this angle. Two main threads seemed to stand out, guiding us in the selections we made: Un-dead surfaces, we thought, usefully captured something of the commonalities and ambiguities inherent in the various sub-cellular, molecular, artificial and semi-organic surface-structures entering the stories told by Hannah Landecker, Thomas Brandstetter and in some ways, of course, also the one by Henning Schmidgen above. The papers by Sara Bergstresser, Lisa Cockburn and Alexander von Schwerin in turn more ostensibly dealt with surfaces as things bio-political: skin, cancer, radiation hazards and protection or waste. Yet, the micro and macro is perhaps not so easily separated, and if we discuss them together here, it is also to bring out more clearly some of the overlaps which emerged as the workshop proceeded.

Both Brandstetter and Landecker's papers made central what is perhaps the most curiously disregarded entity in the history of modern biology when compared with the immense output that has accumulated on evolutionary biology, (molecular) genetics, and things such as DNA and enzymes: the "unit of life", the cell. The latter, of course, has long been construed in terms of surfaces – just take, for example, the chemically manufactured, semi-permeable membranes devised by physiological chemist Moritz Traube in the 1860s. Today, they continue to be of central relevance; say, in the form of membrane transport mechanisms as studied and modelled by biophysicists, synthetic biologists, neuropharmacologists and computer scientists. Brandstetter's paper takes us back to the far end of this historical spectrum, the mid-nineteenth century. It was then, as he argues, that the cell not only made its appearance as a surface-thing amidst heated quarrels concerning the nature of life; as such, the cell, far from being straightforwardly natural, inhabited a space where the boundaries between the living and non-living had become deeply permeable. This "zone of ambivalence" was made up of artificially prepared models and pseudocells (if we dare, anachronistically, say so), and is here being laid out by Brandstetter in an argument again converging on questions of materiality, epistemology, and their intersections.

And there is no reason not to extend such perspectives further into the twentieth century. Brandstetter's paper contains some suggestive pointers as regards the relevance of, and the boundaries that we might wish to (re)draw between, fields such as physiology, physical chemistry and thermodynamics – and their respective disciplinary histories. A century later, the age of molecular biology in full swing, such macroscopic crudities might seem little more than a historic curiosity; yet, as Hannah Landecker demonstrated in her paper (not reprinted here either, unfortunately) on signal transduction research in David Rodbell's lab in the 1960s, localized, large-scale surface-structures – notably, cellular membranes and their receptors – have profoundly shaped the conception of cellular information processing – even on the molecular level. With few exceptions, such supra-molecular or better yet, definite, spatialized organic structures as Rodbell's "G proteins" and the membrane, have not figured in the – typically – DNA-centred narratives historians of the life sciences tell. The successive shifts in conception traced in her paper suggestively illustrated what Landecker called a "democratization" of the molecular gaze on the living. Moreover, these shifts – from classic, hormonal messengers via cybernetic and informational construals of molecules-as-signals to the complex transduction cascades of the more recent, as Landecker labelled them, "relational biologies" – brought into view the numerous ways even the smallest boundaries of the living – the sites of molecular action – were imagined and practically enrolled (say, as "ghosts") in the laboratories. From here, in turn, it seems easy enough to imagine an altogether different, and complex world of 20th century molecular biologies.

Indeed, though approaching things from entirely different directions, the significance of spatialized organic structures in matters of molecular biology is reinforced by the papers of Lisa Cockburn and Alexander von Schwerin. Not coincidentally, their papers appeared in our programme under the heading *The Politics of Surfaces,* along with Sara Bergstresser's account of radiation hazards, accidents and physical damage in the context of post-WW2 Los Alamos (also missing here), which focuses on scientists' bodies. Both Cockburn and von Schwerin expose some of the same, complex entanglements in the constructions of the molecular entertained with negotiations of macroscopic, bio-political boundaries. Cockburn's topic is waste, or more properly, the fluidity and elusiveness of the concept in its interactions with applied microbiology and very recent ecological and biotechnological efforts. By taking waste as a "boundary object" as her point of departure (as argued by Susan Leigh Star and James Griesemer), Cockburn's paper examines the "exploratory space", as she calls it, wherein refuse, the social, microbes, and bioengineering become continually dis- and re-entangled. She thereby traces a historical trajectory somewhat analogous to Landecker above: from classic, reductionist bioengineering to recent, complexity-loving "community approaches", based on metagenomic analyses of microbial diversity and physiology. Schwerin charts much the same complexification of the biological micro-realm in his paper; yet, he does so by way of a rather different divide – the human skin – and a different molecular problematic: mutation, DNA repair and mutagenesis. In tracing approaches to mutagenesis from the days of nuclear fear to our own age of anti-ageing facial creams, Schwerin also tells a story of skin, the protective boundary between human organism and hostile environment, becoming molecular-environmental interface: a multi-layered account of conceptual, scientific and ideological reconfigurations a great deal richer than notions of "molecularization" would seem to suggest.

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These, then, are the surfaces, membranes and boundaries that you will encounter on the following pages. Certainly, it is a selection that not even remotely exhausts the topic. Indeed, though the workshop, we think, was an altogether successful affair, even some of the items on our not too immodest wish-list in the end did not make it onto the programme (for lack of pertinent submissions). Thus, although our intention to make the materialities of surfaces speak, and yield novel, non-disciplinary perspectives on questions old and new, was panning out, we would have liked to have seen, a greater variety of surfaces entering the picture; for instance, more of the various kinds of membranes and surfaces which, we can only assume, shaped, and were being shaped by, technological and industrial contexts primarily, including practices of separation, of coating, or containment – mundane themes which, to be sure, were touched upon, but never quite made it to the centre. But is was not least here, as Ursula Klein pointed out during the panel discussion, that novel ways of thinking about (and going about) surfaces abounded throughout the 19th and 20th centuries – among them: osmosis and diffusion, electro-chemistry and technologies such as electrical batteries, the immensely industrious colloid sciences, and more. Indeed, as Hans-Jörg Rheinberger reminded us from a somewhat more "abstract" angle, it is little exaggeration to suggest that the drawing of boundaries – whether practical or conceptual, economically significant or not – was characteristic of almost any scientific activity; and, as he further suggested, it rarely were activities (and a corresponding semantics) failing to carry with them inextricable, political connotations.

Much, no doubt, remains to be done; and perhaps it is in this sense that we best understood Ursula Klein's reservations which she voiced, characteristically poignant, during the panel discussion; namely, that there was a danger of elevating surfaces perhaps a bit too much, of losing sight of history's concrete singularities owing to an enchantment with things "abstract" and "theoretical". Her point is well taken, and much of our pre-work planning and scheming was indeed about pre-empting this to happen; after the fact, we are more confident than ever that juxtaposing surfaces, as well as the questions addressed to them, in fact does more good than bad. It was in this spirit too, that, during workshop preparation, we became more excited about the idea that a slightly less scholarly event might very well be suited to complement the workshop, the topic being, after all, surfaces. And so as to finally come to an end, let us now briefly touch on the already mentioned exhibition, *Membranes, Surfaces and Boundaries: Creating Interstices,* the opening of which collapsed with the workshop – but which by then had come a long way from our initial little brainwave.

That this was the case is due to artists Susanna Hertrich and Heike Klussman as well as architect Thorsten Klooster and designer Clemens Winkler who enthusiastically responded to a "call for arts" which we had sent out, in an analogous fashion to our CFP. A very big thanks to them. The four of them developed a concept for a surface-themed exhibition to accompany the workshop, and together we pondered ways of making it happen. After we had secured the generous support of the arts and science programme of the Schering Foundation and AEDES Architekturgalerie (our thanks to them as well), we turned into mere bystanders, quite impressed by what we only got to see, in all its concreteness, at the opening. As the pictures we have included in this preprint document, the exposition comprised a great many works and approaches to the topic: From BlingCrete, a novel retro-reflective building material to whimsical filtration devices and interactive pencils which mapped drawings-on-surfaces into the realms of sound. Susanna Hertrich's "Robot" perhaps seized the character of membranes, surfaces and boundaries in the most palpable way: A spotless black piece of sculpture the outline of which bears unmistakable resemblances to organismic structures as well as human artifacts, the "Robot" covers any explicit similarity beneath its impenetrable, velvety appearance. Pure surface, it is, and yet inseparable what it is beyond. Or, as physicist Wolfgang Pauli put it: "God made the bulk, the surface was invented by the devil".

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Epistemically Active Surfaces

Königsberg 1851 – Helmholtz and the Surface of Time

Henning Schmidgen

In the mid-19th century an organic substance derived from a kind of skin came to be used as a recording surface. The substance was called »isinglass«, in German »Fischleim« or »Hausenblase«, and in Latin »Colla piscium« or »Ichtyocolla«. According to the 1910 edition of the *Encyclopaedia Britannica*, isinglass is »a pure form of commercial gelatin obtained from the swimming bladder or sound of several species of fish«, in particular sturgeons. This explains where the German expression »Hausenblase« comes from, namely »sturgeon's bladder«. The corresponding Dutch term is »huisenblas«. As the *Encyclopaedia* suggests, the latter served as a model for the English expression »isinglass«.[1]

Throughout the 19th century, isinglass was used for many purposes, including clarification of wine and beer, repair of broken porcelain and glass, as well as the production of artificial pearls. In what follows the focus will be on the use that physiologists made of this substance in their experimental practice. In the 1890s, laboratory physiologists used isinglass as a powerful kind of glue, for example for fixing paper to metal. The present paper sheds light on an earlier and slightly different use of the substance in question. It deals with the ways in which Hermann von Helmholtz used isinglass as a recording surface in his path-breaking experiments on the »propagation speed of stimulations in the nerve«.[2]

Figure 1 shows one example. Turned into a transparent film, isinglass appears to function here as a display for an isolated body movement, i.e. the contraction of a Königsberg frog muscle in 1851. The transparency of the substance, the black-and-whiteness of the image and the snapshot character of the recording suggest similarities to photography. As I shall argue this is not completely accidental.

In order to introduce my specific take on this surface or membrane I would like to suggest an alternative title for this paper. Alluding to a rather famous history of science book, my heading could also read »Leviathan and the Myograph« or »Leviathan and the graphic method«. You only have to picture Leviathan in its biblical form, as a sea monster; and transplant it into the 19th century, or more precisely into the year 1851, when Herman, the other Herman, published *Moby Dick*.

Bernhard Siegert has generously drawn my attention to the fact that chapter 68 of Herman Melville's novel discusses the »skin of the whale«, what it is and where it begins. Initially, Ismael's opinion on this matter seems to be straightforward, since he equals the skin with the blubber, which in turn he describes as »something of the consistence of firm, close-grained beef, but tougher, more elastic and compact«. Its thickness, so he continues, ranges from »eight or ten to twelve and fifteen inches«.[3]

But then Ismael suggests that there is also something like the skin of the skin. This is where isinglass comes in: »[F]rom the unmarred dead body of the whale, you may scrape off with your hands an infinitely thin, transparent substance, somewhat resembling the thinnest shreds of isinglass, only it is almost as flexible and soft as satin; that is, previous to being dried, when it not

1 *The Encyclopaedia Britannica. A Dictionary of Arts, Sciences, Literature and General Information*, Vol. XIV: Husband to Italic, 11th ed., Cambridge 1910, p.872.

2 See Henning Schmidgen, *Die Helmholtz-Kurven. Auf der Spur der verlorenen Zeit*, Berlin 2009.

3 Herman Melville, *Moby Dick. Or, The Whale*, London 2003, p.332.

only contracts and thickens, but becomes rather hard and brittle. I have several such dried bits, which I use for marks in my whale-books. It is transparent, as I said before; and being laid upon the printed page, I have sometimes pleased myself with fancying it exerted a magnifying influence.«[4]

Reading whale-books by means of whale skins – this appears as a rather fantastic or at least complicated assemblage. Ismael seems to have a similar impression and, in the next paragraph, brings us back to the body of the whale, more precisely: the Sperm whale and its »visible surface«. According to Ismael, this surface sometimes resembles a piece of art: »Almost invariably it is all over obliquely crossed and re-crossed with numberless straight marks in thick array, something like those in the finest Italian line engravings. But these marks do not seem to be impressed upon the isinglass substance above mentioned, but seem to be seen through it, as if they were engraved upon the body itself. Nor is this all. In some instances, to the quick, observant eye, those linear marks, as in a veritable engraving, but afford the ground for far other delineations. These are hieroglyphical; that is, if you call those mysterious cyphers on the walls of pyramids hieroglyphics, then that is the proper word to use in the present connexion.«[5]

To Ismael, »the mystic-marked whale« eventually remains »undecipherable«. The approach of experimentalists such as Helmholtz was strikingly different. They did want to decipher and read the book of nature by means of their instruments and measurements. Perhaps one should even say that they »ciphered« before entering into the business of deciphering, i.e. they aimed at expressing and making manifest organic phenomena and processes by means of innovative semiotic technologies. In Helmholtz's case, these technologies were initially derived from the world of telegraphy. And the semiotic output they produced was rather monotonous. It consisted of numbers, numbers, and numbers. As we will see now, this entailed some severe communication problems.

1. »They take Helmholtz to be a madman«

In February 1850, Helmholtz sent a short note to the Academy of Science in Paris. Based on a similar »Mitteilung« in German language, he briefly presented his findings concerning the propagation speed of stimulations in the motor nerve of frogs. The crucial paragraphs of this communication read as follows: »I have found that the nervous stimulation, in order to travel from the sciatic plexus of a frog to its calf muscle, requires an interval (*espace de temps*) that is not too difficult to determine. [...] While the distance between the two stimulated points on the nerve were between 50 to 60 millimeters, the nervous stimulation needed, in order to travel this space, 0.0014 to 0.0020 seconds [which corresponded, as Helmholtz added in a footnote, to a speed between 25 and 43 meters per second].«[6]

With these sober lines, Helmholtz announced a rather significant scientific event. In 1837, his academic teacher, Johannes Müller, had stated in his famous textbook that physiologists »will probably never gain the means [i.e. the appropriate instruments] for determining the speed of the nervous principle«.[7] Some fifteen years later, Helmholtz had accomplished precisely this kind of measurement. Hence the importance the Königsberg-based physiologist attached to his time

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4 Ibid.

5 Ibid., p.333.

6 Hermann Helmholtz, »Note sur la vitesse de propagation de l'agent nerveux dans les nerfs rachidiens«, *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 30 (1850): 204-206, on p.204 and p.206. Unless otherwise stated all translations are mine (H.S.).

7 Johannes Müller, *Handbuch der Physiologie des Menschen für Vorlesungen*, 3rd ed., vol. 1, Coblenz 1837, p.686.

experiments, and hence the attention that historians of science have devoted to these trials, from Kathryn Olesko and Frederic Holmes to Soraya de Chadarevian and Norton Wise.[8]

Building upon previous work by the Italian pioneer of electrophysiology, Carlo Matteucci, and adopting a method initially suggested by the French physicist Claude Pouillet, Helmholtz had build a rather complex time measuring device. Its most significant parts were a half wooden, half metal frame that allowed for hanging and stimulating nerve-muscle preparations (fig. 2), a galvanometer used as a ballistic timing device (fig. 3), and a telescope for reading off the deflection of the galvanometer needle with great accuracy (fig. 4). The tedious handling of this set up led to laboratory ciphers that Helmholtz, together his wife Olga, wrote down in a note book (fig. 5).

Then Helmholtz used mathematical knowledge concerning the material and temporal properties of torsion balances which he derived from the work that Carl Friedrich Gauss and Wilhelm Weber had done on terrestrial magnetism and telegraphy in Göttingen in the late 1830s. This was the basis for turning galvanometer movements into time measurements. Eventually Helmholtz used the so-called variation and subtraction method. First, he measured the contraction time in stimulations that were applied to different positions on the nerve. Then, by subtracting the results from one another he would arrive at the speed of the »nervous principle«.

Once the calculations and the writing were done, Helmholtz quickly sent his preliminary report to Berlin – to Johannes Müller whom he asked to present it to the Berlin Academy of Science and publish it in the prestigious *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*. Helmholtz also sent it to Alexander von Humboldt whom he asked to pass the report on to the Paris Academy of Science so it was published in the *Comptes rendus*. Simultaneously, he sent it to his friend Emil du Bois-Reymond whom he asked to present it to the Berlin Society of Physics and to deposit it in the files of this Society in order to secure his priority in the matter.

The effect was puzzling. The report was presented to the academic public in Berlin and printed in German journals. At the same time, however, du Bois-Reymond told Helmholtz that it was hardly understandable. »Forgive me«, he wrote to his friend and colleague in Königsberg, »but you have described the matter in such utterly obscure terms that your report could only serve as a short guide for re-inventing the method […].«[9]

Alexander von Humboldt was equally puzzled. In January 1850, he wrote to du Bois-Reymond: »It will be my pleasure to pass on the report of this extraordinary Helmholtz to Paris. However, I find myself confronted with this completely incomprehensible form: there is question of 14/10.000 of a second, without even alluding to the way in which such a fraction of time was measured.«[10] Du Bois-Reymond did what he could. He translated and at the same time revised and edited Helmholtz's article before sending it off to Paris.

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8 Kathryn M. Olesko and Frederic L. Holmes, »Experiment, Quantification, and Discovery. Helmholtz's Early Physiological Researches, 1843-1850«, in *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, edited by David Cahan, Berkeley etc. 1993, pp.50-108, Frederic L. Holmes and Kathryn M. Olesko, »The Images of Precision. Helmholtz and the Graphical Method in Physiology«, in *The Values of Precision*, edited by Norton Wise, Princeton 1994, pp.198-221, Soraya de Chadarevian, »Graphical Method and Discipline. Self-Recording Instruments in 19th-Century Physiology«, *Studies in History and Philosophy of Science* 24 (1993): 267-291, and M. Norton Wise, *Neo-Classical Aesthetics of Art and Science. Hermann Helmholtz and the Frog-Drawing Machine*, Uppsala 2008.

9 Du Bois-Reymond to Helmholtz, March 19, 1850, in *Dokumente einer Freundschaft. Der Briefwechsel zwischen Hermann von Helmholtz und Emil du Bois-Reymond 1846-1894*, edited by Christa Kirsten et al., Berlin 1986, p.92.

10 Humboldt to du Bois-Reymond, Januar 18, 1850, in *Briefwechsel zwischen Alexander von Humboldt und Emil du Bois-Reymond*, edited by Ingo Schwarz and Klaus Wenig, Berlin 1997, p.101.

Once the paper was presented to the Académie des Sciences, reactions turned out to be similarly reluctant. On February 27, i.e. only two days after the presentation of Helmholtz's Note to the Academy, the liberal Paris newspaper *Le National* discussed Helmholtz's work. The context was an article about the Academy questioning the productiveness of the entire institution. Helmholtz's seemingly obscure time measurements were a case in point. After briefly summarizing the results, the two anonymous authors added: »It doesn't need to be said explicitly that frogs were wasted for this kind of work.« Then they continued: »The frogs aside, the work by M. Helmottz [sic] leaves something to desire«.[11] Du Bois-Reymond was alarmed. As he wrote Helmholtz, »your work is ridiculed in France«. And he added: »J'y mettrai ordre.«[12]

In March 1850, du Bois-Reymond travelled to Paris for presenting his own work on electrophysiology. In two talks, one given at the Academy, the other one at the Société philomatique, he also mentioned the time measuring experiences of his friend and colleague in Königsberg. The reactions continued to be harsh. Du Bois-Reymond was confronted with what he saw as the »most stupid objections and doubts«. Obviously frustrated, he wrote to Carl Ludwig shortly later: »They take Helmholtz to be a madman«.[13]

2. An interstice tracing the interstice

Helmholtz began to understand that something went wrong. In the summer of 1850, he had published an extensive report about his time measuring experiments in German language. In a 90 page article in Müller's *Archiv*, he explained his method and findings in quite detailed ways. In fact, his report was so detailed and long that this article was never translated into any other language.[14] In September he started looking for other means to illustrate and make more accessible his research. One was the use of curves for registering muscle movements in clearly visible form. Following Matteucci, Helmholtz had used this method already in the late 1840s, but discarded it because of its lack of precision. Now he returned to it, since the method promised to »put before the eyes of everybody« within five minutes the phenomenon in question.[15]

At the same time, he made a transition from animal to human experiments. In a popular lecture given in December 1850, Helmholtz mentions his physiological time measurements in human test subjects, i.e. his wife and himself. Trying to illustrate the time relations within the human body, Helmholtz, at the end of the lecture, draws a comparison that is striking. In order to explain what he calls the »inaccuracy of our impressions of time« or, in other words, the fact »that fine differences cannot be appreciated simply because the nerves cannot operate more quickly«, Helmholtz compares the human body to that of a whale: »Happily the distances are short which have to be traversed before they [the nervous stimulations] reach the [human] brain, otherwise our self-consciousness would lag far behind the present. […] With an ordinary whale the case is perhaps more dubious; for in all probability the animal does not feel a wound near its tail until a second after it has been inflicted, and requires another second to send the command to the tail to

[11] D. and T., »Feuilleton du National. 27 février. Académie des Sciences«, *Le National* (Wednesday, February 27, 1850): 1.

[12] Du Bois-Reymond to Helmholtz, März 19, 1850, in *Dokumente einer Freundschaft*, p.93.

[13] Du Bois Reymond to Carl Ludwig, April 9, 1850, in *Zwei große Naturforscher des 19. Jahrhunderts. Ein Briefwechsel zwischen Emil du Bois-Reymond und Karl Ludwig*, edited by Estelle du Bois-Reymond, Leipzig 1927, p.88-89. On du Bois-Reymond's trip to Paris, see Gabriel Finkelstein, »M. du Bois-Reymond goes to Paris«, *The British Journal for the History of Science* 36 (2003): 261-300.

[14] Hermann Helmholtz, »Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven«, *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin* (1850): 276-364.

[15] Helmholtz to du Bois-Reymond, September 17, 1850, in *Dokumente einer Freundschaft*, p.106.

defend itself.«[16] The next step is the skin, or rather the skin of the skin, the transparent substance making visible the ›finest line engravings‹. The next step was isinglass.

In September 1850, Helmholtz turned to the Königsberg instrument maker Rekoss, with whom he had already collaborated in the production of the ophthalmoscope, and ordered new equipment for his time experiments: in particular a glass cylinder derived from a champagne glass, a mobile lever, a stylus made of steel, and a new wooden frame. Once these pieces were at hand, he started to retool his initial set-up and repeated his frog experiments (fig. 6).

This time, however, the semiotic output did not consist any more of numbers but of curves. Helmholtz tried to be as careful and explicit as possible. In a first set of trials he simply demonstrated that the new experimental device was able to record muscle movements in reliable form. He had covered the glass cylinder with smoke and conducted three trials that recorded the curves in successive rows. Then he took adhesive isinglass in order to remove the curves together with the surrounding black from the cylinder. As he explained in a text that was meant to become his second report to the Académie in Paris: »I managed to preserve the smoke together with the curves that the stylus has traced by rolling the circumference of the cylinder alongside of its surface onto well united and transparent layers of isinglass, as copper engravers use it for copying. Then, I glued paper to the surface of the layer that had removed the smoke from the cylinder. In this way, I have arrived at the examples that I have the honor to put under the eyes of the Academy.«[17]

The use of isinglass for obtaining copies of images was common in the mid-19th century. For this purpose, sheets of isinglass were placed onto a drawing. With a needle, the engraver would copy the drawing onto the isinglass. Then, graphite powder was rubbed into the needle traces. In the next step, the entire film was lifted, reversed and pressed onto a stone or a copper plate, so that graphite traces were printed onto this surface and the etching could be started. It is not clear how familiar Helmholtz was with this literally graphic technique. However, he come have across it when teaching anatomical drawing at the Kunstakademie in Berlin.

In the accompanying text, Helmholtz explained the form of the curve that he had obtained (see above, fig. 1). In particular, he drew attention to the initial phase of muscle contraction, i.e. the moment right after the electric stimulation. In the curve drawing, the stimulation was marked by a vertical line. Left of it the curve set in, however, only after a small interval of time. The muscle had been stimulated but for a small fraction of time – according to Helmholtz 1/100 of a second – it stood still. The muscle did not move at all. In his German writings, Helmholtz called this phase »Zwischenzeit«, arguing that the stimulation of the muscle did not start immediately (as earlier physiologists had suggested), but lagged behind. As a result, the membrane or surface that Helmholtz used for fixing his traces showed nothing less than a gap, an interval or interstice. One interstice seems to have traced here another.

In his report for the Académie in Paris he called the gap between stimulation and contraction the period of »temps perdu«, i.e. lost time. In the 1860s and 1870s, French physiologist Etienne Jules Marey took up this phrase and frequently used it in his popular writings. Since Marcel Proust was quite familiar with these contexts (his father, the physican Adrian Achille Proust, was a part-time collaborator of Marey), one could argue that Helmholtz contributed to naming one of the most famous novels of the 20th century.[18]

[16] Hermann Helmholtz, »On the Methods of Measuring very Small Portions of Time, and their application to Physiological Purposes«, *Philosophical Magazine and Journal of Science* 6/40 (1853): 313-325, on p.325.

[17] Hermann Helmholtz, »Deuxième Note sur la vitesse de propagation de l‘ agent nerveux: Par M. H. Helmholtz à Königsberg«, Letter to Arago, Archive of the Berlin Brandenburg Academy of Science, Helmholtz Papers, NL Helmholtz 526, on p.6.

[18] On this point, see Schmidgen, *Die Helmholtz-Kurven*, p.27.

3. Lost images

At this point, however, the propagation speed of nervous stimulations is still lacking. All we have is a curve that shows a muscle contraction. So we return, with Helmholtz, to the subtraction and variation method. Indeed, Helmholtz went on to record the respective muscle contractions in stimulations that were applied to different positions on the nerve. In the absence of numbers there was obviously no real subtraction possible. But the respective recordings clearly showed that here was a displacement and deferral of the curves. Instead of being recorded onto another there was an empty space between them, and this space corresponded precisely to the difference in time that was implied by stimulating the nerve in different positions (fig. 7).

The irony is that these curves did not measure anything. Obviously it would have been possible to calculate the time of the recorded events by taking into account the circumference of the cylinder and its rotation speed. But this was precisely not what Helmholtz did. Just in passing he mentioned that the numerical results obtained by means of this new method corresponded »perfectly« with the older results, however without even telling the reader what these earlier results had been. The focus was on something different, i.e. depicting or visualizing a specific experimental technique.

There is more irony. Helmholtz finished his second communication for the Paris Academy of Science in August 1851. When sending it off, he made sure to enclose some of the curve examples he had obtained. He even added a separate manuscript that explained what was to be seen on the curve images. The Academy did not care. That is, they printed the text of the communication without the images.[19] The reason seems to be rather simple. In the 1850s, the *Comptes rendus* did not reproduce any figures. They printed geometrical schemes and included a lot of mathematical symbols. But the journal was not ready for getting Helmholtz's curves to print, not even in the schematized form that he had suggested in his explanatory addendum.[20]

Instead, the Academy cut the concluding paragraph of Helmholtz's paper where it was question of the curve examples. Then, all documents were filed. One week later, a commission was established that was meant to study Helmholtz's experiments more closely. Members were François Magendie, Pierre Flourens and Claude Pouillet, the physicist who had suggested the galvanometer timing method that Helmholtz initially used. However, a report by this commission was never published. Only Flourens, it seems, did take a closer look. At least he wrote the word »*vu*« (seen) on the first page of Helmholtz's manuscript.

This does not mean, however, that Flourens actually read it or really looked at the curves Helmholtz had been so eager to submit. In a sense then, the second communication that Helmholtz sent to the academy was equally a failure. The first was hard to understand because it presented methods and results in extremely condensed ways. The second was much easier to understand, but failed to be printed in its entirety. One could say that both communications remained hieroglyphs – similar to the engravings on a whale's body.

[19] Hermann Helmholtz, »Deuxième Note sur la vitesse de propagation de l'agent nerveux«, *Comptes rendus hebdomadaires des séances de l'Académie des Sciences* 33 (1851): 262-265.

[20] See Maurice Crosland, *Science under Control. The French Academy of Sciences 1795-1914*, Cambridge etc. 1992, p.258.

Conclusion

Since the beginning of the twentieth century, the »in-between« separating and binding ›You‹ and ›I‹, perception and movement as well as cause and effect has been a focus of theoretical attention in authors as diverse as Martin Buber, Eugène Dupréel, Erwin Straus, and Hannah Arendt. »Life lurks in the interstices of each living cell, and in the interstices of the brain,« notes Alfred North Whitehead in *Process and Reality*, while Maurice Merleau-Ponty observes in his phenomenology of speech that »the sense appears only at the intersection of and as it were in the interval between words,« and Gilbert Simondon explains in his philosophical reflections on topology and biological ontology »the living lives at the border, on the border of itself. It is there that life exists in an essential manner.«[21]

In similar ways, traditional science studies, historical as well as sociological, have used the concept of the interstice and/or the interval, even if they did not apply the exact term. The most famous example for this is the idea that, under the conditions of a scientific crisis, »Gestalt-switches«, i.e. radical shifts in the interpretation of an ambiguous visual perception, lead from one paradigm of scientific practice to another. But the same holds true for the notion that epistemological »ruptures« (*coupures*) separate sensorial from scientific knowledge, as well as today's science from its non-scientific past. In both cases, in Kuhn just as in Bachelard, gaps and pauses appear as essential elements marking the collective process of acquiring scientific knowledge.[22]

Recent studies in the history of science have rephrased this leitmotiv in forceful ways. Focusing on single laboratories and experiments, historians of science have filled the empty time between two paradigms or epistemes. They have replaced the relatively closed world of Gestalt-switches and epistemological ruptures with an open universe of micro-fissures or »intersections« as Hans-Jörg Rheinberger has put it in his last book, residing in the space between diverse laboratories and their local milieus, between experimental things and experimental texts, between measuring instruments and model organisms, even between single scientific statements and images.[23]

The Helmholtz curves belong to this porous universe. *First* of all because they stand in between the experimental set-up in its materiality and the published record of the results that Helmholtz obtained by using this set up. Since they were literally glued to this set-up, the curves are indexical signs in the strictest sense. They constitute the surface of experiment.

Second, the Helmholtz curves belong to the world of the »in-between« because what they show is an interstice or interval. They capture (a) the time span between the stimulation and the contraction of a muscle and (b) the difference in time when the muscle is stimulated from different points on the nerve.

Third, the material on which the curves were fixed is in itself an interstice, a boundary, a piece of the inner skin from the swimming bladder of the sturgeon.

Fourth, the experiment that they refer and were attached to marks something like an interstice, a discontinuity or event in the history of science that opened up new possibilities for future generations of scholars.

[21] Alfred N. Whitehead, *Process and Reality. An Essay in Cosmology* [1929], New York 1960, p.161, Maurice Merleau-Ponty, »Indirect language and the voices of silence« [1960], in *The Merleau-Ponty Reader*, edited by Leonard Lawlor and Ted Toadvine, Evanston 2007, pp.241-282, on pp.243-244, and Gilbert Simondon, *L'individu et sa genèse physico-biologique* [1964], Grenoble 1995, p.224.

[22] More generally on this point, see Henning Schmidgen, »History of Science«, in: *The Routledge Companion to Literature and Science,* edited by Bruce Clarke and Manuela Rossini, London etc. 2010, pp.335-349.

[23] Hans-Jörg Rheinberger, *An Epistemology of the Concrete. Twentieth Century Histories of Life*, Durham and London 2010, pp.217-232.

The *fifth* and final point is that the Helmholtz curves belong to the open world of the interstice because they have survived in the archive, i.e. a flat space that lies in exemplary ways between us and history itself.

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Figures

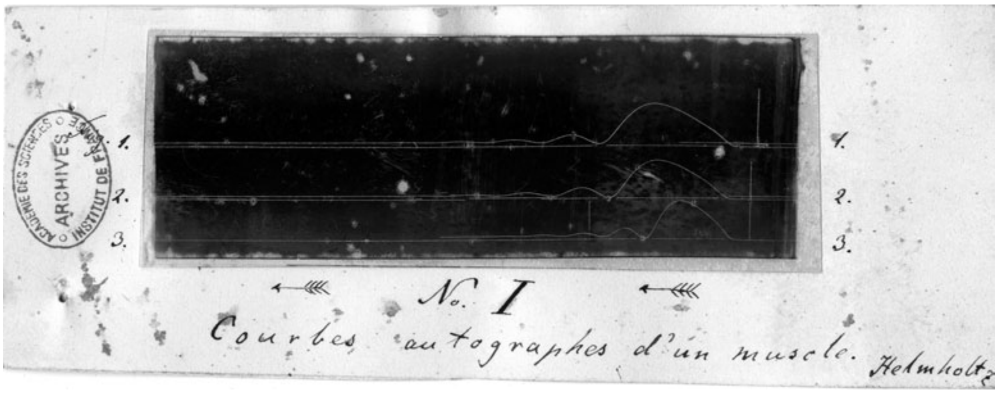

Fig. 1: Helmholtz curves, No. I, »Autographic curves of a muscle«. The trace of the muscle movement is recorded on a transparent film consisting of so-called »isinglass«. The film is glued here onto a strip of white paper. The arrows below the isinglass strip indicate that the recorded movement has to be read from right to left.
Source: © Académie des Sciences – Institut de France, Paris, Proceedings from the Session of September 1, 1851.

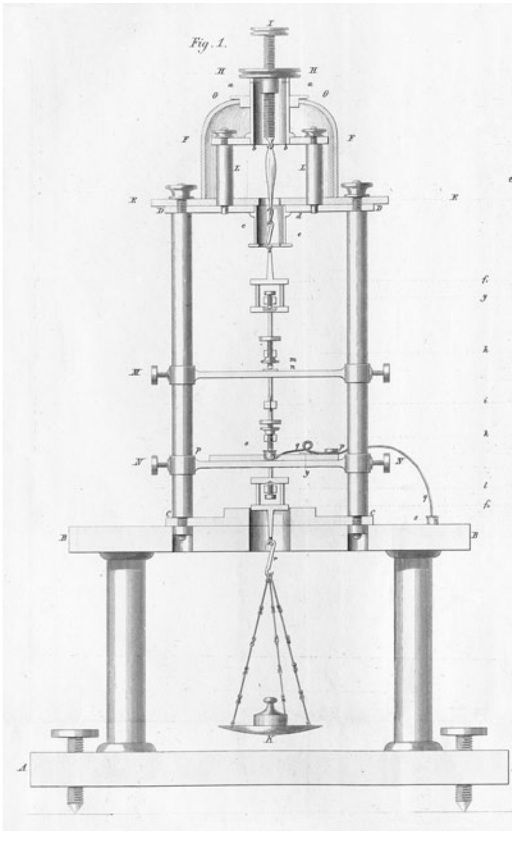

Fig. 2: Frog frame (1850). This half wooden, half metal frame allowed for hanging and stimulating nerve-muscle preparations. The preparation was fixed to the adjustable screw I and protected by means of a bell-jar. To the lower part of the muscle was fixed a device with multiple contacts and adjustable screws as well as a weight for tightening the muscle.
Source: Hermann Helmholtz, »Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven«, Archiv für Anatomie, Physiologie und wissenschaftliche Medicin (1850): 276-364, plate.

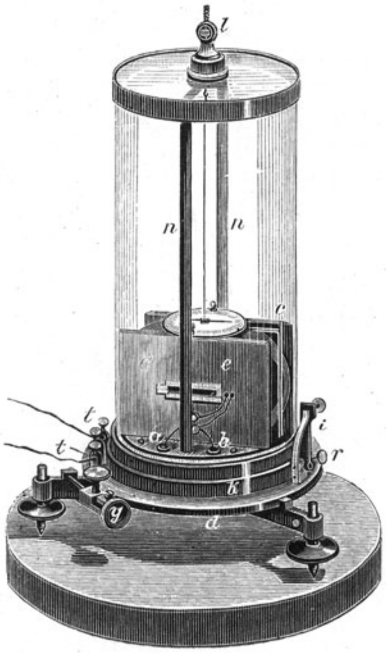

Fig. 3: Galvanometer according to du Bois-Reymond for electro-physiological precision measurements. Crucial components are a coil with windings of copper wire (a, b, c, e) and a magnetic needle suspended by means of a silk thread. Helmholtz used a similar device in his time experiments. It is unclear, however, whether he bought or constructed it.
Source: Elie de Cyon, *Atlas zur Methodik der Physiologischen Experimente und Vivisectionen*, Giessen/St. Petersburg 1876, plate XLIV.

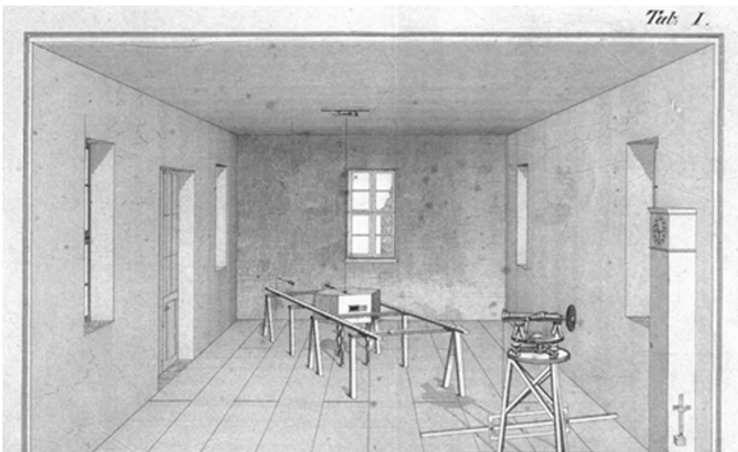

Fig. 4: Observation room according to Carl Friedrich Gauß and Wilhelm Weber for precise measurements of terrestrial magnetism in Göttingen (1837). In the center hangs the "magnetometer", a large magnetic rod, suspended from the ceiling by means of a robust silk thread. The wooden case is protecting the rod against movements resulting from currents of air. The precise position of the magnetometer is determined by looking through the telescope (right). Mounted on a tripod it is pointing into the direction of the rod to the end of which a small mirror is attached. The telescope is arranged in such a way that in the mirror one can read off the scale that is fixed to the tripod. Helmholtz adopted this model when reading off the deflections of the needle in his galvanometer.
Source: Carl Friedrich Gauß and Wilhelm Weber (eds.), *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre 1836*, Göttingen 1837, Plate I.

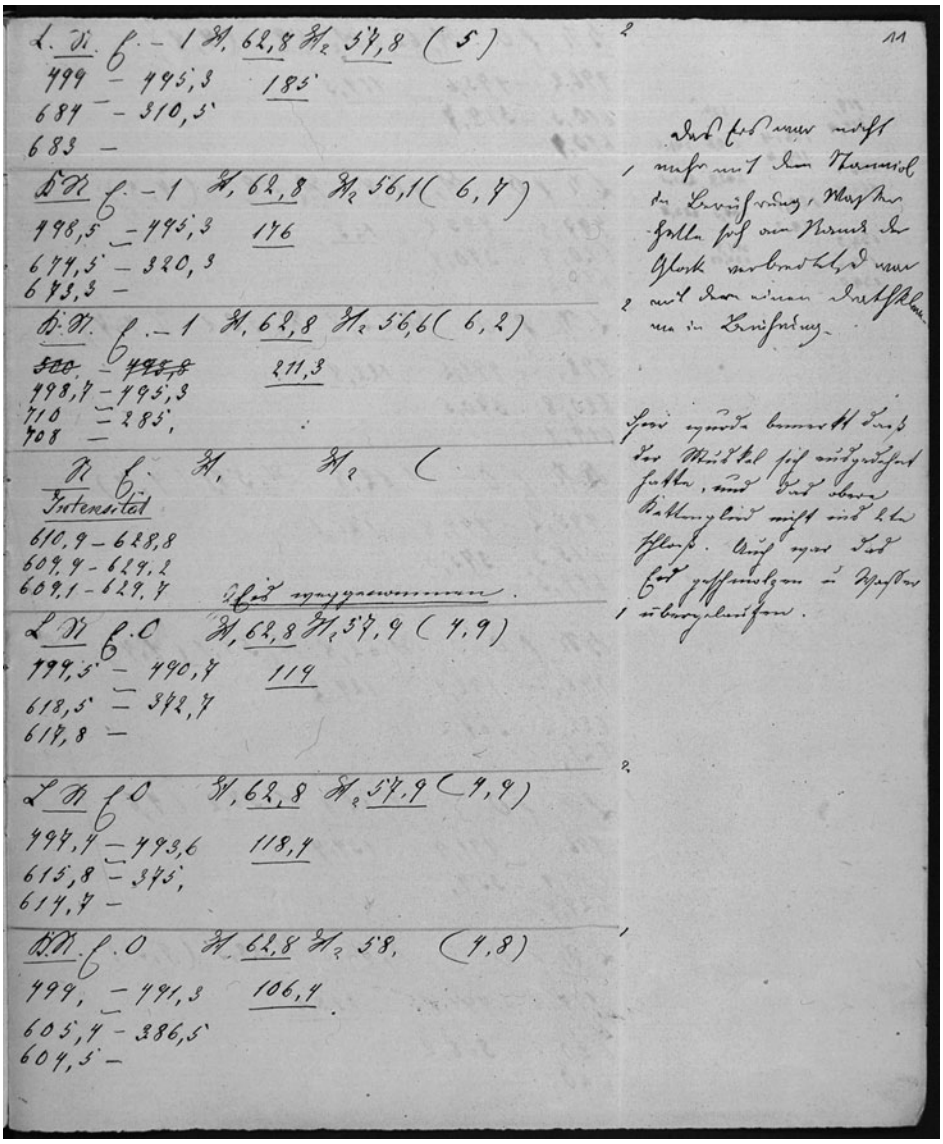

Fig. 5: Page from a laboratory notebook kept by Hermann and Olga Helmholtz from March to June 1850. The notebook concerns experiments with artificially cooled nerves. The left side of the page served for recording all required numerical results (deflection of the needle, size of muscle movement etc.). On the right one finds calculations and commentaries. With respect to the trials carried out on March 30, 1850, Hermann writes (top right): »The ice was not any more in touch with the silver-paper, water had spread at the bottom of the bell jar and was in touch with the wire clamp.« Below, Olga adds: »Later it was noticed that the muscle had extended itself and that the upper part of the chain did not fit into the second [lower] part. Equally the ice had melted and water was overflowing.«
Source: Archive of the Berlin-Brandenburg Academy of Science, Berlin. NL Helmholtz 547, p.11.

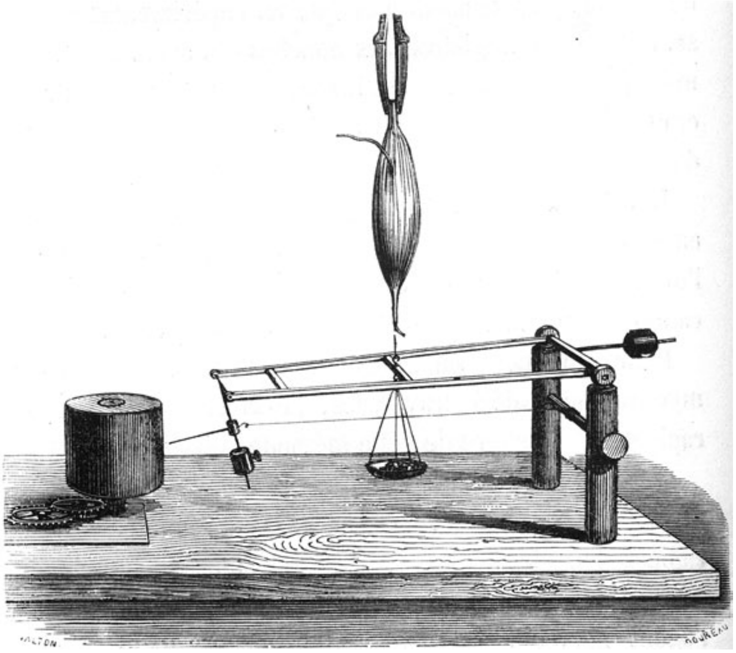

Fig. 6: Depiction of the »myograph« by Helmholtz according to Marey (1868). For the purpose of electric stimulation, a wire is inserted into a suspended frog muscle preparation. A metal frame and weight tightens the muscle. On the left side, a steel needle is attached to register muscle movements on the rotating glass cylinder. In comparison to Helmholtz's device, this myograph is a considerably simplified version of the instrument.
Source: Etienne-Jules *Marey, Du mouvement dans les fonctions de la vie. Leçons faites au Collège de France*, Paris 1868, p.224.

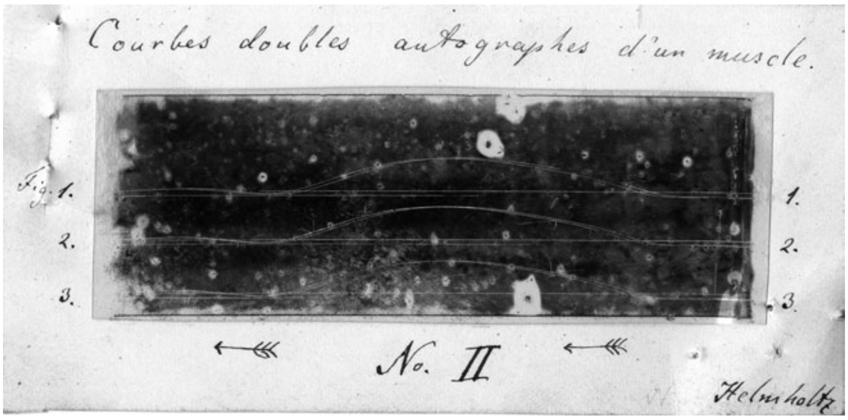

Fig. 7: Helmholtz curves, No. II, »Autographic double curves of a muscle«. On the back of this sheet of isinglass Helmholtz preserved the trace of three curves placed above one another. For producing these »double curves«, he had placed the electrode in two different positions on the nerve. The resulting displacement of the recorded curves was seen as expressing the time required for the nervous stimulation to travel the distance between the two electrode positions.
Source: © Académie des Sciences – Institut de France, Paris, Proceedings from the Session of September 1, 1851.

The Twofold History of Laboratory Glassware

Kijan Malte Espahangizi

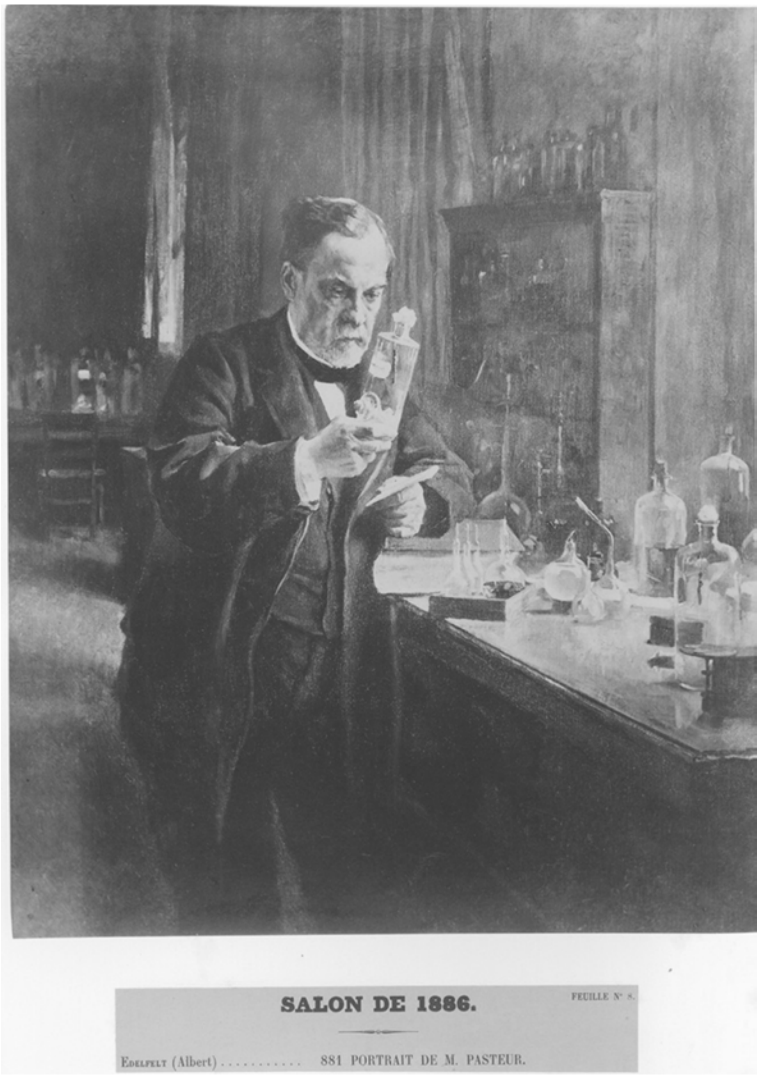

Figure 1: Portrait of M. Pasteur (Albert Edelfelt, Héliogravure, 1886)
Source: Archives nationales, Paris.

The glass topos in the history of science

We all know this very familiar scene: a scientist, brooding over a research object contained in laboratory glassware. This iconic motif can be found already in the well-known portrait of Louis Pasteur, painted by Albert Edelfelt in 1885;[1] similarly today, a Google picture search for the term *laboratory* will turn up a great many pictures of this sort. Even in the age of plastic, glass containers

1 de Wyzewa & Michel (1893). See also the complementary written description of the painting in de Loslalot (1886).

are – and this is supported by empirical evidence – omnipresent in visual representations of science.[2] When it comes to depicting science, we find glassware all over the place, short-hand: on covers of science textbooks; in the context of innovation policies,[3] preferably linking scientific discovery to its putative technological brainchild born in a glass 'retort'[4]; even in our own fields, STS and the history of science, many a book comes decorated with glassware.

The iconicity of laboratory glassware not only materializes in cover artwork, but also in book titles. Just think of Hans-Jörg Rheinberger's *Proteins in the Test Tube* – a title, which resonates, intentionally or not, with the rich cultural connotations of *in vitro* research.[5] But such iconic prominence also has a dark side; fact is, glass containers play at best an ambiguous role in the historiography of experimental research: Thus, although recent (historical) epistemology has convincingly emphasized the fundamental role of materiality in the dynamics of experimental research, the test tube or more general the scientific glass vessel remained curiously invisible: a mere *topos*.[6] In this context, the term *topos* then should be understood in its double meaning as a popular object of imagination on the one hand, and as the abstract, physical place in its original Aristotelian sense on the other hand.[7] So the *topos* of laboratory glassware could be paraphrased as the idea of an ideal place of the experiment, surgically removed from the actual, complex textures of research environments. Indeed it can safely be argued that the glittering, cultural imagery of the transparent container has obstructed from view the real material historicity and functionality of what is a – or the – constitutive inner boundary in laboratory environments. It is telling in this connection that we do have excellent works on the scientific history of optical glasses, but no systematical study of the historical epistemology respectively the historical ecology of glass vessels in laboratory research.[8] Laboratory glass, in contrast, is more typically construed as an ahistorical, 'immaterial material' the properties of which are limited to form, transparency, neutrality, and sometimes, annoying fragility. These are without doubt, fascinating qualities in their own right (and, as such, the raw material of many a *fragile* scientific anecdote).[9] But they are also characteristics that, nonetheless, did not seem to warrant further historical analysis and explanation. Yet the impression, as I shall argue here, is quite wrong.

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2 See Schummer (2008). For the historical development of the glass vessel iconography see also Schummer & Spector (2007).

3 See for example the information brochure of the German patent office. http://www.dpma.de/docs/service/veroeffentlichungen/broschueren/patente_dt.pdf (January 10, 2011).

4 There are different historical layers of the glass vessel imaginary constantly interfering with one another. Parallel to the semantics of glass as an epistemic boundary between the subject and the object of research (cf. Espahangizi 2009) there is an genealogically older layer in which the glass vessel forms a matrix, literally a womb, in which the alchemist / modern man gives birth to a second, i.e. a new artificial nature. See for example Newman (2004). This imagery lives on in the *in vitro* reproduction discourse, as well as for example in the glowing light bulb symbolizing the 'birth' of an idea.

5 Rheinberger (1997), (2001).

6 An exhaustive overview on materiality in science studies can be found in the bibliography provided by Pravica (2007).

7 Cf. Casey (1996). Significantly Aristotle himself abstracted the physical place, the *topos*, from the material object "vessel".

8 Cf. on optical glass Schaffer (1989), Hentschel (2002), Jackson (2009). There is no diachronic study on the historical epistemology of glassware in laboratory research. Nonetheless, there are works, in which the materiality of scientific glass containers plays a certain role. See for example Engel (2001), Müller (2004), Jackson (2006). For the concept *historical ecology of laboratory research* in contrast to the historical epistemology approach see the introduction in Espahangizi (2010).

9 Breaking glassware has an established narrative function in scientific self-historization. It is often used to mark crucial moments of serendipity in research processes. Espahangizi (2009), p. 61.

Glass boundaries in laboratory

One could begin with asking the following question: How was it possible that glass vessels came to perform their epistemically complex tasks, the causal containment of spatially enclosed phenomena without interfering with research, throughout the changes which the experimental sciences have undergone since the scientific revolution? Having learned the lesson of the much-evoked *material turn* in history of science, one might indeed suspect that a rich history is to be told about the formation and the contingent dynamics of these inner, material boundaries of experimental research. This focus on techniques of separation as essential forms of scientific practice is a necessary complement to the translations, inscriptions, mediations, interactions, connections, hybridizations and relational networks that have been paid heightened attention to in the last decades.[10]

On the one hand, revisiting the history of the experimental sciences from this 'marginal' perspective means reconstructing the numerous efforts in science geared towards demarcating boundaries; it would include revisiting the various uses glass was put and revisiting the different glass theories scientists developed, but also glass production, testing and standardizing technologies, as well as the substances, practices, institutions and actors entangled.[11] But this is only one side of the promised twofold boundary history. On the other hand, in so far such glassy boundary work (not to be confused with the notion of boundary work proposed by Thomas Gieryn, nor Susan Star's boundary object[12]) was never a matter of only *using* material boundaries, this history is also, *vice versa*, a history of glassware's epistemic potentials and effects – not least, its effects on the scientific efforts to understand the nature of boundaries itself, natural and otherwise. Glassware, inconspicuous but omnipresent, thus entered scientific reasoning in various ways, from providing experimental and theoretical models to being a vehicle of metaphorical imagination.

The main thesis of my paper is that these two roles of laboratory glassware – first as real boundary material in laboratory research and second as a thinking medium – are historically intertwined. Put otherwise, writing the history of scientific glass containers not only provides insights into processes of spatio-causal separation in modern experimental research, it also provides a methodological approach to the genealogy of a peculiar mode of thinking, *boundary thinking* – a provisional term that I would like to propose in order to be able to capture the historical emergence of what has become a ubiquitous, transdisciplinary scientific awareness for boundary problems, and of the manifold ways of focusing, analyzing, and solving such problems. In my paper, I will develop my argument by introducing two examples for the twofold history of laboratory glassware.

I will begin first with a spotlight on early modern experimentation and revisit Robert Boyle's famous air pump, with a special focus on his notion of glass porology. In the second part, I will turn to another important transition in the history of experimentation: the emergence of modern laboratory research in 19th and early 20th century. There, I will sketch the reconfiguration of the glass boundaries in laboratory focusing the special relation of glassware and water and ending with an outlook on the epistemic effects of this development on two very different forms of biological boundary thinking around 1900.

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10 Strongly influenced by Bruno Latour's agenda.

11 This historical reconstruction of glass boundaries in experimental research has been the core project of my dissertation. See again Espahangizi (2010).

12 Gieryn (1983), Star & Griesemer (1989).

Glass porology in early modern experimentation

In the second half of the 17th century, Robert Boyle developed an experimental device that has become emblematic of early modern science, for contemporaries as well as, of course, for science studies today.[13] Boyle developed an *engine* enclosed by glass and evacuated by a connected air pump in which he staged his famous pneumatical experiments. Steven Shapin and Simon Schaffer's path-breaking book *Leviathan and the Air Pump* has admirably shown how Robert Boyle had to defend and legitimate his new way of reasoning, a problem of producing *matters of fact* about nature so as to silence his critics. One troublesome issue, Schaffer and Shapin described, concerned the relation between the material closure of his "new experimental space" and the causal confinement of the enclosed phenomena.[14] There was an "endemic" problem with air leakage through the interstices and joints of the apparatus.[15] But what about the glass globe, the main spatial boundary of this new experimental space? No doubt, for one, that Boyle's whole enterprise depended heavily on the prospering craft of glass making in England at the time.[16] Melting and blowing these huge, crystal-clear globes – capable of resisting the air pressures Boyle's experiments demanded – was a costly *high-tech* enterprise.[17] But quite apart from these economic and technical issues, there is more to be said about the epistemic integration of this formidable boundary in Boyle's natural philosophy than meets the eye.

Robert Boyle's natural philosophy was mechanical-corpuscular; his understanding of observable phenomena was based on the assumption that there is a microscopic dimension of matter – small, material corpuscles in motion and of varying size and form – that determined the macroscopic qualities of solid bodies.[18] This was also supposed to take place inside the pneumatical globe, of course. And one implication was that the walls of the pump – the façade of the *glass house of experiment*[19] – also had to operate in two dimensions. Technically, it could be more or less stabilized – at least most of the times. And concerning the realm of the invisible, Boyle's famous *New Experiments* offer a good impression of how the glass boundary was supposed to work there. In the course of one experiment, designed to test the solidity of the glass sphere, the experimental glass vessel nested inside the outer globe of glass thus broke. Boyle tried to explain this incident as follows:

> the Imprison'd Air broke its brittle Prison, and throwing the greatest part of it against the side of the Receiver, dash'd it against that thick Glass into a multitude of pieces. Which Accident I mention, partly that it may confirm what we deliver'd in our Reflections, upon the first Experiment, where we consider'd what would probably be done by the Spring of Air Imprison'd in such Glasses, in case the balancing pressure of the ambient Air were withdrawn; and partly, that we may thence discern of how close a Texture Glass is, since so very thin a film of Glass (if I may so call it) prov'd so impervious to the Air, that it could not get away

13 Shapin & Schaffer (1985), p. 30.

14 Ibid, p. 47.

15 Cf. ibid. p. 260. See also p. 29.

16 See Godfrey (1975). In the last years, Ursula Klein has successfully put materials and their circulation "between market and laboratory" on the agenda of history of science. Cf. Klein & Lefèvre (2007) & Klein & Spary (2009).

17 Cf. Shapin & Schaffer (1985), p. 29.

18 Cf. Newman (1996).

19 A term borrowed from Shapin (1988) and slightly adapted.

> through the Pores, but was forc'd to break the glass in pieces to free it self; and this, notwithstanding the time and advantage it had to try to get out at the Pores.[20]

In Boyle's understanding, solid bodies were bulks of corpuscles stuck together forming all kinds of interstices, holes, cavities, and pores. This was the case also for the glass vessel, which would function as a porous membrane. Thus, there was a reciprocal relation of the experimental glassy boundary, the objects of research, and Boyle's natural philosophical construals thereof. The "porology"[21] of the boundary mirrored the corpuscularity of the enclosed experimental space: Pores and corpuscles thus were complementary just like lock and key.

This kind of glass porology[22] would configure the experimental boundaries of he new mechanical natural philosophy. It did so not only passively, but also actively by, in a way, "challenging" research.[23] Since the very beginning of the 18th century experimentalists generated new astonishing electric and luminescent phenomena by the attrition of their glass vessels.[24] Seen from the viewpoint of natural philosophy, these phenomena were discussed as effects of mechanically stimulated porous glass surfaces.

So on the one hand we can state that the glass container as the relevant boundary of the experimental space was, perhaps not too surprisingly, interpreted in the existing framework of corpuscular philosophy; yet on the other hand – and this is the other, less obvious side of this twofold boundary history – the transparent material possessed extraordinary qualities that crucially helped to structure that very framework: Boyle's philosophical porology. In an *Essay* he published in 1684, glass consequently would play a central role in his argument.[25] If he could show that glass was indeed made up of pores, Boyle reasoned there, he would be able to generalize his porology from organic matter like skins, leaves etc. to all inanimate solid bodies

> Since the Subject of this Essay is the *Porousness of Solid Bodies*, and since there is no Body that is generally reputed so close and compact as Glass, it will be pertinent to this discourse, and probably will be expected, that I should here say something about the Question, whether glass be, or be not, devoid of Pores.[26]

The crucial role of glass in Boyle's porology becomes even more apparent in an unpublished paragraph of the manuscript:

> Because many ~~divers~~ of the Moderns as well Chymists as Philosophers looke upon glasse the ~~closest or least porous~~ body of the world most free from Pores, the thing contended for ~~in~~ this Paper will probably be thought abundantly confirm'd, if we can shew that glass itself is porous.[27]

In order to do so, he gathered various experimental histories and reports that all showed how glass vessels functioned like sieves, confining gross corpuscles like air and chemical substances, while

20 Boyle (1660) Cf. also Boyle (1965).

21 Boyle (2000a), p. 107.

22 See the review of treatises on glass porology in Bohn (1696). For the history of glass theories cf. also Ganzenmüller (1956).

23 Cf. the concept of "challenging objects" developed by Jochen Büttner at the *Max Planck Institute for the History of Science*. (www.mpiwg-berlin.mpg.de).

24 See Freudenthal (1981) and cf. Bernoully (1700), Hauksbee (1706). For broader account see Hochadel (2003).

25 Cf. footnote 69 in Shapin & Schaffer (1985), p. 182.

26 Boyle (2000a), pp. 144f.

27 Source: Boyle Online Project (www.bbk.ac.uk/boyle, January 10, 2011), BP 9, fol. 195, MS version of 'Essay of the Porousness of Solid Bodies', *Porosity*, chapter VIII. Cf. Boyle (2000a), p. 147.

being permeable to light, heat, cold corpuscles as well as magnetic *effluvia*.[28] Therefore, the glass vessel not only provided an effective material boundary in early modern experimentation, as shown before, but also generated an epistemic surplus. Glass became a crucial material reference that allowed to think of all solid boundaries as porous membranes.

For the next example for the stimulating interplay between defining and thinking material boundaries in laboratory I would like to turn to another important transitory phase in the history of experimentation: the birth of modern laboratory in 19th and early 20th century.

Glassware in modern laboratory research

In mid 19th century Europe glass industry boomed again, a development that also heavily stimulated the scientific interest in this traditional craft product. Glass, of growing economic importance, was increasingly perceived as a scientifically relevant substance as well, something to be studied systematically.[29] Understanding glass chemistry was an urgent technological desideratum in a time when the famous Crystal Palace of the World Exhibition in London in 1851 (not to mention the more ordinary glass mass products) suffered from corrosion damage induced by water and other atmospherical substances.[30] But this was only one important factor. Parallel to that, in the emerging culture of precision measurement, glass chemistry became a relevant issue as well. It was in this new emerging world of modern laboratory research led by chemistry that the influence of glass boundaries on the enclosed epistemic objects, above all its reaction with water, would become a source of measurement errors.[31] New, measuring techniques were developed in order to quantify the hitherto irrelevant and therefore unnoticed minimal glass-induced errors.[32]

This is not the place to go into detail; the point though, would be to emphasize how the establishment of this *glass error repertoire*[33] in modern laboratory research was intimately entangled with not only the development of scientific glass theories and physico-chemical operations which connected them technologically to the art of glass manufacturing, but also with the development of standardized glass-testing procedures and modified special-purpose container glasses.[34]

Indeed, the 1860s on, with the ongoing professionalization of exact laboratory research not only in chemistry but also in other disciplines and with the diversifying and intensifying use of laboratory glassware, the glass boundary problem became endemic, turning out to be more complex even than had initially been assumed. First, it then became clear, to the manufacturers as well as to the scientific consumers, that there were very different demands and requirements

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28 In his argument he had to struggle with the fact that there were indeed very “subtle“ chemical substances that seemed to pass through the glass pores. Boyle interpreted this technical knowledge of chymists and alchemists as “extraordinary” exceptions that nonetheless proved the rule. Boyle (2000a), pp. 145f.

29 The most important representative of this new technical glass chemistry was Jean-Baptist Dumas. See for example Dumas (1830).

30 Dumas (1832), Péligot (1862), Péligot (1877), p. 54. For the cultural impact of glass in Victorian culture see Armstrong (2008).

31 In mid 18th century there was already a debate on the interaction of chemical glassware and water. But these phenomena were rather curiosities than serious threats to the experimental routines of chemistry. Cf. Eller (1746), Pott (1756), Marggraf (1756), Lavoisier (1770).

32 Beginning with Fresenius (1853), p. 76, Stas (1860), Emmerling (1869).

33 For the term “error repertoire” see Schickore (2005).

34 Cf. the far more detailed account in Espahangizi (2010). To avoid confusion, let me emphasize again that the history of glass as a scientific container material does not coincide with the well-known history of optical glasses.

regarding the use of glass containers in diverse research environments and experimental disciplines: Glasses had to be resistant to acids, to alkaline attacks, water-induced corrosion and so on. In this connection, one has to bear in mind moreover that there was no one single laboratory glass on the market, but rather a wide variety of traditional glass products with very different, traditionally secret batches and therefore mostly unknown chemical compositions. The solution to this problem seemed to be the development of a general glass-testing standard – most loudly demanded by chemists.[35] Action was clearly called for regarding the glass issue, and it was the chemical laboratory of the *Physikalische Reichsanstalt* in Berlin in close cooperation with Otto Schott's glassworks in Jena, which provided, after thorough research, a standard benchmark for laboratory glasses based on the interaction between glass and water. With this standardized measurement of water-induced glass attack by titration, in the 1890s for the first time, all glassware used in laboratories could be compared, classified and improved in terms of their chemical resistance.[36] In more abstract terms, this development led to a transdisciplinary reconfiguration of the all-important inner material boundaries in modern laboratory environments. Laboratory glassware, ceased to be an 'unscientific', traditionally crafted product but turned into a technologically regulated and standardized research infrastructure supplied by a increasingly science-based industry.

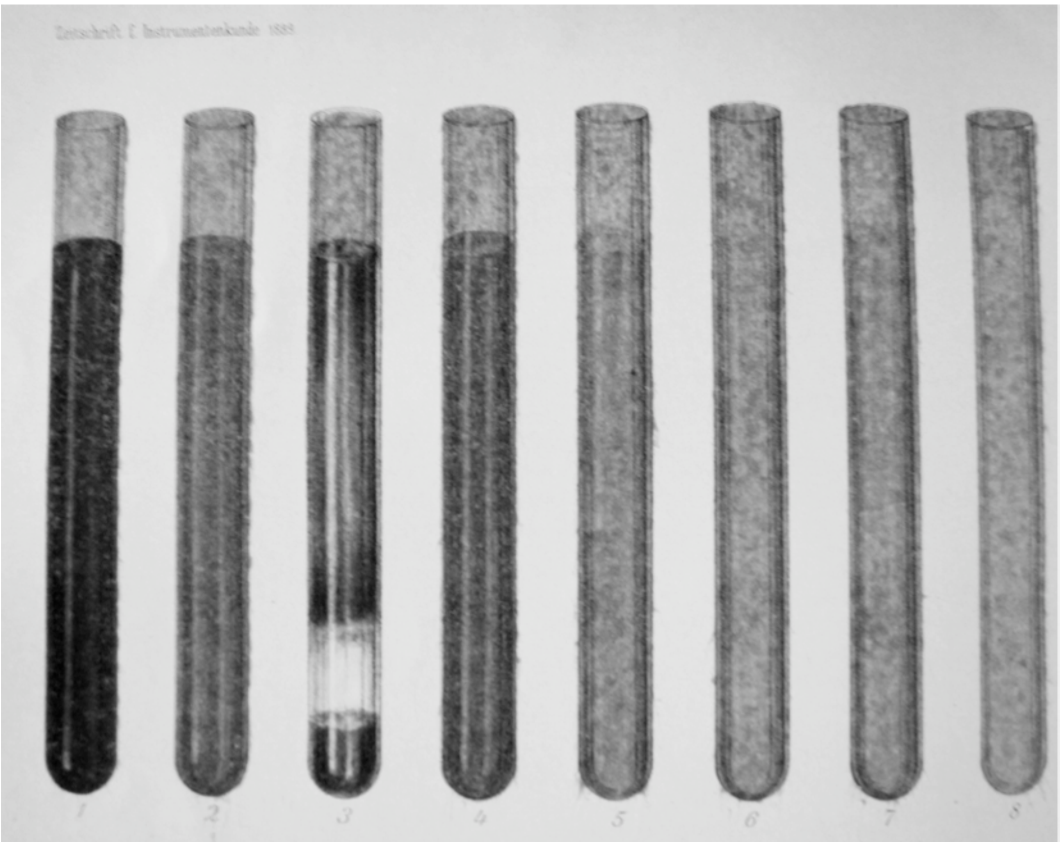

Figure 2: Glass testing with Iodine-Eosin Source: Mylius (1889), p. 52.

[35] Another valid option seemed to be the development of an universal glass, the so-called „Normalglas". But this idea, first proposed by Dumas and further elaborated by Benrath (1868) turned out to be principally impossible because of the varying uses of glass containers. Cf. Foerster (1894), p.390/394.

[36] Foerster (1893), Mylius (1903). See chapter 4 in Espahangizi (2010). Cf. also Engel (2001).

The most obvious epistemic effect of this redefinition of the inner boundaries in laboratory – again coming to the other dimension of this twofold history – was the new awareness for glassy boundary conditions which began spreading through the scientific discourse more broadly. Especially the interaction of glass and water troubled the laboratory sciences, and it 'popped up' in cell physiology, bacteriology, gasometry, cathode ray, X-ray physics, and chemistry as well as physical chemistry.[37] Around 1890 Friedrich Kohlrausch, the "*Master of Measurement*",[38] for example, noticed that the dissolution of glass introduced a significant error into the measurement of the electrical conductivity of pure water, a fundamental parameter within physical chemistry.[39] Kohlrausch, who would become the second director of the *Physikalisch-Technische Reichsanstalt* shortly after in 1895, realized that it made no sense trying to determine the electrical properties of laboratory water without taking into account its interaction with the glass container.

This strengthened epistemological as well as ontological interdependence between the container and the contained became also visible in the transition from the world of research into the sphere of production and circulation of goods. Outside the laboratory the boundary dispute between glass and water turned out to have painful, sometimes even lethal consequences. Water-based pharmaceutical, medical or nutritive products like for example cocaine, strychnine, morphine, blood, beer, and milk needed containers made of special glass to be stabilized – just like in laboratory. It was the technoscientific redefinition of their boundaries, i.e. the packing material, which made these substances *immutable* and *mobile*.[40]

Another very illustrative example for this increased awareness for the glass boundaries inside laboratory research would be the biology of microorganisms. Tellingly, one handbook on cell culturing published in 1907 would even open up with introductory remarks on the interdependence of glass and water:

> Therefore, we will start our considerations with the water, the precondition of every organic development. (…) at this point we will only mention pure water, which cannot be understood without glass, because glass vessels as indispensible containers for water and aqueous solutions that alter the contained liquids constantly by successive dissolution of their substance.
>
> One should not underestimate these unwanted admixtures. It seems reasonable to underestimate the quantity of the soluble substance yielded by the glass and to overestimate the need of the organisms for minerals. But, in reality the amount of potassium and magnesium originating from the glass is sufficient too feed thousands of generations of cells.[41]

In the microbiological laboratory the experimental boundary had to be integrated into the metabolism of biological cells in culture media. Thus, basic glass chemical knowledge now became

[37] Detailed account in Espahangizi (2010).

[38] Cahan (1996), p. 196.

[39] Kohlrausch (1885), Kohlrausch (1891).

[40] For my complementary understanding of Bruno Latour's well-known concept see Espahangizi (2011).

[41] "Wir beginnen unsere Erörterungen füglich mit dem Wasser, der Voraussetzung aller organischen Entwicklung (...) an dieser Stelle soll zunächst nur von dem reinen Wasser die Rede sein, dessen Betrachtung sich nicht von der des Glases trennen lässt; denn Glasgefäße als unentbehrliche Behälter von Wasser und wässrigen Lösungen beeinflussen letztere unausgesetzt durch die allmähliche Lösung der im Glas enthaltenen Substanzen. Man unterschätze diese unbeabsichtigten Beimengungen nicht; es liegt nur nahe, die Menge der lösbaren Substanz, die das Glas liefert zu gering und das Mineralstoffbedürfnis der Organismen zu hoch einzuschätzen; in Wirklichkeit genügen die bei Anwendung bestimmter Glassorten aus dem Glas stammenden K- und Mg-Mengen vollkommen, um

vital even for microbiological research.[42] This awareness for the physico-chemical boundary conditions of a given microbiological system was a direct effect of the redefinition of glassware started in late 19th century. But the epistemic impetus of this development not only led to an increased general awareness for glass boundaries in laboratory, but it also inspired epistemic transfers to natural boundaries. In the case of cell membrane physiology, for example, the outer glassy boundary to the laboratory culture milieus turned into an experimental as well as theoretical model for the outer boundary of the cells themselves: the cell membrane.

Glass cells

Since the 1830s it was obvious that membranes play an important role in cell theory. And already in these early days, the pioneers of this field of research Matthias Schleiden and Theodor Schwann associated the hardly visible cellular boundary with a "watch-glass".[43] This flat glass vessel used by them in their daily laboratory environment functioned as a provisional metaphorical placeholder in a new field of knowledge, losing its epistemic value soon.[44] Between the 1830s and 1900, the epistemic object 'cell membrane', its physiology, was elaborated by means of various physico-chemical concepts and models. Especially the electrochemistry of membranes, which was based on the contributions of Heinrich Hertz, Walther Nernst, Max Cremer and others, promised to be a fruitful approach.[45] But at this crossing of various scientific disciplines, physical chemistry, biochemistry and physiology, around 1900 the glass boundary reentered the scientific discourse. Fritz Haber, famous for his method of ammonia synthesis and less known for his marginal glass studies, developed an electrochemical model for the biochemistry cell membrane based the interaction of glass and water.

In 1905 Fritz Haber had been requested to develop an easy glass-testing method for the bottle glass industry – again making visible the close interaction of the world of laboratory research and the sphere of production and circulation of goods. Scanning the vast literature on glass-testing since the 1860s, he eventually decided to repurpose the above mentioned findings of Friedrich Kohlrausch: If the dissolution of glass in water increased its electrical conductivity, this effect could be used the other way round as a testing method for glass quality.[46] But Haber did not stop here. His aim was not only to measure the effects but also to understand the phase boundary between glass and water.[47] Around 1907, Fritz Haber, who was familiar with these debates at the disciplinary intersection mentioned above realized that the water/glass boundary which he explained by way of a thermodynamic two-phase process could also serve as a model for the electromotive forces at the cell membrane.[48] With this epistemic transfer, the correspondence between glass and the cell membrane shifted from being metaphorical, like in the early case of the watch-glass, to being electrochemically concrete.[49] The experiment that Haber designed in order

das Bedürfnis Tausender von Zellengenerationen nach K oder Mg zu befriedigen." Küster (1907), pp. 6f, translation by K.E.)

[42] Cf. also in bacteriology: Hesse (1904). In plant physiology cf. Benecke (1896). Tellingly, Benecke even contacted Otto Schott in order to learn about the chemical composition of his glassware.

[43] Schleiden (1838), Schwann (1910), p. 5.

[44] Nonetheless, even a couple of decades later Rudolf Virchow could still invoke this "watch-glass form of the membrane". See Virchow (1871), p. 6. My epistemological interpretation of metaphors is inspired by Pickering (1995), p. 16.

[45] Cf. Grote (2010).

[46] Haber & Schwenke (1904b).

[47] See also the appendix of the above mentioned paper: Haber & Schwenke (1904a).

[48] Haber & Klemensiewicz (1909).

[49] As there was no single coherent membrane theory at that time, the glass membrane model was added to

to study his cell model was again based on two nested glass vessels. The outer beaker was made of the new technologically modified SCHOTT laboratory glasses from Jena whereas the inner cell had to be made of the chemically less resistant old type of Thuringian glass in order to be affected by water-induced corrosion, here, of course, a desired effect. In a sense, the twofold history of glassware as a material boundary of experiment *and* as a medium of thinking boundaries converges in this experimental design: On the one hand Haber's glass cell incorporated the traditional as well as the new, modified laboratory glass types, on the other hand the whole setting was meant to be a model for the cellular membrane. Speaking of the epistemic potentials of laboratory glassware, one has to note that the importance of Haber's glass cell goes far beyond the story of bioelectric potentials: Haber also had noted that the phase boundary force at the glass increased with the logarithm of the acidity respectively alkalinity. In other words, Haber's Glass cell potentially was a kind of chemical yardstick. Indeed, it turned out to be a perfect measuring device for a fundamental quantitative index of (bio-)chemical milieus. In the same year Haber published his results, in 1909, Søren Sørensen proposed to use this index, which we know today – not only in research but also in everyday life – as the *pH-value*.[50]

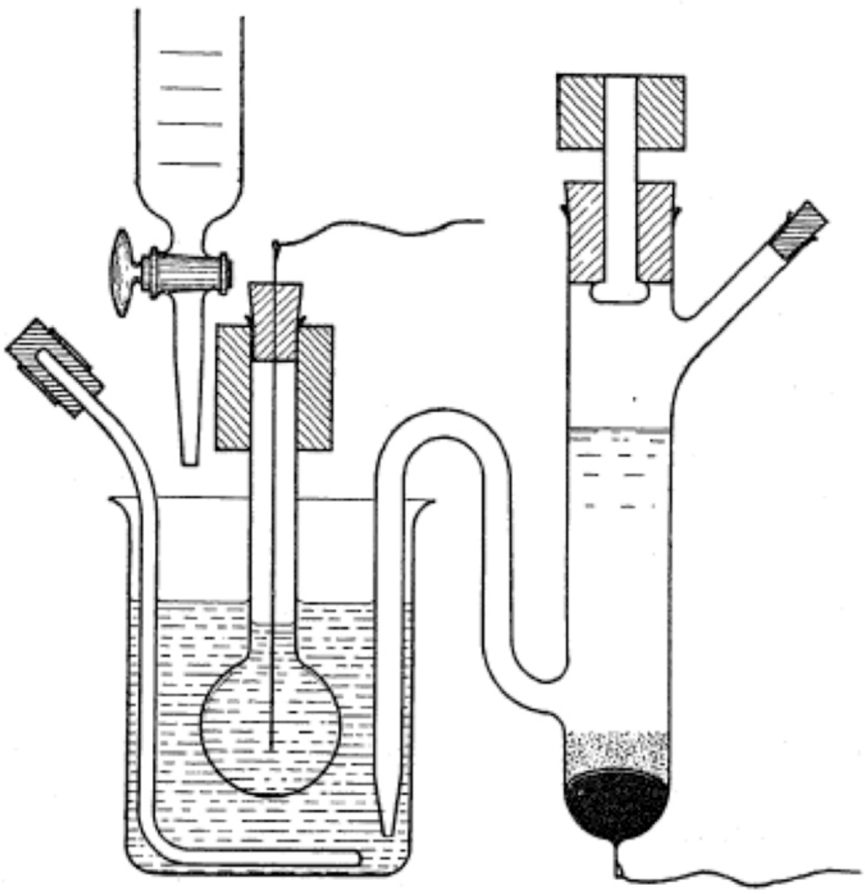

Figure 3: Haber's glass Cell Source: Haber & Klemensiewicz (1909), p. 411.

the conceptual mosaic. Michaelis (1926). For the history and materiality of cell models in the 20th century cf. the dissertation of Max Stadler "Membranous Things: An Archeology of the Nerve Cell, 1920-1960". See also Stadler (2010).

50 Sørensen (1909).

In order to give an impression of the range of possible epistemic effects of laboratory glassware on scientific boundary thinking I would like to contrast Fritz Habers electrochemical model in the last part of my paper with another more "metaphorical" glass cell developed at the same time.

Glass environments

In the first decade of 20th century the experimental physiologist and zoologist Jakob von Uexküll developed a new approach to biology based on the assumption that each living being exists in his own particular surrounding world shaped by its perceptive and action, his theory of *Umwelten*.[51] In order to understand his notion of *Umwelt*, this monadic enclosure of existence, Uexküll asked his readers to imagine it as a soap bubble, but also – and this fact has been paid less attention to in intellectual history – as a "firm though invisible house of glass".[52]

Methodologically, it is never trivial to reconstruct the concrete historical paths of cultural imagery into the amalgam of individual imagination. But in this case we do have at least three plausible sources for Uexküll's glass environment metaphors. The first possible reference would be the so-called green house effect. The idea that the earth's atmosphere functioned like a giant glass house was first developed by Jean Baptiste Fourier in the 1820s and further popularized by Svante Arrhenius around 1900.[53] Tellingly, we find Arrhenius' influential book *World in the Making* in Uexküll's library.[54] The second more substantial reference would be Uexküll's early contact with the German glass architecture avant-garde, i.e. with Paul Scheerbart, Bruno Taut and Adolf Behne who imagined future, modern humanity to live in glass-enclosed milieus and who, the other way around, integrated Uexküll's *Umweltlehre* into their expressionist *Weltanschauung*.[55] But parallel to this cultural context, there is a third link, which leads us back to the laboratory and which connects Uexküll's scientific imagination to the materiality of his own working space.

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51 Uexküll (1909).

52 Uexküll (1926), p. 71. For the German original cf. Uexküll (1920), p. 58.

53 Jones (1990).

54 Arrhenius (1896), p. 237, Arrhenius (1908), p. 52. Cf. also Wood (1909). The library is part of the Uexküll-Archive at the University of Hamburg.

55 Botar (2001), Gutschow (2005).

Figure 4: Johann Jakob von Uexküll's "Flying Aquarium" in 1914. Source: Johann-Jakob-von-Uexküll-Archiv at the Universität Hamburg.

If we take a look at this photograph of Uexküll's 'mobile' laboratory in 1914, we see that glassware, i.e aquaria, flasks, petri dishes, test tubes etc played an integral role in his experimental practice, as concrete material boundaries, as spatio-causal separators, and we can also imagine why glass containers could eventually have an impact on the thinking of experimentalists. Also, if we take a look at the inventory of a workplace at the Zoological Station of Naples where Uexküll did his early research, we see that he was literally surrounded by bell jars, watch-glasses, beakers, flasks, petri dishes, test tubes, and experimental aquaria.[56] Zoological physiology, like other experimental disciplines, had followed the chemistry's lead and had turned more and more into an 'in vitro' science in late 19th century.[57] Most of Uexküll's objects of research, not seldom living animals, were in fact enclosed in glass and water. In order to understand their *Umwelt*, the biologist had to – metaphorically speaking – reverse the glass *topos* I presented in the beginning of my paper: He had to project himself empathetically on the other side of the glass boundary separating subjects and objects of research.[58] Indeed, a careful reading of Uexküll's texts explicitly shows that his imagery was at the same time very much inspired by his own day-to-day laboratory environment. He not only imagined living beings to be enclosed in "houses of glass", but also in "watch-glasses" and "bell jars".[59]

The intellectual progenitors of Uexküll's *Umweltlehre* are well known. No doubt, he stood on the shoulders of such giants like Gottfried Wilhelm Leibniz, Immanuel Kant and Johannes Müller.[60] But the history of Uexküll's notion of *Umwelt,* obviously, cannot be reduced to this intellectual genealogy. Especially when it comes the spatial boundaries of existence so important to his whole subject-centered biology, it was also the metaphorical dimension, which inspired and

56 Dohrn (1876).

57 Not surprinsingly, this was the time in which the term 'in vitro' was coined. Cf. Rheinberger 2007.

58 The special imaginative potential of the aquarium is evident in Uexküll (1908).

59 Cf. Uexküll (1922).

60 Lassen (1939), Helbach (1989), Mildenberger (2007).

molded his imagination. And one vehicle of this imagery was in fact the glassware that populated his laboratory. Glassware provided an imaginary boundary medium to reintroduce the existential anthropogeocentric enclosure that modern sciences had, in Uexküll's opinion, lost since scientific revolution. Enclosed in glass, mankind's view could be redirected self-reflectively back to its own scientifically neglected, but uncircumventable subjectivity.

Conclusion

In my paper I have presented two historical examples – no more than spotlights of course, on nonetheless two important episodes in the history of experimentation– in order to develop a twofold methodological approach towards a history of boundaries. I have related the historical process of defining and redefining material boundaries and causal-spatial separation in scientific practice to the epistemic "surplus" generated in this process, be it an increased awareness for the glass boundaries in laboratory or, in second order, the epistemic transfer from glass to other material boundaries. A materialist intellectual genealogy of what I have provisionally called *boundary thinking* should ask for the epistemic effects the material boundaries themselves have had on our way of thinking boundaries, be it physico-chemically like in the case of Fritz Haber's glass cell or metaphorically like in the case of Uexküll's *Umwelt*. Glass vessels – in contrast to the *topos* dominant in historiography of science – can bring both historical narratives together: the genealogies of defining and thinking material boundaries.

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The Penetrated Surfaces of Ballistic Photography

Lars Nowak

Historical accounts of ballistic and detonistic photography and cinematography or, to simplify things, of ballistic photography[1] have mostly emphasized the temporal achievements of this kind of scientific image making, namely, its short exposure times, exact moments of exposure and fast sequences of several exposures. However, ballistic and detonistic movements do not only consume tiny portions of time, but also rather large portions of space. In addition, measuring ballistic times is in no way specific to photography and film, but had already been accomplished with the help of chronometric devices long before any ballistic application of these two media or even their invention. What was really new about the introduction of photography into the field of ballistics was that, by visualizing the trajectories of ballistic objects, it made accessible to the qualitative and quantitative observation not only the temporal but also the spatial side of these objects' motions. Within this spatial observation, the relations between surface and depth play a decisive role, as ballistic photography is characterized by a tension between two conflicting tendencies; on the one hand, reducing the three-dimensional spaces of shots and explosions to two-dimensional surfaces, and on the other hand, exploring these spaces' third dimensions as well.

However, far from being limited to an investigation of these representational strategies, this paper will also look at the spatial aspects of ballistics' material culture. For, in addition to representing spatial depth, ballistic photography also creates actual depth by laying several surfaces on top of each other. These superpositions are not always made in order to increase the plates' sensitivity, but sometimes, quite conversely, serve to protect them from the destructiveness of the objects to be depicted. Although being penetrable by depth-revealing methods of visualization, ballistic objects are primarily designed to break through surfaces themselves, and sometimes these surfaces are not only similar, but actually identical to those of photographic plates and cinematic film. As I will ultimately try to demonstrate, these mutual penetrations of the weapons' and photography's surfaces, which sometimes even use the same physical forces, are just one example of a whole set of exchanges between these surfaces, which in turn can be regarded as paradigmatic for the oscillations between the epistemic and the technical object that characterize experimental ballistics in general.

1. Depth versus flatness

If material pictures, which are always two-dimensional, are not ony able to represent spaces that are as flat as they are themselves, but also spaces that possess an additional, third dimension (Günzel 2009: 69), the pictures of ballistic photography employ both kinds of spatial representation. On the one hand, although the central perspective, one of the most compelling means of suggesting spatial depth, is built into every photographic and cinematic image, in ballistics, these two media make extensive use of three special techniques that generate pointedly flat representations. The first of these techniques, called 'shadowgraphy,' simply consists in using back-lighting provided by an electric spark (fig. 1). While even this back-lighting is capable of visualizing the shock waves produced by explosions and supersonic shots, the other two techniques complement it with a special optical apparatus inserted between the ballistic object and the camera in order to bring out

[1] In the following, I will often use the term 'ballistics' for both ballistics and detonics and the term 'photography' for both photography and cinematography.

these shock waves more clearly. In the case of schlieren photography, a schlieren apparatus allows the user to filter out those beams of the back-light that were deflected by the shock waves (fig. 2). Interferometry, in turn, involves an interferometer which splits the back-light into two beams that are directed on different paths and finally superimposed in such a way that they reveal the shock waves by producing characteristic interference patterns (fig. 3). Now, the back-lighting used in all three of these techniques does not only make shock waves visible, but also makes the three-dimensional extension of these waves, as well as of bullets, shrapnel, guns and target objects *in*visible. By transforming these entities into black silhouettes and the environment into an indefinite white or grey expanse, back-lit pictures reduce this three-dimensionality to their own two-dimensionality, which is why they are actually more "appropriate for 2-D phenomena" (Settles 2001: 29).

On the other hand, in a shadowgraph Dayton C. Miller published in his book *Sound Waves* (1937), a projectile which had begun tumbling seemed not to be turned into a shadow itself, but rather, to have cast a shadow onto the picture plane (fig. 4). Moreover, as exemplified by a photograph taken by Harold E. Edgerton, ballisticians have also produced images of ballistic objects that were lit from the front or the side or that emitted light themselves and, therefore, were depicted in all three dimensions (fig. 5). Finally, while for this essay the surfaces of ballistic photography constitute the very object of investigation, ballistic photographers themselves have at times even considered the surface of the perspectival picture an inadequate expression of spatial depth and the surfaces of the objects in that picture an obstacle to its visual penetration. Therefore, ballistic photography also applies techniques that do not negate the depth of space, but, on the contrary, provide means of accessing it that go beyond the possibilites of the central perspective. Although in the context of this publication, not all of the resulting pictures can be presented in a manner that will generate a sense of depth, it can be affirmed that all methods in question were designed to make accessible the third dimension, just as ballistic photography in general tries to uncover hidden aspects of the fourth dimension. Nevertheless, although opposed to the back-lighting methods in this respect, the depth-exploring techniques are quite often combined with the former types of image production and sometimes even bear great resemblances to them.

2. Multiple perspectives and stereoscopy

To begin with, many of the three-dimensional techniques are, paradoxically, applied to back-lit objects. For instance, in an aeroballistic range described by Peter Fuller (Fuller 1997b: 224-225), a projectile is illuminated by two sparks and casts two shadows on screens at right angles, which are then photographed by separate cameras (fig. 6).[2] Although here, too, the projectile is reduced to silhouettes, its position and orientation are captured from two perspectives, allowing a reconstruction of its trajectory in all three dimensions of space.[3]

This ambivalence also characterizes the spark cinematograph invented by Lucien G. Bull, an assistant of Étienne-Jules Marey, in 1904 (fig. 7). On the one hand, this device, too, used back-

[2] In its basic form, this ballistic range dates at least as far back as the 1950s. In a variation of it that was described by Edward McShane, John Kelley and Franklin Reno, the shadows were cast on two photographic plates, a vertical and a horizontal one. The bullet passed through a spark gap and triggered a discharge spark in a second gap, which immediately produced a shadow picture on the vertical plate and, mediated by a mirror, a second picture on the horizontal plate. (McShane/Kelley/Reno 1953: 685-686)

[3] In addition, since several spark units are placed in regular distances along the projectile's trajectory, several pairs of photographs are produced, so that the fourth, temporal dimension of the bullet's motion is captured as well.

lighting which reduced the subject matter, including ballistic events such as shooting a projectile through a soap bubble (fig. 8), to shadows. On the other hand, in order to eliminate certain ambiguities of the back-lit pictures with regard to the position, orientation and movement of the objects in the depth of space, it could be equipped with two spark gaps, two objectives and two film strips (fig. 9). The right side of figure 9 shows that the two gaps, which were connected in series and thus discharged simultaneously, could be placed at right angles, so that, in a way similar to Fuller's aeroballistic range, two perspectives on the same event being perpendicular to each other were generated.[4] Alternatively, as indicated by the left side of the figure, the two spark gaps could be placed next to each other. Since the distance between the two objectives corresponded to the interocular distance, a person watching the two films produced in this way through a stereoscope or another device that presented each of them only to one eye would have an impression of three-dimensional space that was immediately evident. (Bull 1910: 61-63; Vivié 1967: 14) In fact, *most* of Bull's preserved films, some of which also show ballistic events such as back-lit shots through soap bubbles, are stereoscopic (Lefebvre/Mannoni 1995: 146).

Others have taken back-lit stereographs of shock waves as well, as is demonstrated by a stereoscopic schlieren photograph made by the German ballistician Wolfgang Struth in 1963 (Struth 1963: 444, 446) (fig. 10). Stereographs with frontal and lateral lighting were in fact among the first ballistic photographs ever made, as images of this kind were already produced by Thomas Skaife in 1858 (Skaife 1858a; Skaife 1858b; Skaife 1858c) (fig. 11) and George Mackinlay in 1866 (Volkmer 1894: 17-21) (fig. 12). Later, this sort of lighting for stereoscopic imagery of ballistic objects was also transferred from photography to cinematography. In 1940, Ewald Fünfer, a physicist at the *Technische Akademie der Luftwaffe* in Berlin-Gatow, presented a secret report in which he described a new stereoscopic high-frequency film camera which could record fast-moving objects that reflected or emitted light, such as explosions, by using two Kerr-cells as shutters (Krehl 2009: 521). Even some of the semi-popular instruction and propaganda films about the American nuclear tests of the 1950s that were produced by the U.S. Air Force 1352nd Photographic Group made use of the new 3-D-techniques that were developed for commercial cinema in those days (Kuran 2006: 38-39, 48).[5]

3. Holography

As anyone who ever looked through a stereoscope knows, stereoscopy's ability to simulate spatial depth is limited. Although this technique creates an immediate sense of three-dimensionality, this effect is restricted to the spatial relations between the objects, while the objects themselves appear as flat as the silhouettes of a back-lit picture. Thus, the spatial depth constructed by stereoscopy merely consists of a positioning of several surfaces behind each other. Franz P.

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4 However, Bull's high-speed camera differed from the aeroballistic range in that it re-routed the light emitted by one of the sparks with the help of a mirror. Bull admitted that the diverted beam had to travel a longer way to reach the film and thus produced a smaller picture than the other one, but added that this could be compensated for by placing a lens in the first beam's path, thereby enlarging the image. (Bull 1910: 62-63)

5 A long time before this, Carl Cranz had recognized stereophotography's potential for making the photogrammetric determination of the positions of ballistic objects in the open field more precise. However, although Cranz also pointed out that watching the stereophotogrammetric pairs of images through a stereoscope gave rise to a spatial visual impression, in this case, the impression was artificial. The reason is that Cranz's efforts towards precision resulted in placing the two cameras that took the pairs of photographs not in the interocular distance, but in far greater distances, which could amount to several hundreds of meters. (Cranz 1927: 285-315)

Liesegang, in his book *Wissenschaftliche Kinematographie* (1920), considered this limitation so severe that, for him, it excluded stereoscopy from any scientific application (Liesegang 1920: 204).

However, in 1966, the Americans Ralph F. Wuerker, Robert E. Brooks and Lee O. Heflinger, who worked at the Physical Electronics Laboratory of TRW Systems, produced holograms showing the head waves of bullets flying through different gases (Brooks/Heflinger/Wuerker 1966) (fig. 13)[6], and two years later holograms of ballistic shock waves were also made by R.D. Buzzard (Buzzard 1968). Invented in 1947 by Dennis Gabor, holography was made applicable to ballistics by its combination with the laser, which had been developed by Gordon Gould, Theodore Maimon and others in the early 1960s. Lasers not only produced the coherent light optical holography was dependent on, but they could also be adjusted to give a pulsed output, and their extraordinarily short pulse durations going down to a few picoseconds (Fuller 1997a: 44, 46; Fuller 1997b: 211) even undercut the exposure times that could be achieved with the help of the electric spark. Due to the laser's high energetic intensity, these pulse durations still provided enough light to expose a photographic film (Fuller 1997b: 211), making it possible to take sharp pictures of ballistics' extremely fast-moving objects.

Apart from the shock waves generated by supersonic shots, those of explosions can be captured in holograms as well. For, while the light emissions of explosions, which are sometimes very bright, make it difficult to photograph these ballistic events with ordinary means, the laser's sharply defined frequencies enable the experimenter to filter out their light (Fuller 1997a: 46; Fuller 1997b: 211). Last but not least, holography can be used to visualize the surface deflection and the debris formation caused by a projectile's impact. While this application to terminal ballistics is the most important usage of holography within ballistics (Fuller 1997b: 227; Hough 2005: 299, 301-302), the visualization of ballistic events in general is among the primary uses of this technique whatsoever, as was even acknowledged by none other than Gabor himself[7].

Just as the technique of stereoscopy can be applied to both photography and cinematography, repeatedly pulsed lasers make it possible to produce a whole series of holograms (Hough 2005: 299, 304-305; Racca 1997: 333-336). However, holography's advantage over stereoscopy is that it provides much more information about spatial depth, since it allows its user an actual reconstruction of the parallaxes as well as later displacements of the focal plane (Racca 1997: 332).

At the same time, holography is even more closely related to ballistics' back-lighting techniques than are multiple-perspective and stereoscopic photography. Like these, it can be combined with all three of these techniques (Krehl 2009: 1011). For example, while Buzzard's holographic experiments incorporated the schlieren method, Wuerker, Brooks and Heflinger had combined the holography with interferometry, since their holograms had superimposed two exposures, "the first without and the second with the bullet and shock wave present" (Brooks/Heflinger/Wuerker 1966: 646). However, what is more important is that holography is connected to all three back-lighting methods by analogies. For although the laser it employs differs from the light emitted by an electric spark in being coherent, monochromatic and very intensive, holography, like shadowgraphy and schlieren photography, can work without an objective and, like interferometry, is based on the superposition of two light beams, an object beam and a reference beam (Brooks/Heflinger/Wuerker 1966: 642).

6 These were the first holograms of shock waves in general (Krehl 2009: 690).

7 In his Nobel Prize speech of 11 December 1971, Gabor referred to the progress made in holographic interferometry and illustrated this with a picture taken by Wuerker, which showed the interference of a supersonic bullet's shock wave with another shock wave (Krehl 2009: 562).

4. Radiography

Nevertheless, holography coincides with multiple-perspective and stereoscopic photography in that all three techniques only penetrate the two-dimensional surface of the picture itself. This is different with X-rays, which can pierce the surfaces of opaque objects *within* the picture. Although in popular imagination, X-rays are mostly used to visualize the interior of living bodies, it is as popular to understand the resulting pictures with their emphasis on the skeleton as a modern kind of *memento mori*. Thus, it comes as no surprise that X-rays also have been applied to the lifeless and deadly objects of ballistics. Interestingly, Wilhelm Röntgen himself, who discovered this kind of radiation in 1895, took an X-ray photograph of his own rifle just one year later (Krehl 2009: 132). While the weapon in Röntgen's picture was motionless, in 1938 the German physicist Max Steenbeck took the technique one step further, making X-ray images of bullets penetrating softer and harder types of wood (Steenbeck 1938) (fig. 14). Steenbeck, who worked for Siemens, used the X-ray flash tube he had invented immediately before, an invention that was also made independently in the same year by the American physicists Kenneth H. Kingdon and H.E. Tanis of General Electric (Jamet/Thomer 1976: 15). In both cases, the tube could generate X-ray flashes of about 1 µs. Flash durations between 1 and 0.1 µs are still used in ballistic radiography today. A few years later, during the Second World War, the X-ray flash became an important diagnosis instrument in detonics. While this was particularly the case in Germany (Krehl 2009: 132), the X-ray flash also played a decisive role in the American Manhattan Project, where the perfect three-dimensional symmetry needed for the implosion used to ignite the plutonium bomb was not only examined by stereoscopic (Szasz 2006: 176) but also by radiographic (Hoddeson/Henriksen/Meade/Westfall 1993: 277-278; Rhodes 1986: 573-574) means (fig. 15).[8]

As demonstrated by the radiographic sequences shown in figures 14 and 15, ballistic radiographs, like multiple-perspective photographs, stereographs and holograms, were also produced in sequences. Having been discovered in the same year the medium of film was invented, X-rays were already combined with cinematography just one year later by the Scottish scientist John Macintyre, who was joined a little later by Jean-Charles Roux, Victor Balthazar, Joachim-Léon Carvallo, P.H. Eijkman, Franz M. Groedel and Lewis G. Cole (Tosi 2005: 169-171). Although at the beginning, the picture frequencies of X-ray films were quite low due to the long exposure times of early radiography[9], they later reached a value of 150 pictures/s, which was sufficient for ballistic purposes, and after that even increased to 1,000,000 pictures/s (Fuller 1997a: 42)[10].

Apart from terminal ballistics and detonics, exterior, intermediate and interior ballistics belong to radiography's fields of application as well. In all these areas, X-rays do not only penetrate guns, target objects, dust, smoke and opaque gases, but are also able to visualize shock waves. Since they are hardly reflected or refracted, this cannot be achieved by combining them with shadowgraphy, schlieren photography or interferometry. However, X-rays can make density variations visible because they are absorbed and diffracted. Thus, it is not only possible to take radiographs of shock waves in fluids and gases by using soft X-rays and very sensitive photo

8 In the development of implosion detonators for later nuclear bombs, the X-ray flash was still a preferred and sometimes even the only means of visualization (Krehl 2009: 132), while it is also applied to *ex*plosions (Held 1997: 242-243).

9 While Groedel only obtained 4 pictures/s, Jean Comandon and André Lomon, in 1911, still worked with a pulsed X-ray source that was not able to expose more than 16 pictures/s, which equalled the standard speed of film at that time (Tosi 2005: 170-171).

10 Today, frequencies of 1,000-10,000 pictures/s are typical and usually applied to up to 50 pictures, a maximum dictated by the anode's wear. Although usually based on flash tubes, X-ray films can also be produced by means of tubes emitting X-rays for longer durations, which are then interrupted by an image converter that exposes a moving film strip. (Fuller 1997a: 41-42)

emulsions (Krehl 2009: 132-133), but additionally, unlike all the other imaging techniques deployed in ballistics, radiography can even detect shock waves in solid objects such as explosive charges or targets hit by a projectile (Held 1997: 232; Krehl 2009: 132).

Being unfit for a combination with ballistics' back-lighting techniques, radiography, like holography, also differs from them in using another kind of electro-magnetic radiation. But, once again as in the case of holography, it also resembles all of them, since X-rays, like electric sparks, are produced by discharging a high voltage across a pair of electrodes and the X-ray tube itself is also placed behind the object to produce a shadow picture (Fuller 1997a: 42; Held 1997: 241; Krehl 2009: 132).

5. Combination of techniques, superposition of surfaces

Thus, ballistic photography and cinematography employ numerous depth-revealing techniques to penetrate their objects in all conceivable ways, and sometimes these methods are even combined with each other, as is the case, for example, when two X-ray tubes are directed towards a ballistic event from two different perspectives (Held 1997: 241). At the same time, two of these techniques do not only pierce surfaces, but also stack several of them. For, just as every hologram is based on the superposition of two light beams, several holograms belonging to the same sequence can be superimposed as well (Racca 1997: 335-336; Hough 2005: 304-305)[11], a method that is also practiced in serial radiography, where it is possible to lay up to five pictures on top of each other (Fuller 1997a: 41)[12]. Also, the stacking of several layers is one of the methods of increasing the sensitivity of X-ray plates and thus reducing their exposure times, which were initially quite long due to emulsions with a low sensitivity and weak X-rays.[13] Whereas Carvallo, who, like Bull, worked at the *Institut Marey* and began making X-ray films at around 1900, raised the sensitivity of cinematic film to X-rays by coating it on both sides (Tosi 2005: 170), in ballistics, the sensitivity of the X-ray plate is mainly improved by overlaying it with additional surfaces, namely, fluorescent screens, which do not only transform the X-rays into visible light, but also intensify them (Held 1997: 241). While in the beginnings, these screens were put up at some distance from the radiographic plate, today the latter is usually enclosed between two of them and the whole arrangement placed in a cassette (Fuller 1997a: 42; Held 1997: 241). As each intensifying screen is in itself an assemblage of several layers (Cavailler 1997: 348-349), a large number of surfaces is superimposed here.

6. Penetration of surfaces by ballistic objects

However, shots and explosions are not only pierced by photography and film, but also serve to pierce surfaces such as armour-plates, fortress walls or bunker walls themselves. This is already implied by the stereoscopic and radiographic images of shots through soap bubbles and pieces of wood presented in figures 8 and 14, for just as these objects were optically penetrated by Bull's stereoscopic and Steenbeck's radiographic techniques, they were mechanically penetrated by the projectiles. Moreover, although conventional weapons break through their targets by mechanical

[11] If at every exposure the reference beam or the object beam hits the photographic plate from a different angle, a small number of images can even be superimposed in such a way that, during the latter's reconstruction, it is possible to separate them once again (Racca 1997: 335-336). Naturally, serial holograms can also be *juxta*posed.

[12] Levy Dorn produced sequences of radiographs that overlapped each other on a rotating disc (Liesegang 1920: 155). If instead, a *juxta*position of the radiographs is preferred, several X-ray tubes are arranged in a parallel, orthogonal or diagonal fashion (Fuller 1997a: 41-42).

[13] For instance, Eijkman only achieved 1/50 s, while Comandon and Lomon still used a pulsating X-ray source that produced exposure times of 1/32 s (Tosi 2005: 170-171).

and thermal means, others even employ the same kinds of electro-magnetic radiation used by ballistic photography and film for their optical penetrations. Since laser beams can vaporize very hard materials, several attempts have been made in the U.S.A., the U.S.S.R. and other countries since the 1970s to build laser cannons that fend off rockets and grenades by heating and thus disturbing or destroying their optical and electronic components. For various reasons, which include the high energy consumption of lasers and their susceptibility to atmospheric disturbances, even today these weapons have not yet reached a mature level of development. However, X-rays played a role in weapons research as well. In this case, the weapons achieved full maturity and were even deployed in actual combat, for, although the gamma rays emitted in the detonation of a nuclear bomb differ from X-rays with regard to their origins and their photon energy, the two kinds of radiation have wavelengths that overlap in an area between 2 and 40 pm. Thus, hard X-rays, which are employed in ballistics for the piercing of hard materials, possess the same wavelengths as soft gamma rays. Finally, even the ordinary visible light used by ballistics' multiple-perspective and stereoscopic image techniques can be used for destructive ends in the nuclear explosions' light flash, which, due to its extreme intensity, can blind human eyes, as was already pointed out after the *Trinity* test (Hoddeson/Henriksen/Meade/Westfall 1993: 377, 485). Thus, just as toxicity is a property not of a particular substance, but of a particular dosage of a substance, X-rays, laser and ordinary light, depending on their intensity, can be used for the purposes of visualization as well as destruction, a fact that was revealed in ballistic photography itself when at the *Institut Franco-Allemand de Recherches de Saint-Louis*, Paul Smigielski and Antoine Hirth made laser holograms of a shock wave generated by another laser (Smigielski/Hirth 1970: 323) (fig. 16.1, 16.2).

For these reasons, ballistic experimenters who employ holography and radiography do not only have to be wary of the ballistic event to be recorded, but also of the means of recording it. While laser beams, like the nuclear flash, can harm their eyes (Fuller 1997a: 46-47), X-rays are particularly dangerous to their germ cells (Fuller 1997a: 43-44). For the same reasons, the surfaces of photographic plates and cinematic films are not only optimized for their reception of the electro-magnetic rays utilized for the recording of that event, but must also be protected against the latter's destructive effects.

One of the simplest protections against mechanical and thermal damage is increasing the distance between the ballistic object and the photographic equipment. For example, in radiographic experiments both the X-ray tube and the X-ray plate, which enclose the target object or the explosive device, are placed as far away from it as possible (Jamet/Thomer 1976: 132), and this applies to the Marx pulse generator feeding the tube as well (Held 1997: 241). At the U.S. government's atmospheric nuclear tests, the photo and film cameras were even positioned at distances of hundreds of meters from the epicentres of the detonations, while these explosions' massive light emissions were reduced by equipping the cameras with dark filters or extremely long, light-absorbing telescopes, which at the same time optically compensated for the large distances.

From this essay's point of view, though, the more interesting steps taken to protect the photographic surfaces are those complementing them with other surfaces. Sometimes, these measures serve to compensate for drawbacks of the other methods. For example, in radiography, the reduction of the picture's sharpness resulting from an extension of the distance between the X-ray plate and the ballistic object can be avoided by using a fluorescent screen which casts its light on a mirror that transmits it onto the plate, eventually capturing the image before the screen and the mirror are destroyed (Fuller 1997a: 43). Also, reducing the light emitted by the atomic explosions averted heavy overexposures by the nuclear flash, but also caused *under*exposures during later, darker stages of the detonations. This problem was solved when Charles Wyckoff of Edgerton, Germeshausen & Grier, in cooperation with Eastman Kodak, invented the XR film

(XR=extended range), in which three panchromatic emulsions with different sensitivities were applied in layers. In order to reduce the film's density, Wyckoff coded its different emulsions in artificial colors, using cyan for the slow layer, magenta for the medium-speed layer and yellow for the high-speed layer.[14] (Kuran 2006: 56-57, 135)

In other cases, the added surfaces serve as protective shields against flying debris, shock, heat and radiation. While Alfred Siersch, who, around 1900, photographed the light emissions of small conventional explosions to find out which explosives were safe enough to be employed in mining, could be content to place his camera behind a wooden wall with a viewing hole and the lens behind a glass plate (Siersch 1895-96: 7) (fig. 17), the photographic equipment used at the American nuclear tests was concealed in heavy bunkers, such as the one built for the new high-speed cameras that in 1952 recorded the test *Ivy Mike*, the world's first detonation of an H-bomb, at a frequency of 3,500,000 pictures/s (Brixner 1993: 54) (fig. 18). In order to protect the photographic films from the radioactivity, the cameras were also covered with lead plates whose thickness varied in accordance with the respective distance from the epicentre and the sensitivity of the film (Kuran 2006: 68-69, 104). These measures were of particular importance at the thermonuclear tests with their heightened amount of radioactivity. Here, the cameras were not directed at the explosions themselves but at mirrors reflecting their image, and even the lenses were covered with heavy plates of lead glass. (Kuran 2006: 77)

Although smaller damages of a holographic plate would be acceptable as every part of such a plate contains all its information (Hough 2005: 303), these plates, too, are put into a refractive index-matching fluid and placed behind sheets of polycarbonate (Hough 2005: 303, 306), a material that is also used for bullet resistant windows. In a similar way, the fluorescent cassettes of X-ray plates are usually enclosed in a second cassette made of thick aluminium, which shields the plate from mechanical damage as well as from visible light, while at the same time being permeable to the X-rays themselves (Fuller 1997a: 42-43; Held 1997: 242). The latter protection widens the number of radiography's potential uses. For example, just as in holography, in which the light emitted by explosions can be filtered out due to the laser's narrow range of frequencies, the protective cassette makes it possible to apply the radiographic method to self-luminous objects such as explosions (Fuller 1997a: 40; Krehl 2009: 132) or the ablation of supersonic bullets, which in ordinary photographs is concealed by the luminosity of the surrounding air (Jamet/Thomer 1976: 111-112).[15] Thus, in ballistic radiography, the stacking of several layers does not only serve to intensify the plate's sensitivity to the X-rays, but also to protect it against the penetration by other physical forces. In this process, up to 18 surfaces can be overlaid (fig. 19).

Nevertheless, since the efforts to protect the photographic surfaces are not always successful, these, too, are sometimes penetrated by the weapons' destructive effects, a phenomenon that typically occured at the U.S. nuclear tests. At several of these tests, the detonations' excessive light emission did not only temporarily blind human observers, but also overexposed photographic plates, which were turned completely white and, thus, produced and destroyed in the same physical process. At the *Trinity* test, where no earlier experiences were at hand and the theoretical calculations heavily underestimated the amount of light to be expected, the overexposures reached such a high degree that several films were solarized, melted and perforated (Amelunxen 1992: 36),

[14] The XR film became the forerunner of many color negative films, including the type of film that was deployed for recording the first moon landing (Kuran 2006: 57).

[15] Additionally, blocking out the visible light makes radiography employable in light spaces, including outdoor spaces, without the necessity of using a camera shutter (Held 1997: 242). However, the protective cassette also absorbs a part of the X-rays and thus diminishes the intensifying effect of the inner fluorescent cassette (Cavailler 1997: 348-349), a fact which emphasizes that X-rays can be as destructive as the flying debris produced by explosions and the impact of projectiles.

as can be seen, for instance, in two pictures that were published in *Atomic Energy for Military Purposes* (1945), Henry D. Smyth's official report on the Manhattan Project (fig. 20.1, 20.2). The test explosions' radioactivity even damaged photographic stock over long temporal intervals and large spatial distances. For instance, although the journalists taken to the *Trinity* test site two months after the event were only allowed to stay for 20 minutes, some of their photographic films were affected by the radioactivity still lingering in the bomb crater, a fact *Life* brought to its readers' attention by publishing one of the strange, amorphous pictures that resulted from these processes (Anonymus 1945: 30) (fig. 21). The radioactivity's detrimental effects on photographic plates that were hundreds and thousands of kilometers away from the test areas are exemplified by operation *Ranger* of 1951, the first test series conducted at the Nevada Proving Ground, whose fallout contaminated snow, which then came down in Rochester where it ruined film stock at Eastman Kodak (O'Brian 2008: 142).

7. Functional exchanges between epistemic and technical objects

Thus, in ballistics, the boundary between the surfaces in front of the camera's lens and those behind it has been crossed by mutual penetrations of these surfaces. These penetrations illustrate a whole set of interactions between these two kinds of surfaces, which, for instance, also exchanged their potence for danger and their epistemic activity. On the one hand, it is well known that nitrocellulose is not only made into celluloid used for cinematic roll film by adding camphor, but also suited for the production of bombs (Virilio 1989: 26; Starl 1992: 7; Kittler 2002: 195). In fact, nitrate, the standard material for 35-mm films during the first fifty years of cinema's history, is flammable to such a high degree that during their projection, these films were always in danger of being set on fire by the projector's heat. Obviously, cinematic surfaces can be as dangerous as explosives. On the other hand, the surfaces of ballistic target objects can be as informative as those of photography and cinematography. For instance, the last section of Hubert Schardin and Wolfgang Struth's high-frequency film *Beschuß von Drähten und Panzerplatten* (1937) demonstrates that projectiles are liquefied and partially evaporated when striking armour-plates at a very high velocity which are stronger than they are themselves (fig. 22.1, 22.2). However, this peculiar phenomenon is also represented by photographs of the "permanent splash formations" left by such impacts (Edgerton/Killian 1954: 54). Since these pictures, examples of which can be found in publications by Arthur Worthington (Schaffer 2004: 187-188) or Edgerton (Edgerton/Killian 1954: 54) (fig. 23), were taken *after* the event, they show something that could as well be recognized by inspecting the terminal objects' surfaces themselves.

The exchanges between both types of surfaces, in turn, exemplify a multitude of transgressions over the boundary between the epistemic and the technical object which are characteristic of experimental ballistics in general. If, according to Hans-Jörg Rheinberger, the epistemic objects of experimental systems are never fixed but constantly displaced (Rheinberger 1992: 47-65), these displacements apparently include crossings of the border which separates them from the technological objects, for Rheinberger presents his distinction between epistemic and technological thing as relative instead of absolute. Since the two concepts can be applied to the same material entity, the difference between them is not material but functional in nature (Rheinberger 1992: 69). However, although Rheinberger claims that their "nichttriviale[n] Beziehung" (Rheinberger 1992: 70) includes mutual exchanges and even transformations (Rheinberger 1992: 71), his more detailed description of it denotes two rather unilateral processes. According to him, technical objects participate in the construction of epistemic objects, and epistemic objects, by way of their gradual stabilization and their participation in the construction of new epistemic objects, are themselves transformed into technical objects (Rheinberger 1992: 70). By comparison, nowhere does Rheinberger mention inverse transformations of technological objects into epistemic ones or material entities that oscillate between both identities. Here, ballistic photography offers the chance to exhaust the full potential of Rheinberger's general characterization of the relationship between the epistemic and the technical thing, which is actually very interesting. For in this field, several objects, such as flashes and shock waves, play both roles at the same time.

8. Assimilations of represented to representing surfaces

Returning to the surfaces involved in ballistic photography, it can be added that the boundary between those before and those behind the camera's lens is also transgressed by numerous assimilations of the former to the latter. Just as the photographic plates destroyed by the nuclear test explosions also *represented* these destructions, likewise, many of the other objects whose mechanical penetrations were registered by the photographic and cinematic images of ballistics bore striking resemblances to those pictures, regarded as physical objects, themselves. For one, the soap bubbles filmed by Bull were virtually reduced to pure surfaces like any material image and as transparent as the glass negatives of early photographs and the celluloid of cinematic films. Glass spheres and light bulbs, whose ballistic penetrations were filmed by Bull as well (Lefebvre/Mannoni 1995: 148; Bull 1924: 10), were also made of the same material as the negatives, a shared feature that was complemented with flatness in the glass plates pierced in ballistic photographs and films produced by Charles V. Boys (Boys 1893: 443-445) and others (fig. 24). Similarly, the flatness and the material of photographic prints was shared by the paper screens whose penetration by a torpedo was photographed by Albert Londe and Hippolyte Sebert at around 1890 (Londe 1890) (fig. 25). Finally, penetrations of surfaces that are not perpendicular to the picture plane but parallel to it can even give the impression of a shot through the picture itself, as is the case in Hubert Schardin, Wolfgang Struth and Dietrich Elle's film *Der Bruchvorgang im Glas* (1942-44), which, among other things, shows a glass plate parallel to the picture plane being pierced by a bullet from an angle of 25° (fig. 26.1, 26.2, 26.3).[16]

However, no picture draws the analogy between the penetrations of the ballistic target objects and those of ballistics' photographic plates more clearly than a photogram made by Edgerton in 1962, which represents a projectile piercing a Perspex plate while being illuminated by a flash tube placed behind it (fig. 27).[17] For here, the two kinds of piercing are not merely assimilated to each other, but actually united in the same image. Like the cracks of the glass plate in *Der Bruchvorgang im Glas*, the little shock waves surrounding the plate's splinters look like breaks in the photogram itself. But what is really important here is that a fraction of a second after the exposure, the slivers hit the photographic plate, actually perforating it on its right side just as the bullet had penetrated the Perspex plate.

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[16] This film also presents other glass plates with particular analogies to photography's own surfaces. Some plates possess an elongated shape that reminds the viewer of a film strip. Others are mirrored plates on which indexical images could appear as if they were photographic surfaces. Finally, there is a shot through two cemented glass plates which resemble photographic plates and cinematic films in that these, too, often comprise several layers, which, as mentioned before, applies to radiography in particular.

[17] I call this picture a 'photogram' because, although it is not based on a direct physical contact between the photographic plate and the object depicted, it was produced without a camera – a peculiarity that is shared by a large number of ballistics' photochemical images.

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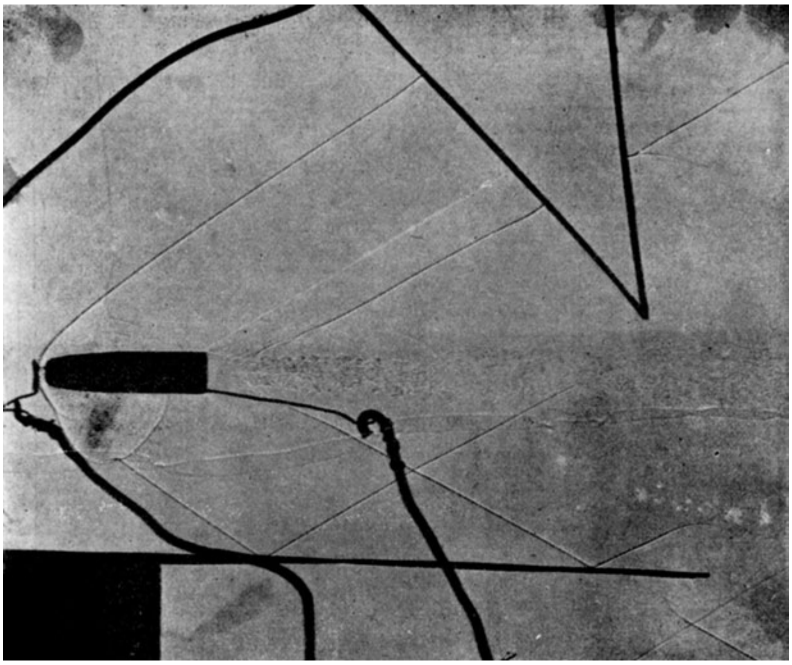
Figures



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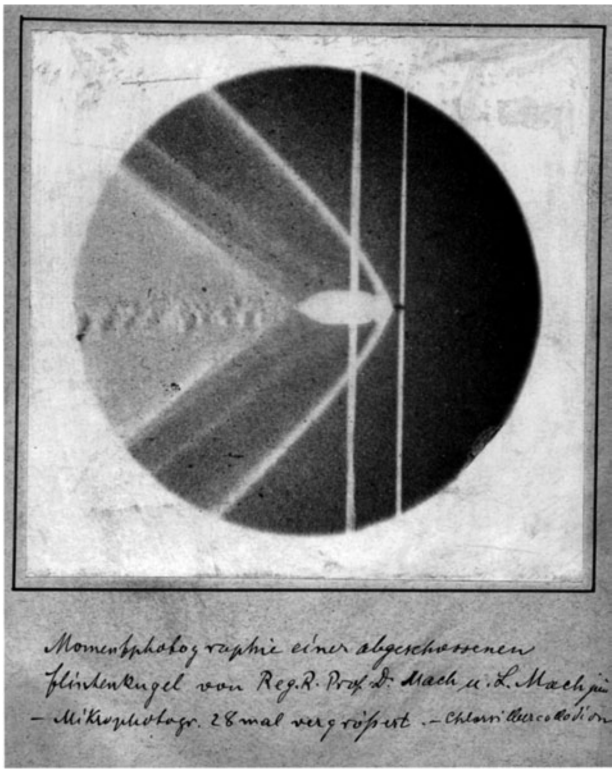

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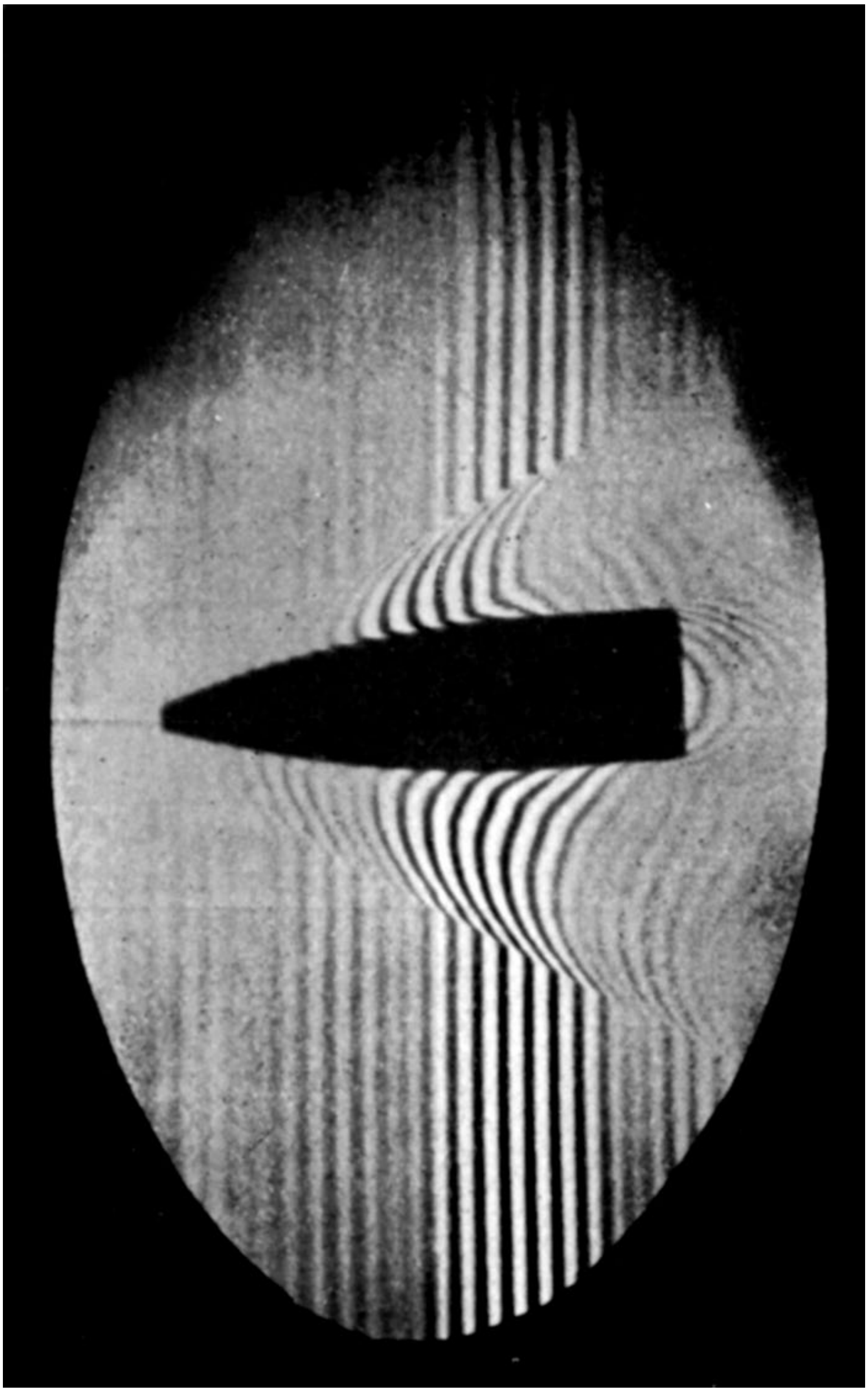

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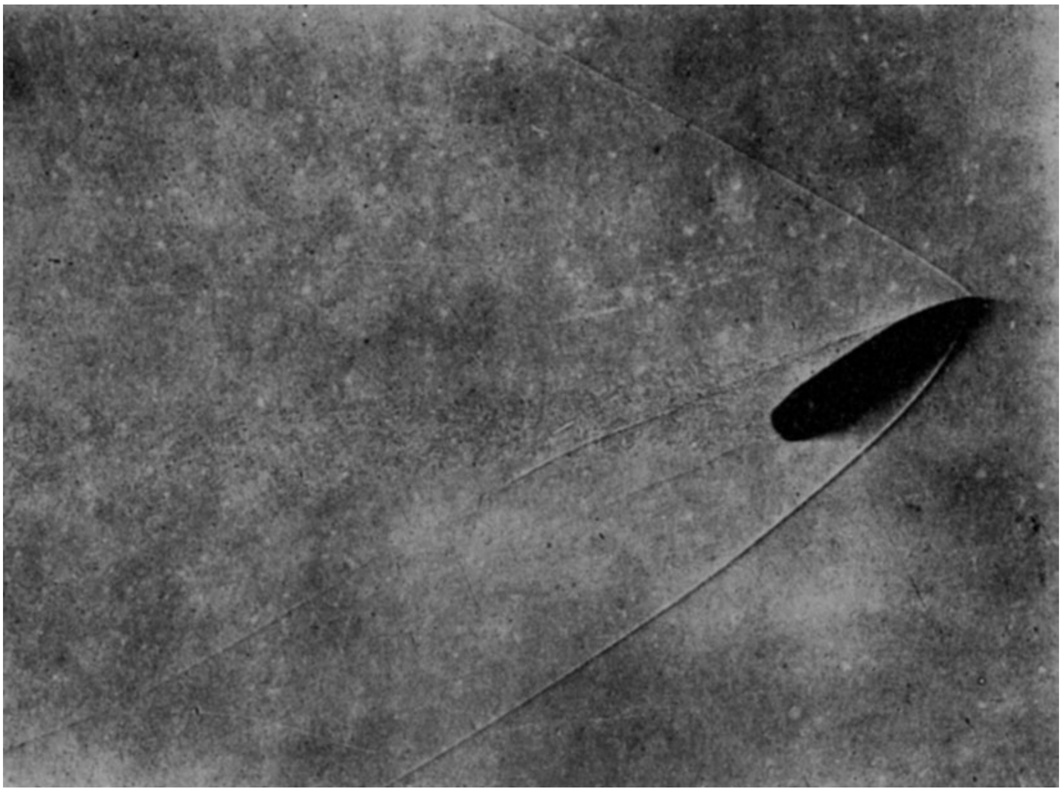

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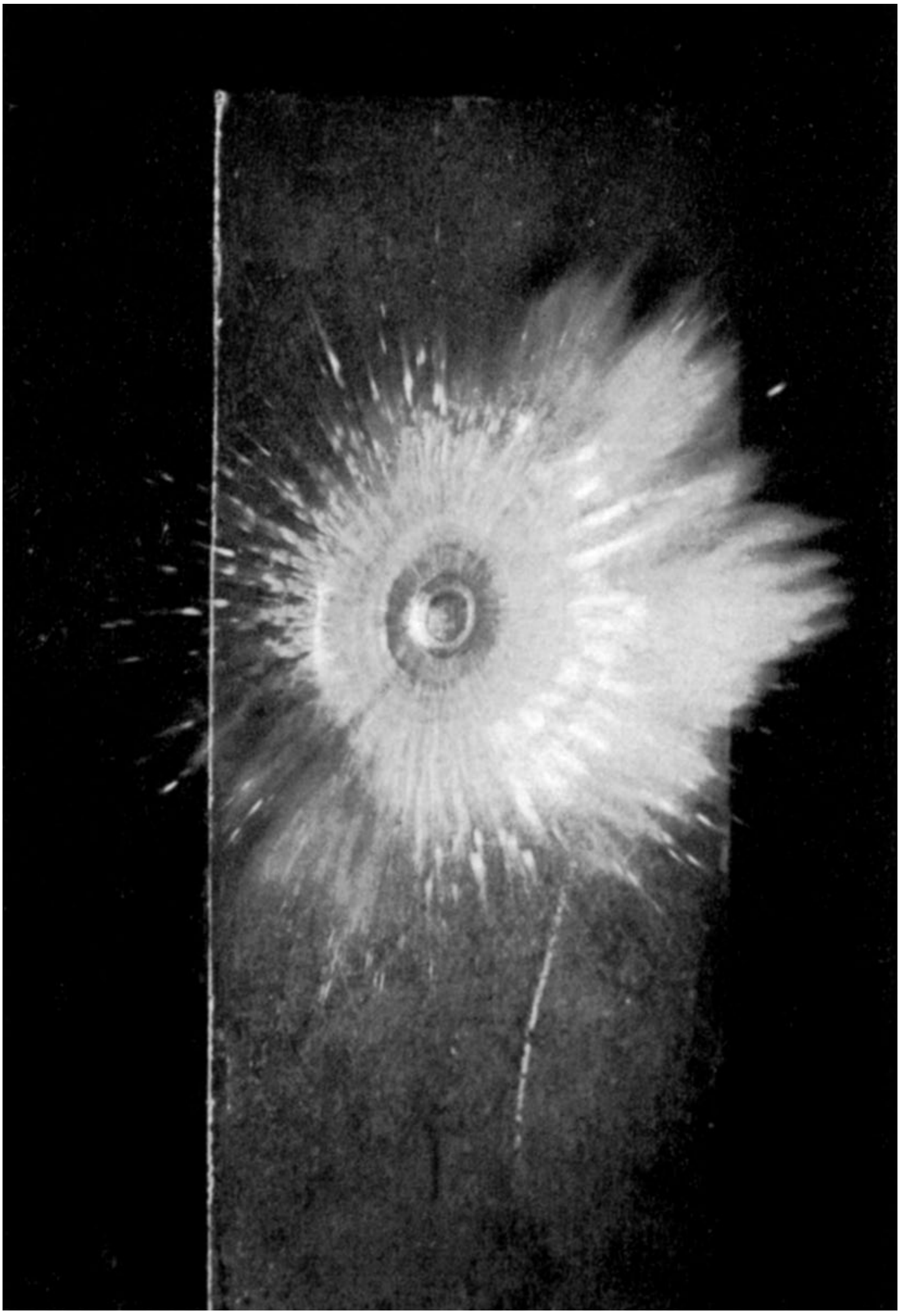

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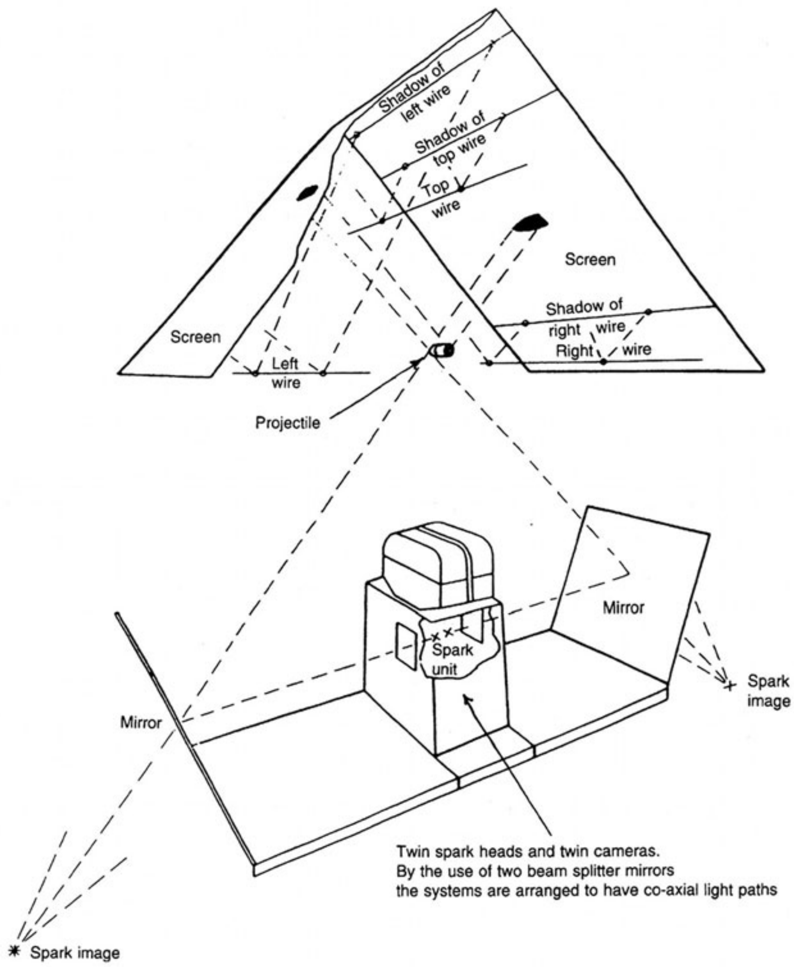

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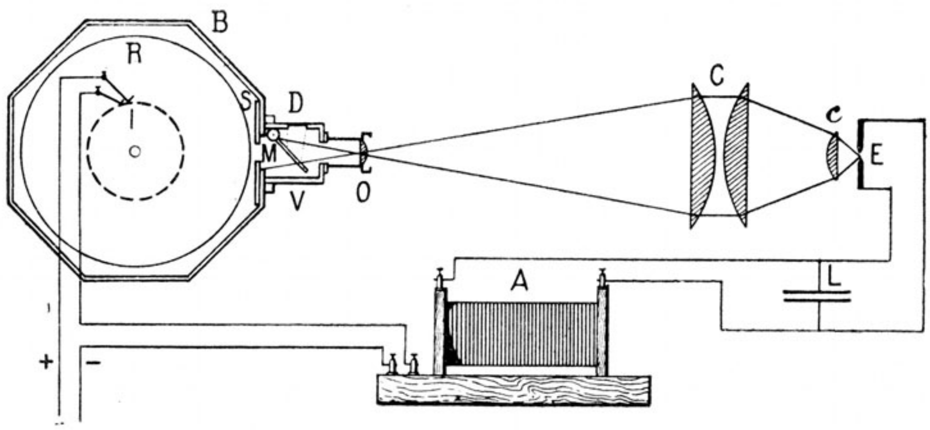

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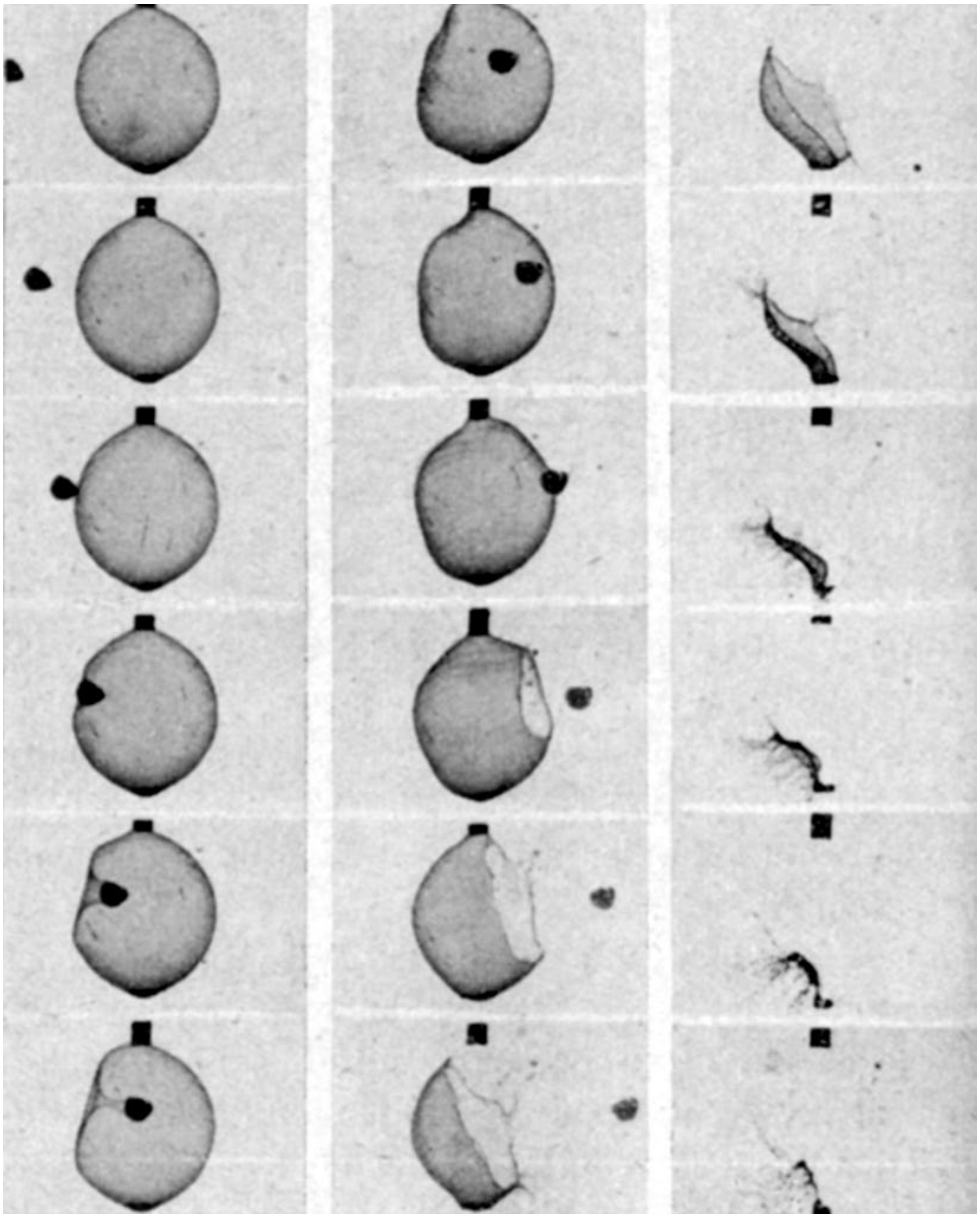

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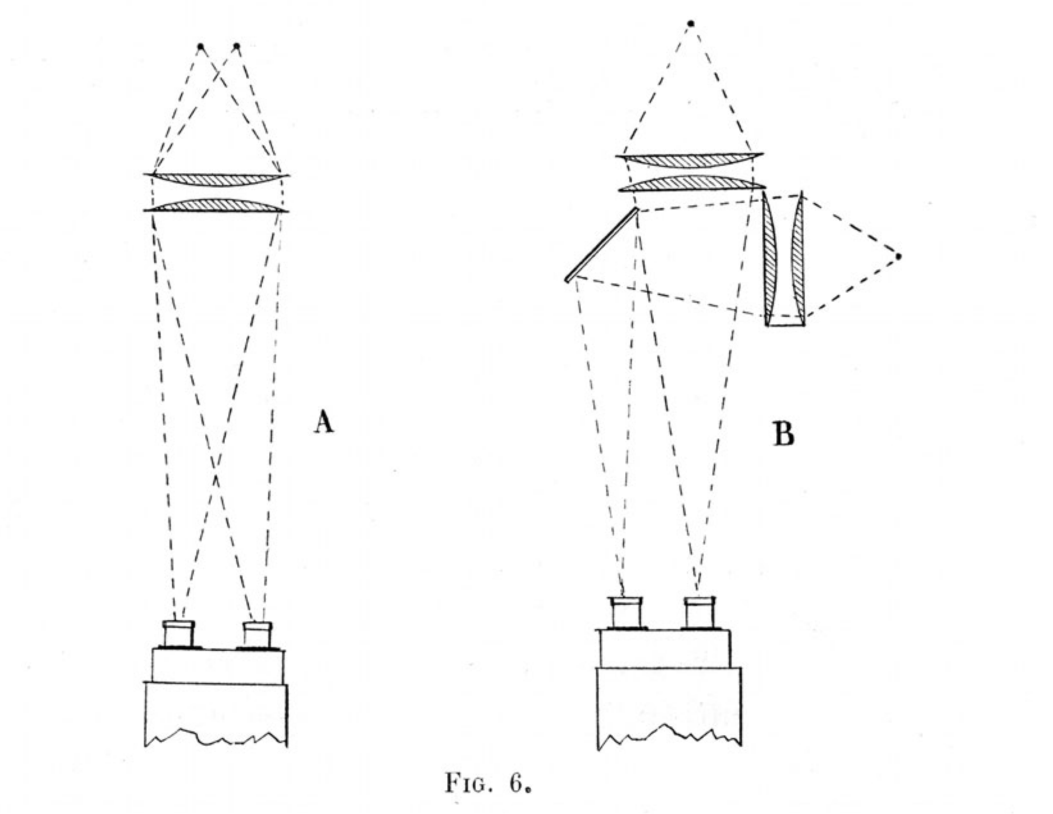

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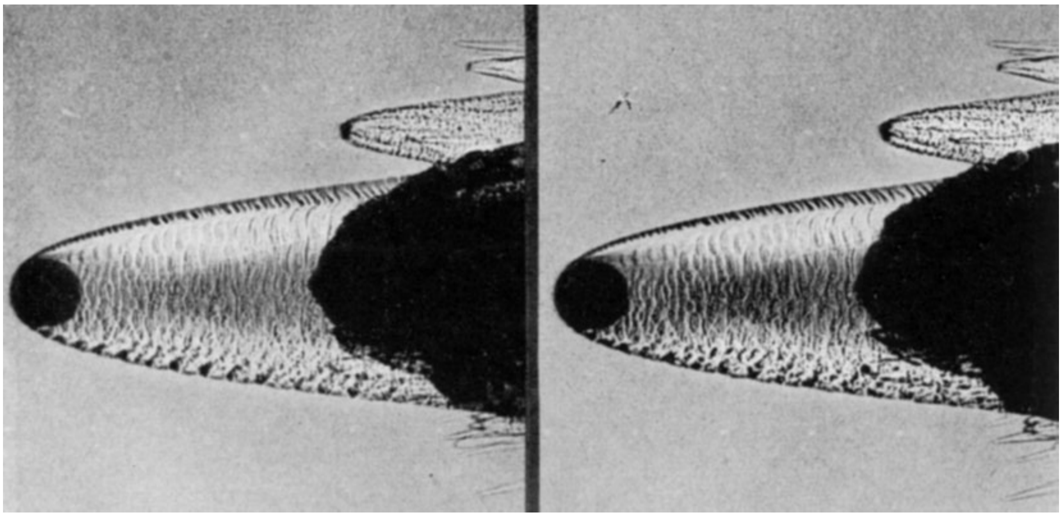

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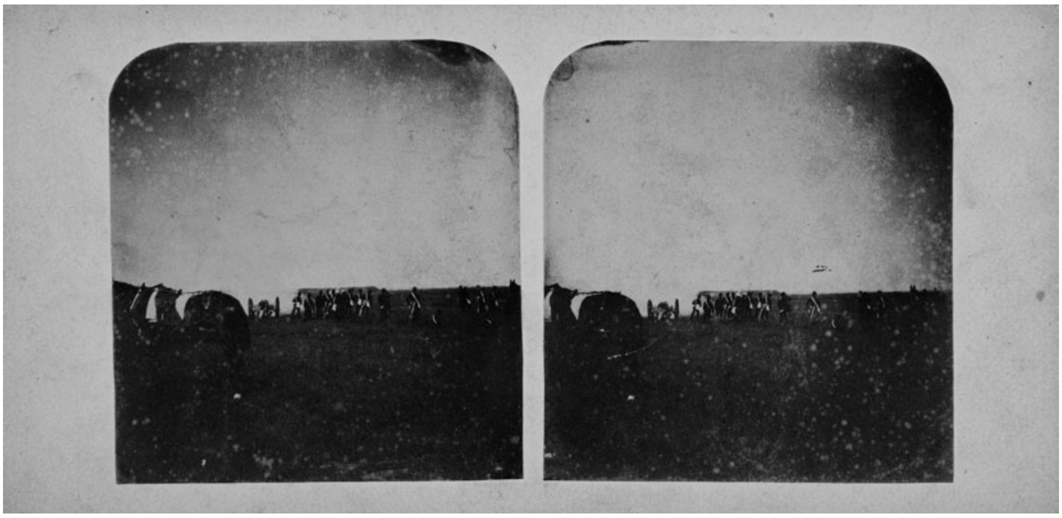

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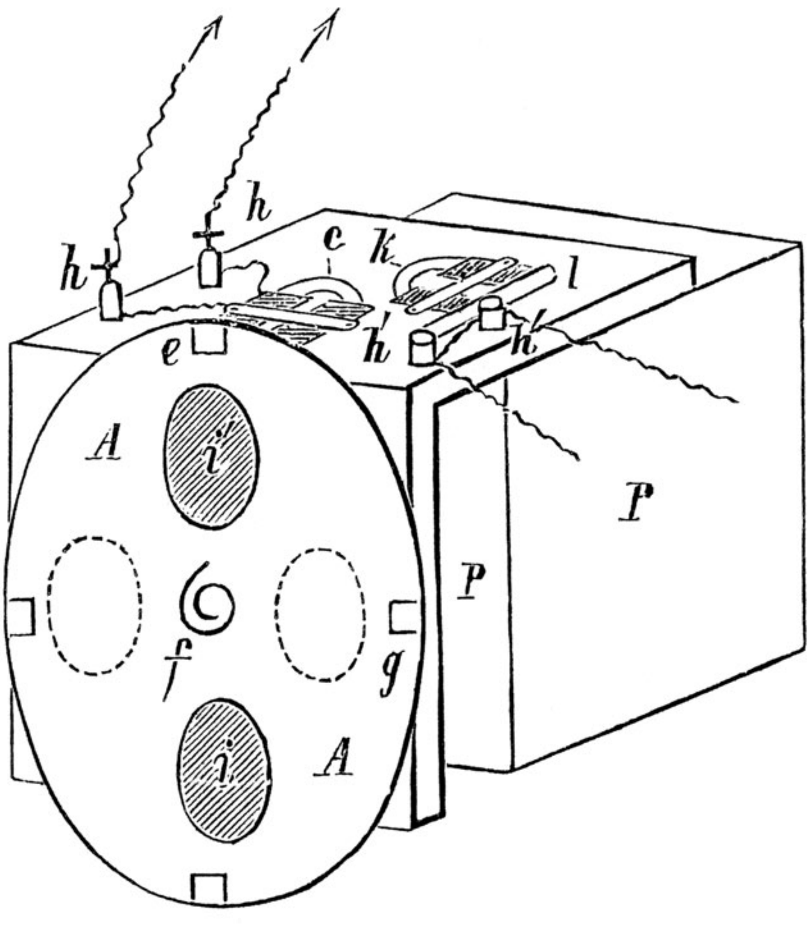

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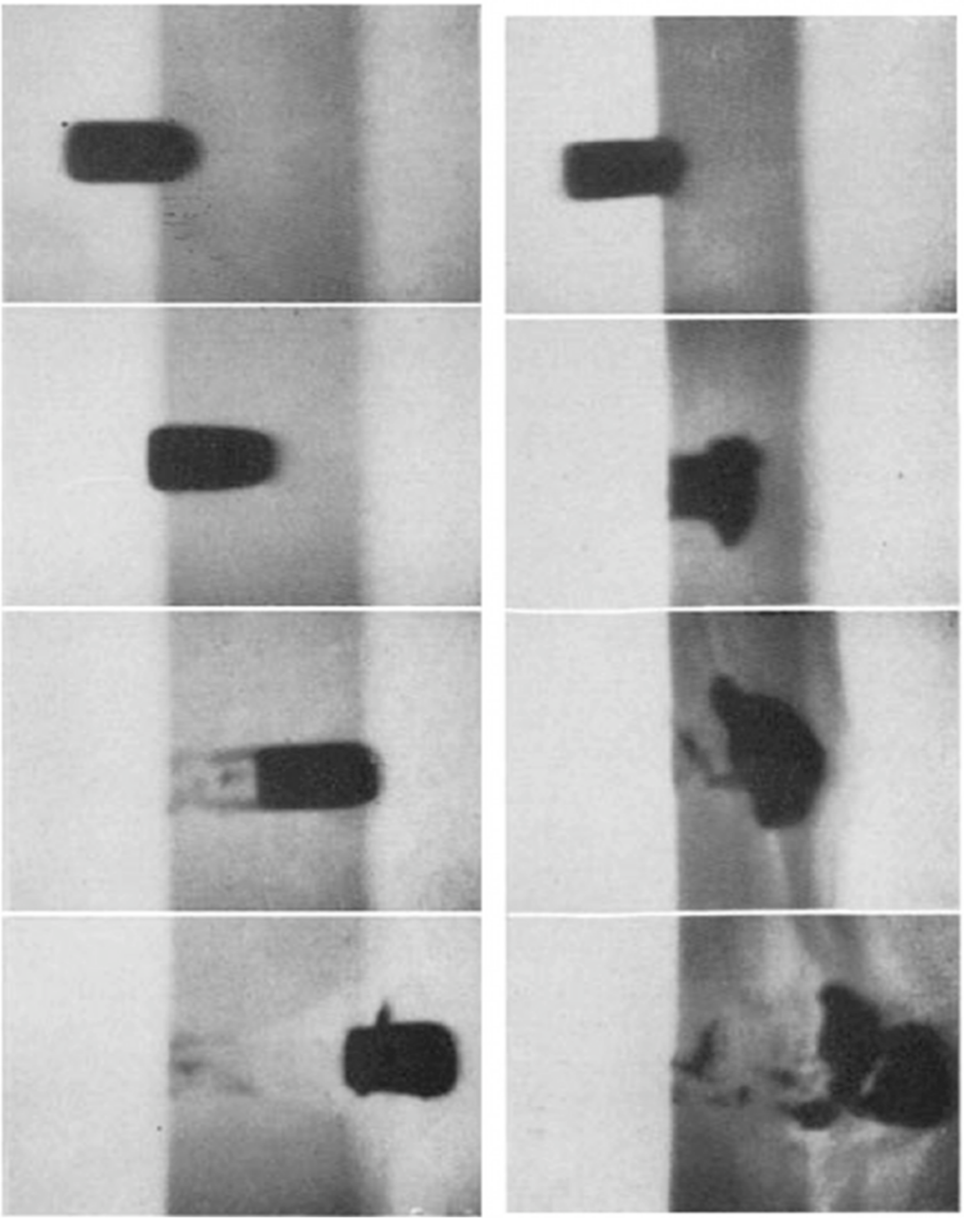

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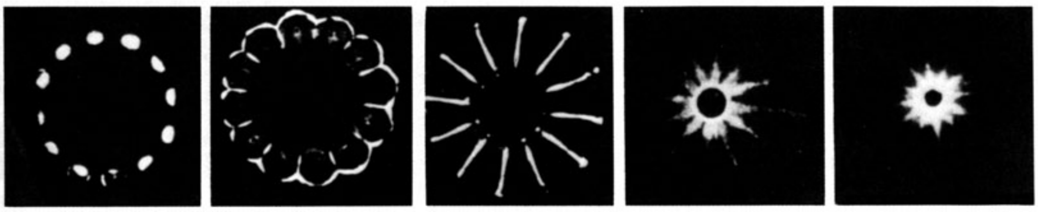

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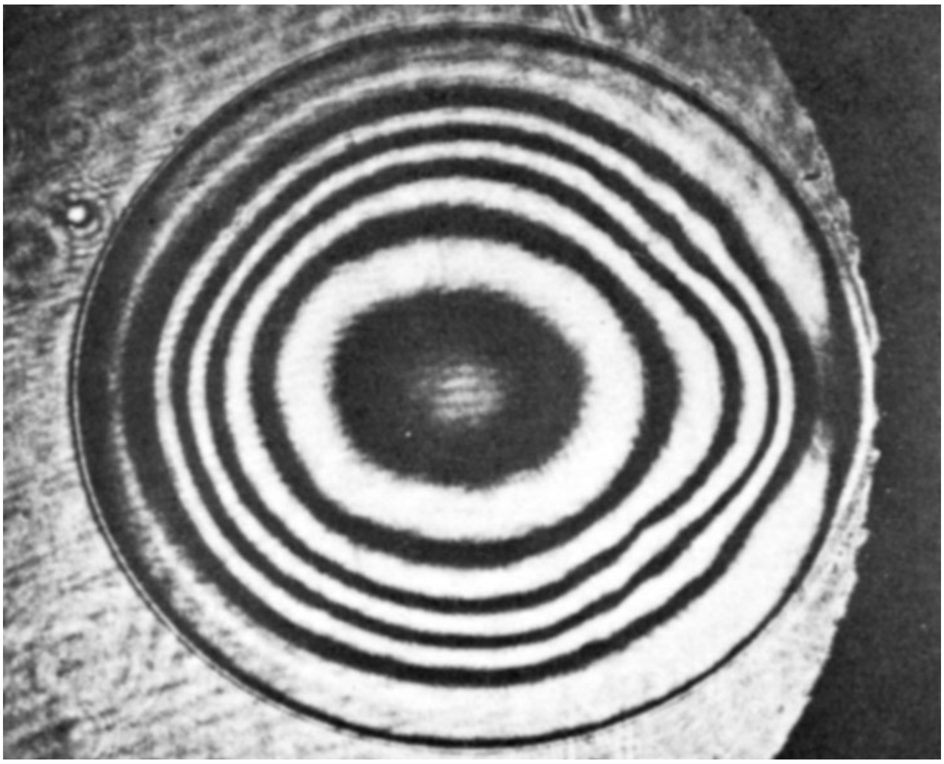

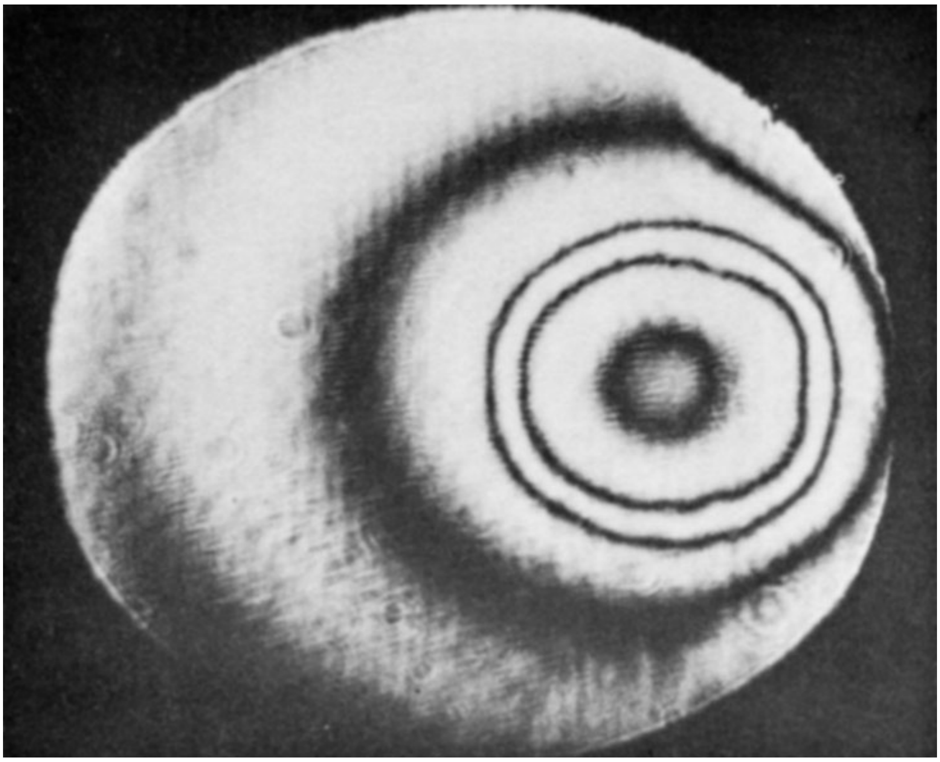

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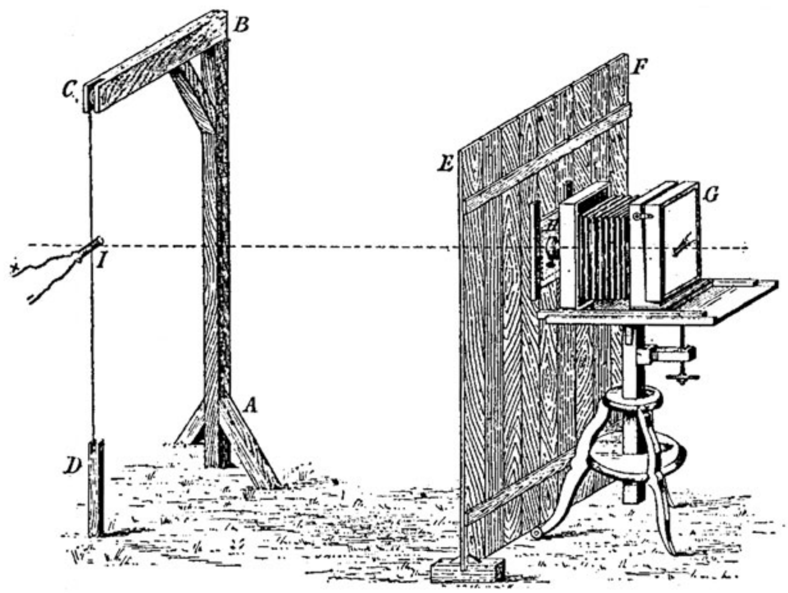

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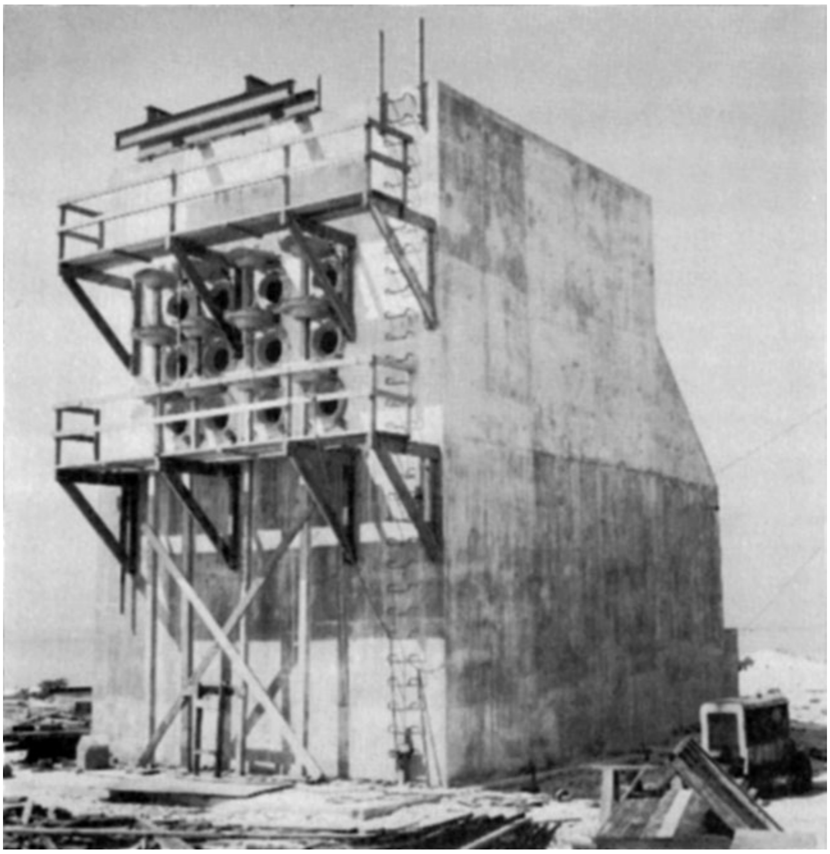

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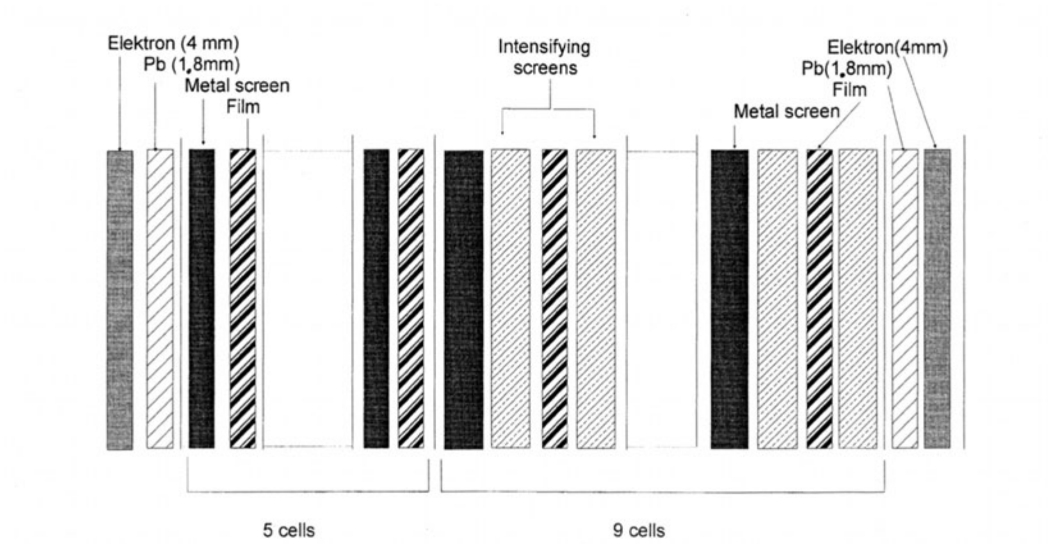
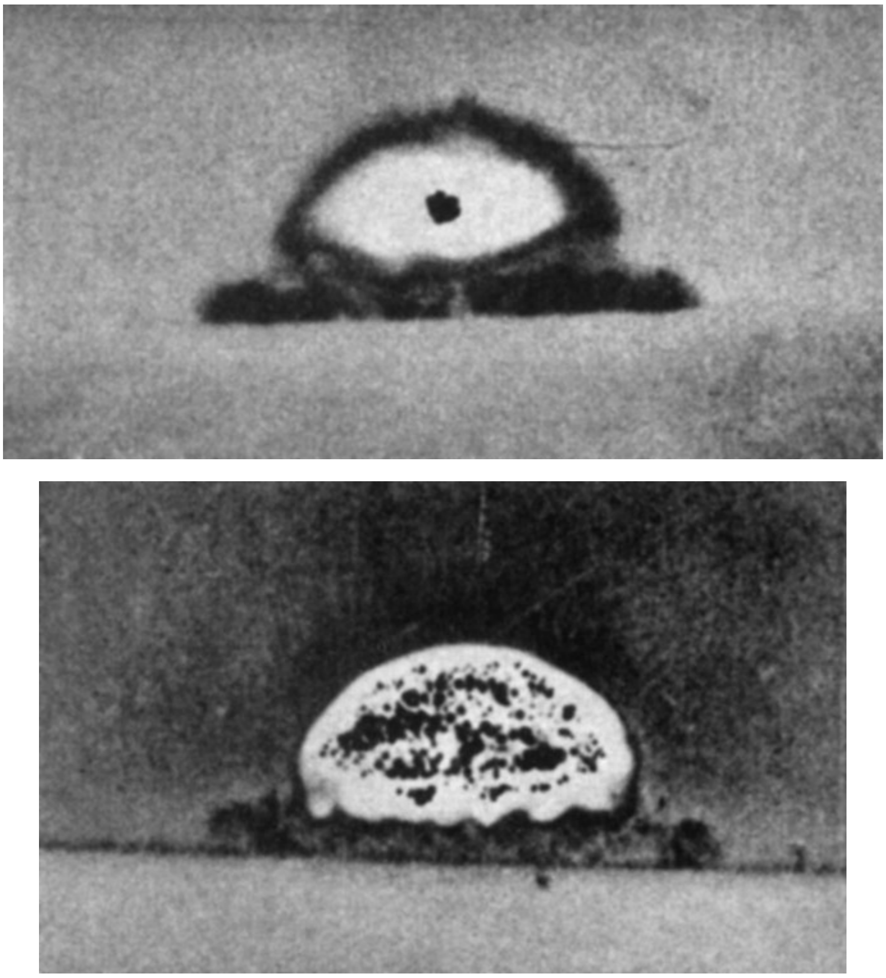


Fig. 19: Interior of cassette for X-ray film. Source: Cavailler 1997: 348.



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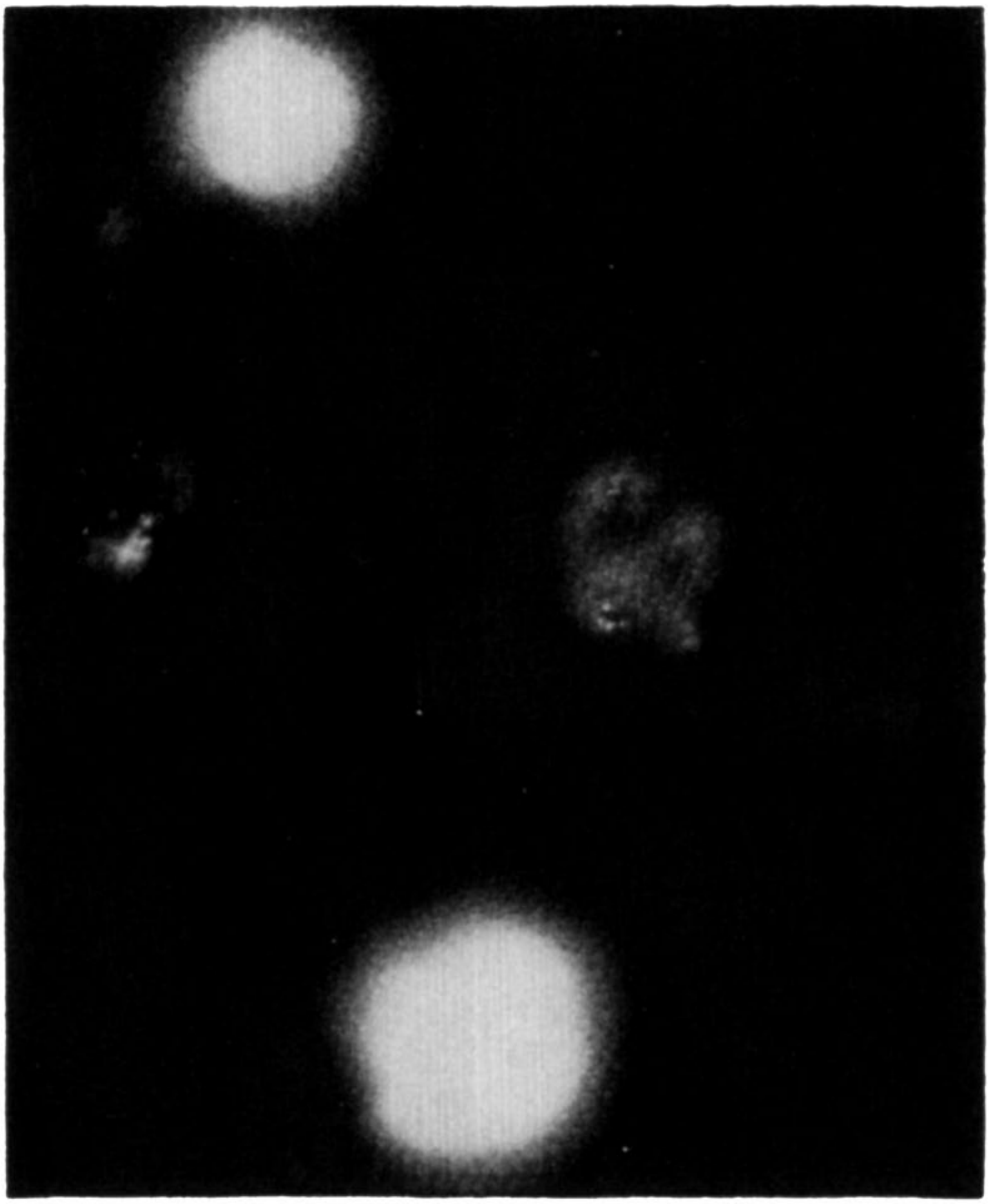
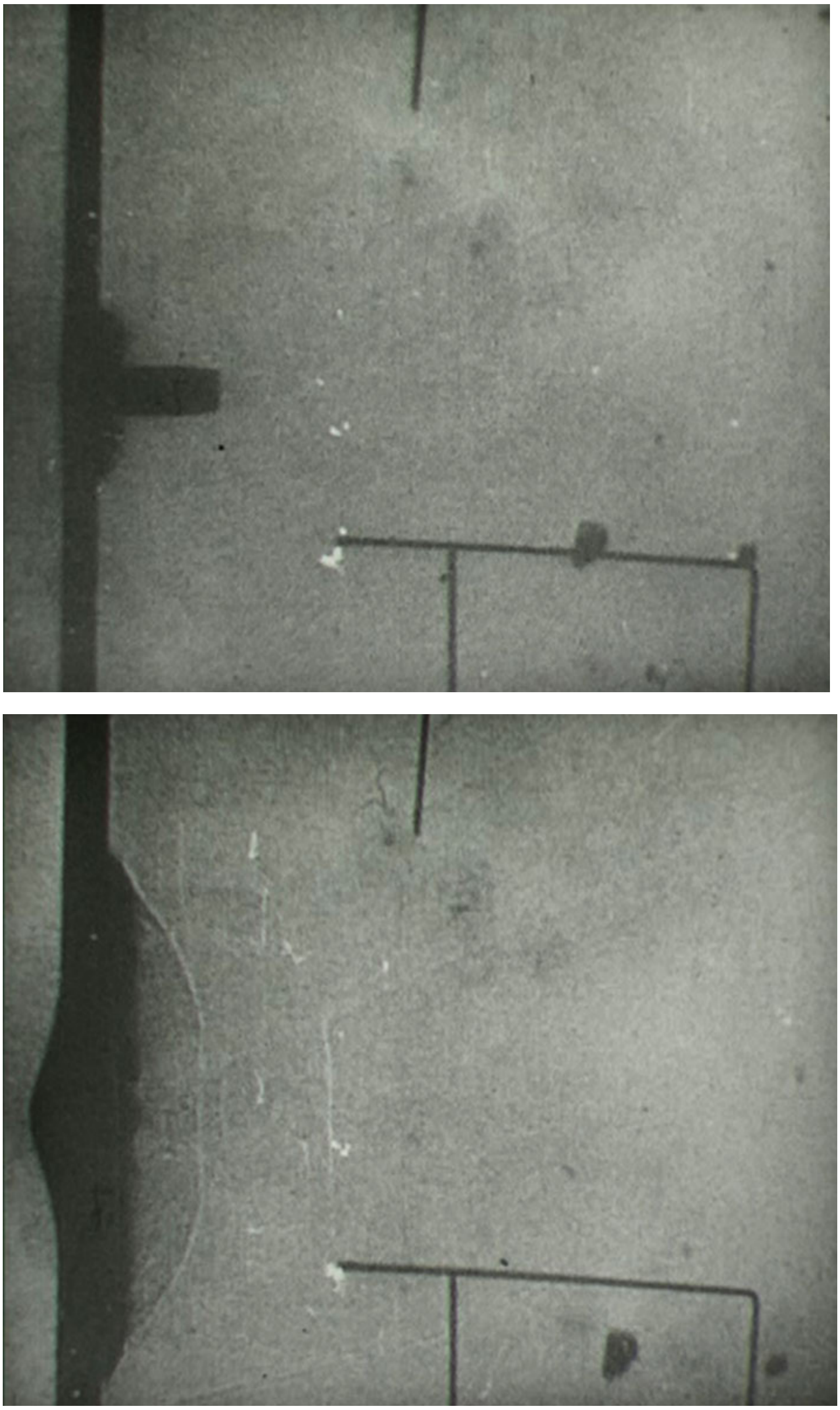


Crater's radioactivity is proved by photographic film exposed to fragments. The film is affected by radioactivity as it would be by light.

Fig. 21: Photographic plate affected by the residual radioactivity in the *Trinity* bomb crater, as published in *Life* on 24 September 1945. Source: Anonymus 1945: 30.



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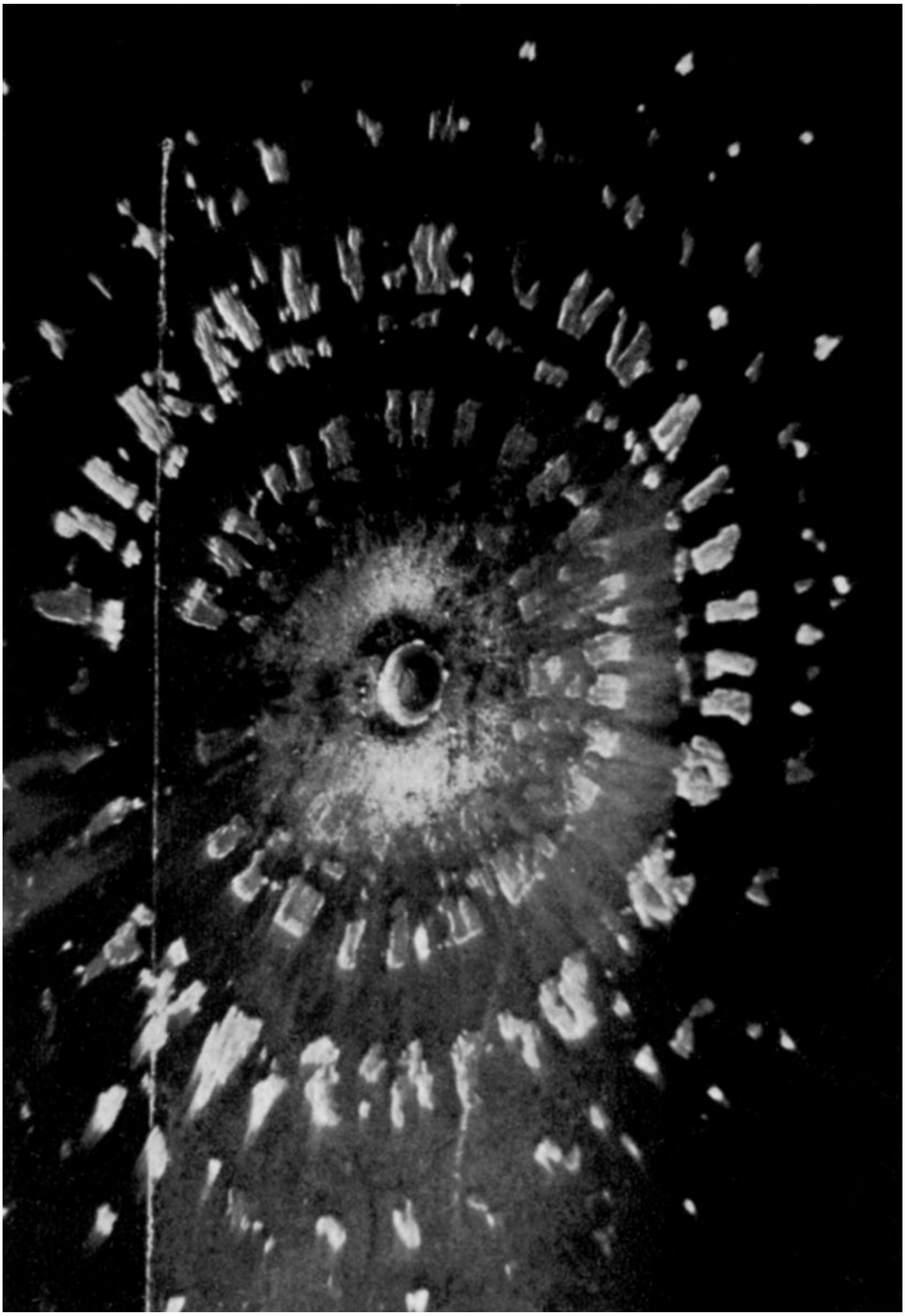


Fig. 23: Harold E. Edgerton. Photograph of the 'permanent splash' caused by a projectile's impact on a steel block. 1954. The picture belongs to the same sequence as the one reproduced in figure 5. Source: Edgerton/Killian 1954: 54.

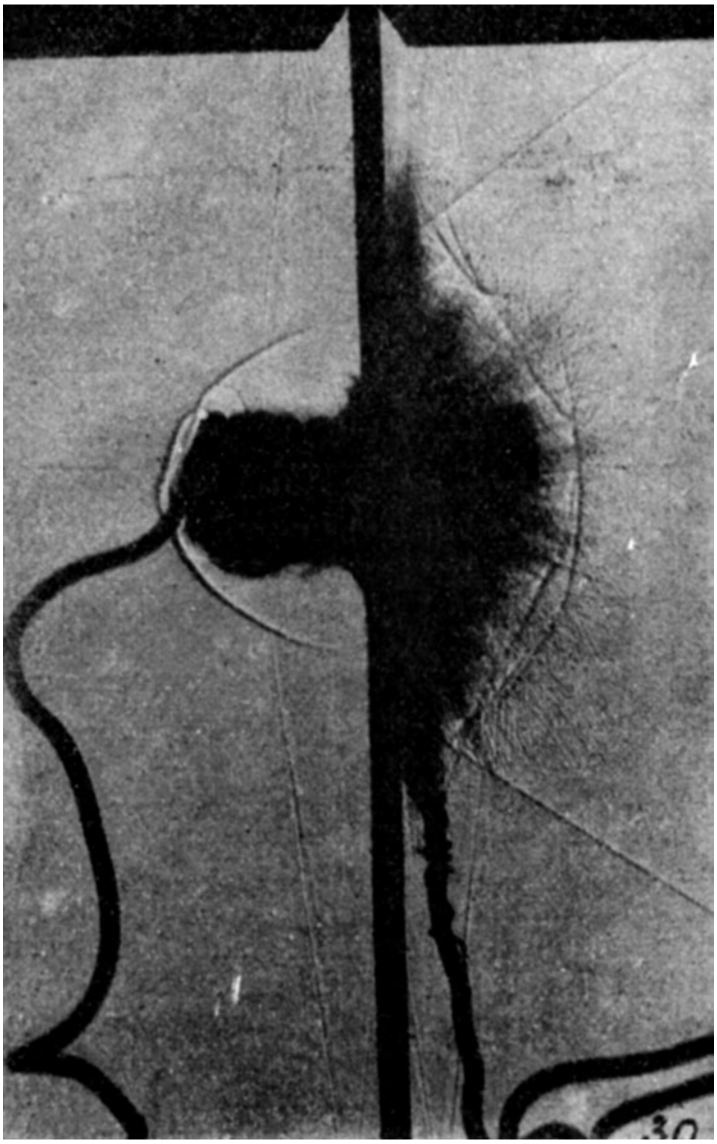
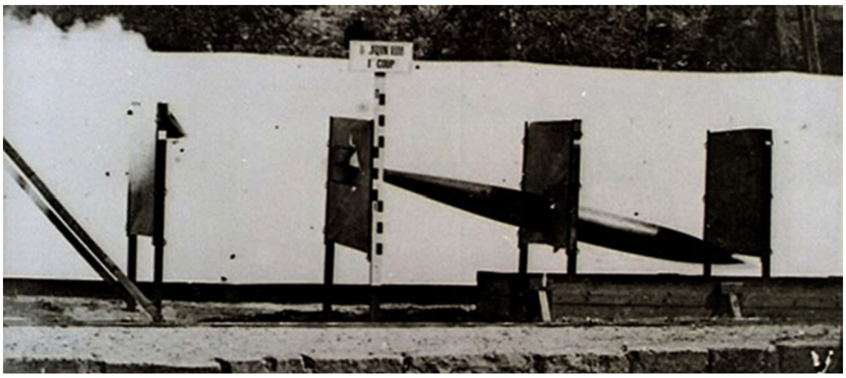


Fig. 24: Charles V. Boys. Shadowgraph of a shot through a glass plate. 1893. Source: Boys 1893: 444.



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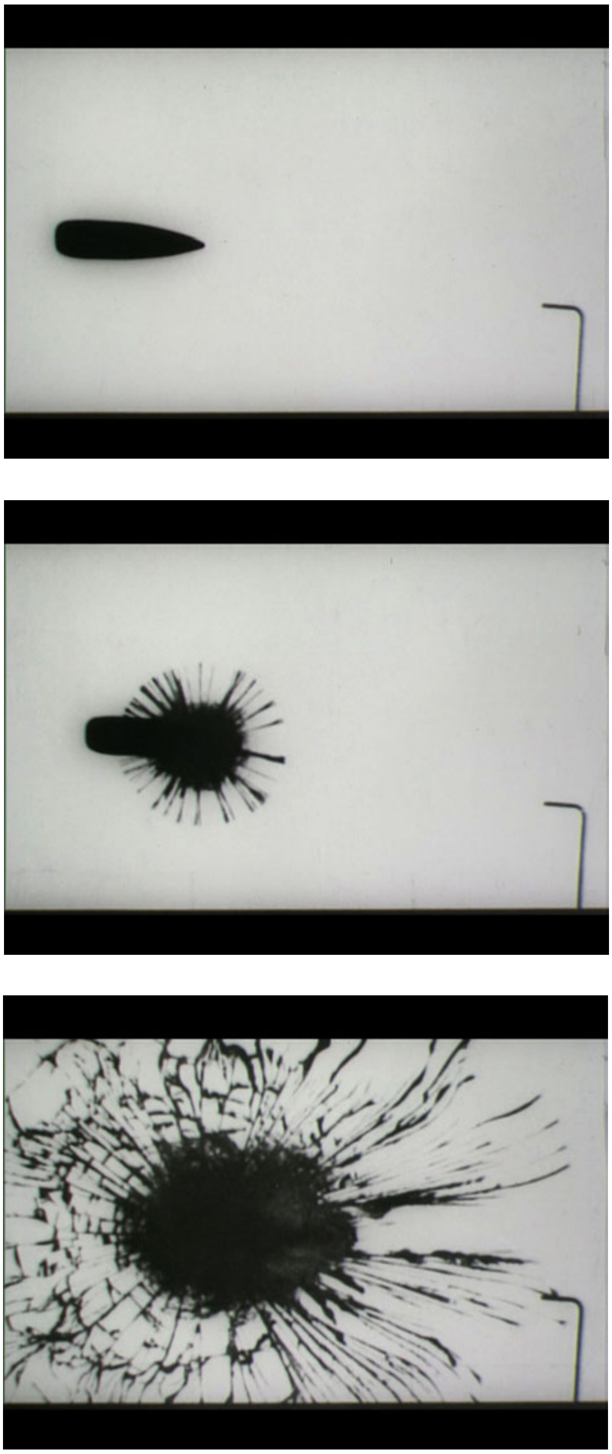

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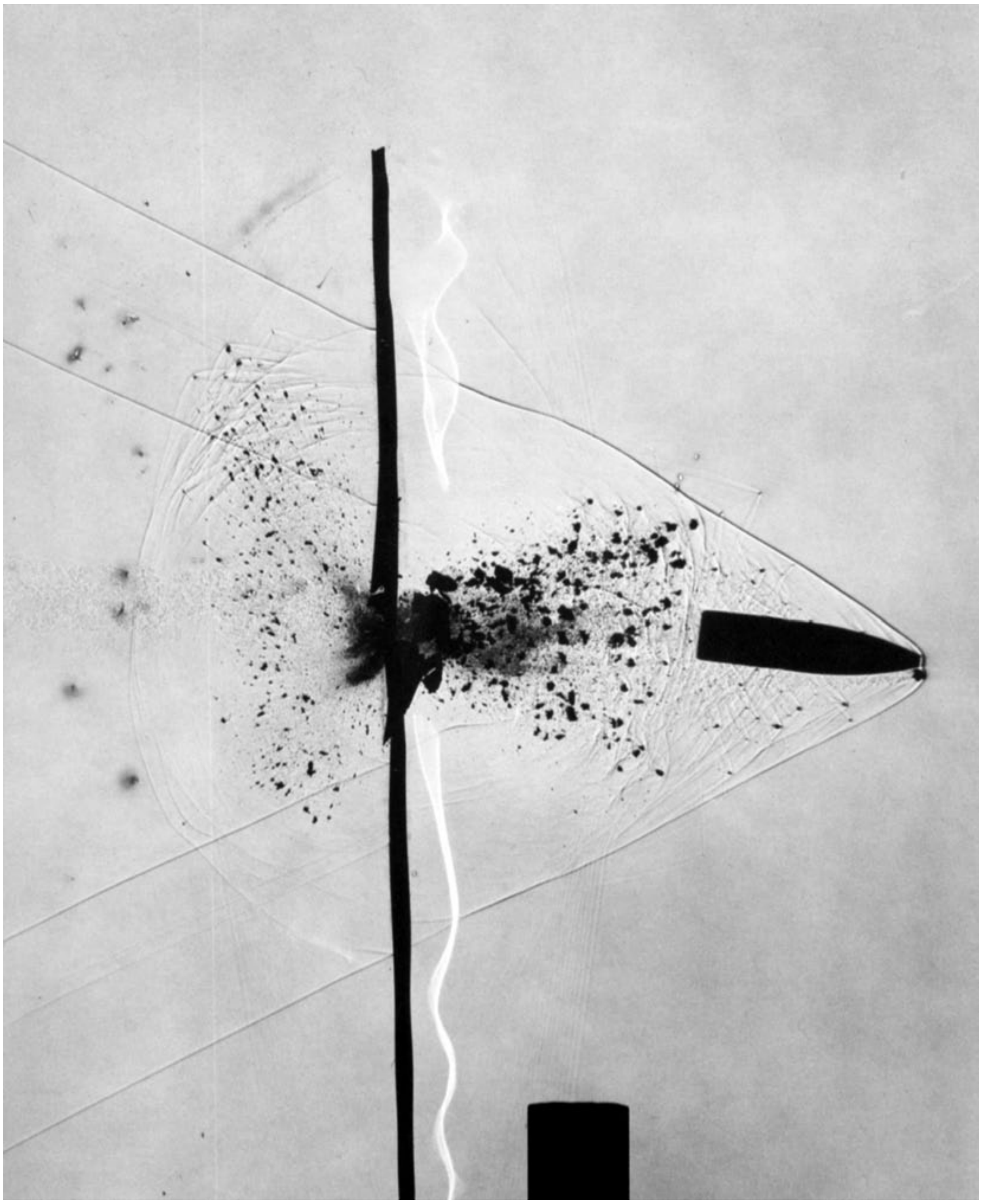

Fig. 27: Harold E. Edgerton. Photogram of a shot through a Perspex plate. 1962. Source: Edgerton 1970: 134.

Perceiving Boundaries

From Harmony to Intensity – The Cultural Life of the Membrana Basilaris

Veit Erlmann

The human auditory system is a densely packed landscape of bones, cartilage, nerve cells, and various sorts of fluid. Some of these components have been part of a rich metaphorology for centuries. For instance, in a tradition that stretches all the way from the ancient physician Galen to Friedrich Nietzsche the part of the middle ear that comprises the semi-circular canals has been linked to a labyrinth, to Theseus and to Dionysos. During the seventeenth-century the cochlea was often compared to grottos and ruins and attempts to understand inner-ear acoustics frequently drew on the Baroque obsession with such things as prosopopeia and echo.

But our organs of hearing are also lined with and partitioned into different sections by a variety of membranes. The most conspicuous and thus perhaps also metaphorically prominent of these membranes is of course the eardrum. Since its first serious scientific description and parallel gloss as *tertium comparationis* during the late Renaissance, the tympanum has been alternatively seen as emblem of resonance and adjacency or, as the symbol of philosophy's obsession with the absolute demarcation of its Other. In "Tympan," one of his early essays, Jacques Derrida invoked the eardrum to advance his critique of what he calls the "metaphysics of presence." The oval-shaped membrane, Derrida argues, not only separates the inside from the outside, it also distinguishes speech from hearing, self from other. It is, one might say, philosophy's master organ (Derrida 1982).

Somewhat less prominent than the eardrum is a trapeze shaped membrane nested deep inside the cochlea, called the membrana basilaris, the basilar membrane. Of a partly fibrous, partly gelatinous texture, the basilar membrane separates the cochlear duct or scala media from the scala tympani and supports the organ of Corti. Because of its intimacy with the osseous lamina (to which it is attached), the endolymph filling the scalae, and the hair cells of the organ of Corti, the basilar membrane for more than three centuries has occupied an interstitial position at the intersection of two, starkly opposed, narratives: a "hard," ocularcentric concept of modern selfhood that is grounded in the perceived dichotomy between eye and ear, one the one hand, and a counter-narrative that uses the ear's alleged inability to resist invasion to maintain "soft" versions of modern subjectivity, on the other hand.

In contrast to the relatively homogeneous body of cell theories that scientists and writers such as Robert Koch, Rudolf Virchow and Thomas Mann used to advance fictions of selfhood based on exclusion – and that Laura Otis in her classic study "Membranes" calls the "membrane model" of identity – theories of auditory perception from the seventeenth to the nineteenth century often served more conflicting agendas (Otis 2000). For instance, during the eighteenth century the oscillatory, resonant qualities of the membrane were crucial to sensualist concepts of perception and cognition and, hence, models of enlightened selfhood that stressed anti-aristocratic, "social" values such as responsiveness, adjacency and sympathy. At the same time, these qualities also served to buttress a more rigid set of power relationships and a type of subjectivity grounded in the politics and culture of the ancien regime.

There are two parts two my presentation: I will begin with a discussion of the first resonance theory of hearing advanced in the late seventeenth century by Claude Perrault and Joseph-Guichard Duverney and the divergent interpretations and models of subjectivity it has given rise to. In the second part I will examine an alternative, non-resonant theory of hearing that emerged during the late eighteenth century in writers such as Johann Wilhelm Ritter and culminated in the physiology of Johannes Müller.

In the late seventeenth century, within the space of only three years and riding on one of the most sweeping waves of modernization France had seen up to that point, two men revolutionized the physiology of hearing. In 1680, Claude Perrault had published *Du Bruit*, a work on the ear in which he provided rudimentary evidence that the perception of pitch was not based on arithmetic as in the ancient Pythagorean tradition in which musical intervals were determined by dividing a string according to mathematical proportions, but on subjective perception mediated by vibrations (which he called "peristaltique") of air, the tympan and the basilar membrane (Perrault 2003). Three years later, in 1683, Perrault's student Joseph-Guichard Duverney published *Traité de l'organe de l'ouïe* (Treatise of the organ of hearing) (Duverney 1683). In this short tract Duverney expanded on Perrault's hypothesis by suggesting that, to quote from the English edition of the *Traité*, the basilar membrane is

> wider at the beginning of its first revolution than at the end of its last, where it finishes in a point, and its other parts diminishing proportionately in width, we may suppose that since the wider parts may be vibrated without the others participating in that vibration they are capable only of slower undulations which consequently correspond to the low notes; and that on the contrary when its narrow parts are struck their undulations are faster and hence correspond to high notes; so that, finally, according to the different vibrations in the spiral lamina, the spirits of the nerve, which spread through its substance, receive different impressions which in the brain represent the different appearances of pitches (Duverney 1683, 96-98).

Perrault and Duverney were extremely influential as scientists and public figures. Claude Perrault was not only the brother of Charles Perrault, a close acquaintance of Louis' prime minister Colbert and a disputant in the infamous *Querelle des anciens et des modernes*, he also designed the eastern façade of the Louvre and co-founded the Académie Royale des Sciences. Duverney, for his part, was the director of the Jardin du Roi and an expert in osteology who first described osteoporosis. But the two otologists were also heavily invested in the cultural agendas of the absolutist regime, albeit from contrasting positions.

Duverney's description of the basilar membrane gained widespread acceptance, no doubt in part because it surpassed that of Perrault in anatomical detail. But it also resonated with an emerging model of subjecthood in which individuals were not subjects as much as they were exemplars of a species called "Man," natural entities whose physical and mental operations can be subsumed under the principles of Cartesian mechanism and its strict separation of body and mind. The sympathetic resonance of the basilar membrane was *the* operative mechanism aligning listeners' auditory habits, aesthetic choices and indeed sense of self with the absolutist order.

But there are subtle differences in the way this resonance of power and personal identity was figured in Perrault's and Duverney's work. Although it appears that Duverney himself abstained from philosophical reflection, the biomechanics of the basilar membrane he had described would prove crucial several decades later to Jean-Philippe Rameau's attempt at linking the physics of sound and the physiology of hearing to musical aesthetics and, implicitly, a Cartesian metaphysics of the subject. As such his theory, expounded with great persuasion in his *Génération harmonique* (1737), was due in no small part to his discovery of the work of Joseph Sauveur (1653–1716) on harmonic partials published in 1701 and to his collaboration with the scientist Jean-Jacques Dortous de Mairan (1678–1771). Rameau was particularly drawn to Mairan's concept of the *corps sonore*, or sounding body that transmits its vibrations to the perceiving ear, and to the concomitant idea that the ear perceives pitch on the basis of the sympathetic vibrations of the basilar membrane discovered by Duverney. Harmony, Rameau confidently begins his treatise, "is a natural effect, the cause of which resides in the air agitated by the percussion of each individual *corps sonore*." (Rameau 1968, 15)

The crux of Duverney's theory is of course the fact that he assumed the a priori integrity of the entire relationship between perceiving subject and the sounding object. For Perrault that integrity was very much in doubt. In a bold attempt at revising the Cartesian mind-body dualism – an attempt that became known as "animism" and was staged in a book on sound no less – Perrault argued that the animal body was ensouled throughout. There was, in other words, no clear separation between what he, anticipating Leibniz's famous "petites perceptions," called "confused perceptions" and rational judgment. Both occur at the same time in the sentient organ, whether this be the skin of a decapitated snake that returns to its favorite hiding spot to die or the ears of a human being that deem an interval to be harmonious when mathematical reason would clearly recognize it as a dissonance, as in the case of a major third.

Perrault's animism and ensuing aesthetic relativism raised numerous epistemological issues whose resolution required him to delve deep into the theory of architecture and the politics of opera. But despite its shortcomings, it is clear that the concept of the "animated" basilar membrane could not have emerged in a vacuum. It was the product of a potentially explosive political climate in which audiences began to assert the legitimacy of pleasure over deference to the rational norms and heroic images of absolute power embodied by the king.

By the middle of the eighteenth century resonance as the dominant aspect of the bio-mechanics of the basilar membrane was beginning to be called into doubt. In fact, bio-mechanics as a whole fell into disrepute. Apart from the shift toward vitalism and *Naturphilosophie* sweeping the life sciences more broadly, there are several factors that prompted otologists to explore alternatives to Cartesian style resonance theories and to rethink the physiology of the basilar membrane along different lines. One of these factors was Ernst Florens Chladni's discovery of the so-called "sound figures." Another factor were the "galvanic" experiments a motley cast of characters such as the physicist (and inventor of the dry cell battery) Johann Wilhelm Ritter, the electro-chemist Christian Oersted, and the associationist psychologist Jan Purkinje conducted on their own bodies and that impressed on them the notion that auditory perception was a matter of what Purkinje called "organic subjectivity." In other words, the sensation of sound was possible even in the absence of an outside acoustic stimulus or a one-to-one correspondence to such a stimulus. And, finally, there was Domenico Cotugno's discovery in 1780 that the scalae of the cochlea were not filled with air (the air implantatus of old) but with a watery substance, the endolymph.

While important in their own right, these findings and experiments were shot through with epistemic ambiguity, most notably an eclectic blend of aesthetic and organicist concepts such as intensity and *Kraft*, and Romantic theories of subject formation derived from Kantian transcendental aesthetics. The person, however, who synthesized this heady mix into a theory of auditory perception of far-reaching consequence was Johannes Müller. As is well known, Müller maintained that "individual nerves of the senses" have "a special sensibility to certain impressions by which they are supposed to be rendered conductors of certain qualities of bodies, and not of others." (Müller 1826, xxiv) Or, more floridly, the "nervous system here illuminates itself, there sounds itself, here feels itself, there again smells and tastes itself." (50) There are several consequences of this "law of sensory energy," as Müller's neurology became known, for the physiology of the basilar membrane. Because the cochlear nerve fibers are concentrated in the smallest of spaces while at the same time being spread over a "considerable" surface, the fibers receive the waves pulses of the endolymph "nearly simultaneously" thus ensuring a maximum degree of compression and rarefaction of the waves. The miniaturizing role of the membrane further combines with the fact that in air-water transfers movements retain a maximum of their intensity even when such movements are transmitted by a membrane such as the membrane of the round window. Inside the cochlea, Müller further claimed, the intensity is maintained because the spaces between the fibers, already infinitesimal because of the coiled shape of their terminus on the basilar membrane, are further reduced because the fibers of the cochlear nerve share with the endolymph the same overall liquid composition.

The concept of intensity, in combination with Müller's focus on the surface qualities of the basilar membrane, is of particular interest. Intensity, during the early nineteenth century, was a relatively novel category. It had emerged in conjunction with the shift in biology, chemistry and philosophy toward process, flow, energy, and gradation and as such it corresponded with dynamic models of sensation that stressed the discontinuities between our sensations and the endlessly differentiated object world rather than the need for differentiations itself. In addition, during the late eighteenth and early nineteenth-centuries Cartesian understandings of subjectivity as the acquisition of knowledge through our given ability (or obligation) to represent were eclipsed by an alternative, aesthetic concept in which our status as subjects is achieved by the constant application of what *Aesthetica* author Baumgarten, for instance, called Kraft (*vis*). The primary relationship a subject has to him or herself is not one of knowledge (as in Descartes' certainty of one's existence), but one of power. Subjectivity is the power to make oneself through aesthetic practice; an endless, self-perpetuating process without purpose or norm.

Contrary to the prevailing image of Müller as a theorist of vision and in contrast to the tendency to assume a paradigmatic rift between Müller's early, "romantic" work and a subsequent, scientific phase, there is ample evidence that Müller's concept of non-resonant cochlear function was shaped by this convergence of empirical research and a metaphysics of the subject precariously hovering between self-loss on the one hand and the stability of culture, especially music on the other hand. Just as vision for Purkinje consisted in a "forward displacement of the entire visual field of the retina," (Purkinje, vol. 5, 51) hearing entailed placing oneself into an auditory space filled with "after sounds." (Purkinje, vol. 2, 194) More than a century before Freud famously declared "psyche is extended, knows nothing of it," Müller had defined listening as the unconscious projection of the ear into a virtually unlimited space.

The subject-object relationship that ensues from this auditory projection is of course hardly that between a forever unknowable world and Kant's immutable transcendental I. Rather it may be described, using a phrase from Carl Schmitt, as a form of "occasionalism." (Schmitt 1986) The romantic subject, Schmitt had argued, far from being a mere plaything of all kinds of ersatz religions, was a dangerous subject because it reduced the world to a mere occasio, a stimulus for its imaginative powers. Its relationship to the world was thus determined by purely aesthetic, formal criteria and not, as the arch-conservative Schmitt had hoped, by criteria such as acquiescence to power. The romantic subject was, in that sense, a useless subject, adrift in the fantasies of its own singularity and demiurgic potency.

It would be a mistake, however, to stop here and limit our interpretation of Müller's gloss of the basilar membrane as an intensity-enhancing device to only the self-generating subject. Here, as in Perrault's and Duverney's resonance models of the basilar membrane, a tension exists between the progressive, "occasionalist" aspects of Müller's physiology and a rather more conservative agenda based on the view that, to quote Laura Otis again, "the Prussian government [was] a reasonable bringer of order." (Otis 2007, 35)

On several occasions Müller invokes a bizarre mix of Spinoza's ethics and romantic musical aesthetics to argue for listening as a stabilizing factor. In the final paragraph of *Zur vergleichenden Physiologie des Gesichtssinnes* (1826), he writes:

> The passions relate to each other through affinity [*Verwandtschaft*] or animosity [*Feindschaft*]. The consonances and dissonances of tones stand in a similar relationship. It is thus that out of the sensory energies there emerges the symbolism of all of our changing moods and most secret doings inside our passionate being. Music…symbolizes the movement of our soul due to the combination and sequence of harmonic energies of the sense of hearing. It is unique in this respect among the arts, because every other art represents not so much the most subtle stirrings, relationships, transitions of the emotions, than the result, the outcome of this process, as it may be grasped in words and signs. Music would ignore its vocation if it depicted the actions resulting from the passions and the free will. There is also a kind of music that does not so much aim at accompanying the movements of the soul through the sensory energies as to make us aware, in a playful way, of the entire spectrum of the ear's sensuousness in tones. Yet this is only the first, youthful consciousness of our sensuousness in art [*das erste jugendliche Bewußtsein unserer Sinnlichkeit in der Kunst*]. From here, art branches out into two directions: as symbolism of the waxing and waning in nature and of the creative and destructive natural forces in *instrumental music*; as symbolism of the transitions in the movements of the soul in *vocal music* (461-62).

There are several familiar motifs here, motifs Müller clearly adopted from the aesthetics of "absolute" music, such as music's antimimetic, "organic" nature or music's unique status among the arts. The most striking aspect of Müller's music theory, however, is a reference prior to the passage quoted to Baruch Spinoza. The seventeenth-century philosopher figures prominently in several of Müller's works, including the *Handbuch*, and was an important factor in Müller's early project of fusing physiology and philosophy. Müller's work was split between two conflicting demands: that of integrating reason into an equilibrium of forces, on the one hand, and the desire to foreground the scientist as a genius commanding an extraordinary degree of inventiveness and imagination, on the other. Although Müller failed to resolve the tensions between these two demands, he concurred with Spinoza in positing that ethical behavior could result only from an equilibrium or, as he called it, from "life's harmonious movement" (*harmonische Lebensbewegung*) (Müller 1826, 88).

Within this broader ethical framework Müller assigns a special role to musical listening. On a superficial reading, the reference to Spinoza and the analogy between music and the passions would appear to be a crossover between seventeenth-century *doctrine of affects* and eighteenth-century *Gefühlsästhetik*. Yet Müller immediately undermines this impression by shifting the focus from music as mimetic representation to the organ of hearing and its "harmonic energies." Energies of the ear, *bien entendu*, not musical energies. Harmony, in Müller's view, cannot be derived mechanically from the series of overtones, but must be understood as ensuing from what he calls "fantastic auditory sensation" (*phantastische Gehörempfindung*). "The cause of musical harmony," he writes, "resides physiologically in the organ [of hearing], just as the cause of color harmony lies in the eye." (Müller 1826, 458)

If, then, music's ability to influence the passions is limited, how does the ear achieve its ethical mission of reining in the passions when its natural predisposition toward the "fantastic" appears to suggest otherwise? How can listening become the basis for ethics? Here the companion essay to *Zur vergleichenden Physiologie des Gesichtssinnes*, *Ueber die phantastischen Gesichtserscheinungen* (1967) offers some important clues. In this work, Müller had famously devoted the last chapter to a comparison between the imagination as deployed by the poet and the imagination as the basis for the scientist's work. Both, he said, work along similar lines. The "fantasy" of the artist and the "intuitive perception of the natural scientist" (*anschauender Sinn des Naturforschers*) reproduce, each in its own way, the metamorphosis of forms pertaining to all living beings.

Music shares this Goethean logic, keeping imagination and reason in balance. From youthful, sensuous beginnings, it "branches out" like a plant, naturally progressing toward ever higher levels of maturity and perfection. In this way, music history's upward thrust mimics as it also becomes a prime vehicle for the broader Romantic project of replacing the finite divine cosmos with a world forever in the making. But music, more importantly perhaps, is also irreducibly bound up with language. It is in the physiology of the inner ear, where the waves of the labyrinthine water, "intensified" by the basilar membrane, meet the auditory nerve, that Müller reanchors the subject in what he calls "symbolism." And so, contrary to the assertion that music's unique position among the arts is due to its anitmimetic qualities, it is precisely the denial of "absolute" music's freedom from representation that allows Müller to mobilize the ears for the broader ethical project of harnessing the passions in "vital harmonious movement."

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'B Moves Farther than it Should Have Done'
Perceived Boundaries in Albert Michotte's Experimental Phenomenology of Perception

Sigrid Leyssen

Perception research has often been occupied with finding boundaries. Determining threshold values of perceivable differences (*the boundaries of the perceivable*) was a main occupancy in 19th-century psychophysics. Reaction time experiments, which ruled 19th-century physiology and early experimental psychology, measured the time needed for stimuli to be perceived by differentiating it from the time needed to think and to react (*the boundaries of the perception process*). Time and again, also the *boundaries of the category of perception*, enclosed by sensations on the one side and judgements on the other were debated, i.e. the borders of the psychical process of perception in between mechanical, physiological sensory processes and higher cognitive processes. These boundaries shifted sometimes in one or both directions, making perception into a larger or smaller category. In this paper, I turn to the experimental psychologist Albert Michotte (1881-1965) and his main work on the perception of causality, which he pursued from the late 1930s through the early 1960s. In his 'experimental phenomenology of perception', Michotte was not only interested in the *boundaries of* certain kinds of *perceptions* – i.e. determining the specific stimulus conditions in which a certain impression appears or disappears – but especially in *the perception of boundaries.*

Albert Michotte was a second generation experimental psychologist. Having studied briefly in Leipzig in 1905 and 1906 under Wilhelm Wundt (1832-1920), acknowledged as one of the founders of experimental psychology as a scientific discipline, and with Wundt's student Oswald Külpe (1962-1915) in Würzburg in 1907 and 1908, Michotte went on to make the laboratory for experimental psychology in Leuven, Belgium into an internationally recognised centre for scientific psychology. The Louvain laboratory had been established in 1894 at the *Institut Supérieur de Philosophie*, at the *Université catholique de Louvain*. The *Institut Supérieur de Philosophie*, where Michotte got his education, was itself a new institute, started in 1889 in the spirit of the encyclical *Aeterni Patris*, with the aim of reconciling catholic philosophy and the sciences through a neothomist approach. In the vision of its founder, Désiré Mercier (1851-1926), the new scientific psychology had an important role to play in this undertaking, and he made sure that one of the better equipped laboratories of the time[1], built after Wundt's model, came to be erected at the centre of the institute.[2] It was in this context that Michotte worked on different topics: the distribution of tactile sensitivity, logical memory, and voluntary choice at first; then, between the wars, motor reactions and different topics on perception, such as constancy phenomena and 'sensory synthesis' in the tactile-kinaesthetic domain.[3] It was in the 1930s that his approach of an 'experimental phenomenology of perception'[4] began to take shape, which will be discussed in this

1 'A ce cours est annexé un laboratoire, ce qui constitue un enseignement complet de psycho-physiologie normale qui, à l'heure actuelle, n'existe pas encore en France.' Quote from the French psychologist Alfred Binet, complimenting the Louvain laboratory, in August Peltzer (1904) *L'Institut Supérieur de Philosophie à l'Université de Louvain (1890-1904).* Louvain. Quoted from Brock, 2010, p. 73.

2 On the history of the Institute, see e.g. De Raeymaeker (1952), Struyker Boudier (1989).

3 For a short overview of Michotte's professional career and his research topics, see his 'Autobiography' (1991b)[1954]. A full bibliography of Michotte's work can be found in Michotte et al. (1991).

4 On Michotte's experimental phenomenology, and experimental phenomenology more generally as an

paper.[5] This approach can be considered as his hard-won conclusions on what a scientific method for psychology could be. It was his line of critique on the first generation of masters of the discipline, a critique in which he had much affinity to gestalt psychology and adopted some of the behaviourist critiques. It was moreover an approach which had grown out of his interaction with many strands of psychology, and in which a combination of different aspects of them could still be found.[6]

It had been one of gestalt psychology's exemplary insights that boundaries are not pre-given in our perception. The parts and elements that we see, and therefore also all the boundaries between them are newly created with each act of perception, according to the same laws, as to which the impression of the whole is created.[7] Where, when and how we see boundaries depends on the perceptual organization of all that is given to perception.

Previously, in perception studies, elements and boundaries had been thought of in much more fixed terms. As long as there was considered to exist a one-to-one correspondence between sensations and stimuli, any perceived element – and thus any perceived boundary – had to be brought back to its corresponding local physical stimulus. This close correspondence of stimuli and sensations, and the sensory atomism that went with it had been under attack for decades when the gestalt psychologists set upon it, but it had still been lingering at least as a necessary hypothesis (in different versions e.g. Hermann von Helmholtz, Wilhelm Wundt, Carl Stumpf): even though such elemental sensations were never given to us as pure sensations, but always already as compounded in more complex perceptions, they were considered to be the actual building blocks of our perceptions. Every perception then had to be understood by recovering these elemental sensations and their corresponding stimuli.

In the new gestalt approach, physical stimuli no longer had to determine one per one the sensory elements. As a complex of stimuli they could now be taken to determine the perceived complex as a whole. The elements (and their boundaries) were no longer considered to be the actually given and physically determined building blocks of our perceptions.[8] Michotte's work is taken here as a case study of how, once boundaries were no longer pre-given to perception but created in every perceptual act, perceived boundaries could be found where none were previously expected, and where no local stimulus had indicated them. I will argue how they not only became an interesting research topic in themselves, but also received an interesting methodological function as working tools for building a theory.

'Perceived boundary' is itself not a term which was used by Michotte. I have introduced it to indicate an important content of the observer reports with which he worked, and which I consider as playing a central role throughout his investigations. In what follows, I explore the different perceived boundaries Albert Michotte distinguished in his work on the perception of causality, his method of finding them, and some of the methodological functions perceived boundaries came to perform for his theory of causal impressions. In a last section, I discuss how this method

approach in psychology, see Thinès (1988), Bozzi (1999), van Hezewijk and Stam (2008).

5 This research was first presented in an article, Michotte (1941) 'La causalité physique est-elle une donnée phénoménale?' His book came out after the war, Michotte (1946) *La Perception de la Causalité.*

6 Albert Michotte was an international figure, situated in between German, French, and Anglo-Saxon psychology. He hardly ever missed one of the international congresses of Psychology, and was in contact with many international befriended colleagues, as becomes clear from his extended correspondence. He discussed evolutions in the discipline in different countries with many of them, and several of his different topics of research reflect explorations in those different directions.

7 Metzger, 1975, p. 114.

8 On gestalt psychology and how it demarcated itself from its predecessors, see Ash (1995), Harrington (1996, Chapter 4), Smith (1988).

of working *with* perceived boundaries was also used to understand more straightforward cases of the perception *of* boundaries.

The problem of the perception of causality

Albert Michotte was interested in the perception of dynamic, moving events. He came to similar conclusions and enthusiastically shared the gestalt approach, which enabled the study of those more complex phenomena which had previously been excluded from experimental study. In his eyes, however, gestalt psychologists had still kept too much to reinterpreting known perceptual problems, such as studying the perception of shape and movement. They had not yet explored what he saw as the new possibilities this approach allowed, i.e. studying the perception of dynamic events of interacting objects.[9] Michotte emphasized the study of 'functional relations' (PC4) between objects during such events, and he was convinced that these were most important for our knowledge of and behaviour in the world. Whereas many investigations on the perception of space, form and movement had been performed, the perception of dynamic, functional relations was hardly researched. Michotte asked: 'do we not see wine *coming out* of the bottle, *running into* the glass? This is something quite different from a simple change of position in place' (PC4).

Michotte went some way in agreeing with the behaviourist view that psychology should be a study of behaviour or action, yet for him this also included 'studying how people and animals "understand" the situation in which they are placed and the actions of people and animals, as well as those they perform themselves' (PC34). Michotte wanted to study perception as one phase of action. Perception for him was not just about detecting objects of a certain form or colour, but also about *meaning*. An object affects behaviour only when it has meaning, Michotte stated, and this meaning arises in the first place from relations between objects: spatial and temporal relations, but also functional relations, such as causal relations, or relations of finality. These relations, he would argue, are all *given* in perception.

Michotte's first and main case of study in his 'experimental phenomenology of perception' was that of the causal impression[10] of seeing one object causing the movement of another. In order to study this causal impression, Michotte designed an experimental setup that enabled him to show moving and colliding coloured figures to his subjects. With these displays he was referring to the example of colliding billiard balls, which had been the standard example in discussions on causality for a long time. One of the standard examples he showed was that of an object A moving towards an object B, stopping when it reached B, at which point B starts to move. Observers described that they saw A bumping into B and sending B off or launching B (Figure 1). Michotte recognised this impression, which he called the *launching effect*, as one of the main types of perceived mechanical causality.

[9] On Michotte's relation with and his demarcation from Gestalt Psychology, see his 'Autobiography' (1991), p. 34ff; and Michotte (1963) *The Perception of Causality*, p. 5-6. Hereafter, I will refer to this latter work in text as PC5-6.

[10] Michotte noted how he preferred the term 'causal impression' over 'perception of causality', which had been used for the title of the book, because the former 'brings out more clearly the idea of an immediate datum, of something directly "lived"', PC15, note 20. Causal impression will become an umbrella term for a range of different impressions described in the book which share that they are described in causal terms and, as will become clear in the end, are all characterised by a similar perceptual structure. Note that these are all cases of *mechanical* causality (e.g. one object causing the movement of another object); the experiments showed that there was no causal impression found for cases of *qualitative* causality (the linking together of purely qualitative events, e.g. changes of colour), see PC 25-26, 229ff.

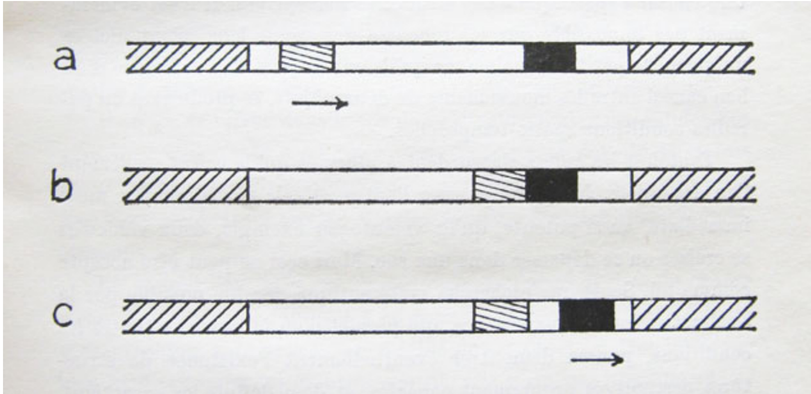

Fig. 1: Schematic representation of the launching effect. In: A. Michotte, with G. Thinès (1963) ‹La causalité perceptive›, in *Journal de Psychologie normale et pathologique, 60,* p. 13.

Michotte wanted to find out whether the use of causal terms when describing the events shown in the displays was justified by what is given at the perceptual level, and if so, which perceptual factors would be responsible. Why is one combination of moving objects described as the succession of two separate events, for example, as A approaching B, and then, when A has reached B, B withdrawing? And how can it be explained that another combination, objectively only slightly different, is described as one complex event, described in causal terms: as A giving B a push, as A setting B in motion, as A launching B?

Michotte never explicitly formulated this problem of the causal impression as a boundary phenomenon, as in the case of other impressions he studied, which will be referred to in the last section. The way he dealt with causality, however, indirectly characterizes it as such. When he wants to find out why the relation perceived between these events is one of mere succession or one of causality, he is trying to understand what makes it that the point at which A reaches B is perceived in the first case as the boundary between two separate events, and in the second case as constituting a specific link between two parts of a single event. Michotte's work thus studies dynamic boundaries, it studies how links between different moving objects are perceived in dynamic events.

Traditionally, the perception of causality was not thought to be a case of perception at all. Since Hume, those who had studied the problem had considered the causal impression to be the result of an interpretation of or a projection onto what was given in perception. The only thing that was taken to be given in perception was a succession of events. Based on 'habit' and 'constant conjunction', secondary interpretations and expectancies were projected onto the perceived successive movements. Michotte wanted to investigate whether indeed our perception is 'limited to the impression of two movements spatially and temporally co-ordinated' (PC15). He asked if there was not given something more directly in perception, and whether the words the observers used to describe the events (launching, entraining, setting in motion, etc.) could not be rather a direct translation of specific characteristics of the phenomenon.[11]

Michotte set up an extensive range of experiments in which he proves that there is indeed a specific perceptual character of those cases that are described in causal terms. He then set out to determine the structure of this specific perceptual organization as well as the perceptual laws which brought about this particular structure.

[11] « Ou si, peut-être ces notions […] ne traduiraient pas directement des caractères spécifiques de phénomènes répondant à l'action de systèmes déterminés de stimulants sur notre organisme. » Michotte, 1959, p. 19.

First step of the experiment: the boundaries of the causal impression

First, based on trial and error, Michotte tried to obtain a clear example of a causal impression, i.e. where all observers describe the event in clear causal terms. He found it in what he termed the *launching effect*, which we encountered above, and the *entraining effect*: here object A starts moving towards object B, when A reaches B, B also starts moving at the same speed so that they are moving together in contact with one another. Observers reported that A seems to be carrying or pushing B along, that A takes B with it. (Figure 2)

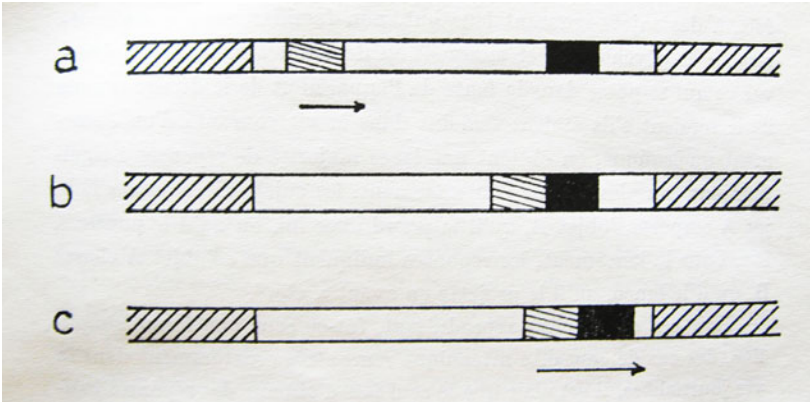

Fig. 2: Schematic representation of the entraining effect. In: A. Michotte, with G. Thinès (1963) 'La causalité perceptive', in *Journal de Psychologie normale et pathologique, 60*, p. 13.

He then stripped these events that led to clear causal impressions of all their irrelevant aspects. He tried to 'reduce the event to its skeleton,'[12] in order to find out what is minimally necessary to evoke a causal impression. Through a number of experiments it became clear that the causal impression could equally well be evoked by displays with images as by displays with material objects. (PC18) Also, colour, form, or size of the objects did not matter much, whereas differences in speed, moment of starting and stopping, or the introduction of pauses, had an important influence on how the events were described.

What was needed in order to do the experiments was thus a way to present very simple schematic figures in different combinations of movement. To this end Michotte designed the Disc Method (Figure 3) and the Projection Method (Figure 4). In the first method, a stimulus presentation instrument is used where large paper discs with coloured lines drawn on them rotate behind a screen. A small horizontal slit in the screen cuts out the view on a small section of these lines, creating in this way a display in which colliding figures are seen. Temporal, spatial and kinematic conditions of the stimuli could be controlled and easily varied by drawing different lines on new discs. In the Projection Method, a mechanism of two connected rotating projectors could project similar images on a screen, yet allowing for figures of different sizes, as well as showing them move in different directions. Observers were asked to look at these displays and report what they saw. The displays and the observers' responses would be the main materials for the experimenter to work with.

[12] Michotte, 1959, p. 20. All translations of Michotte 1959 are my own.

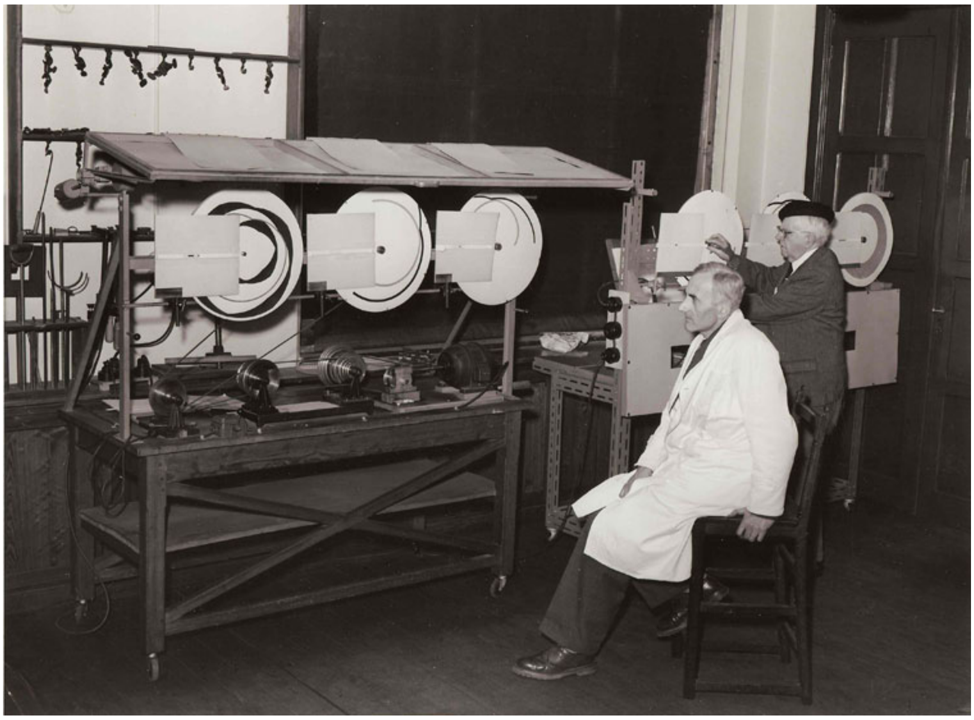

Fig. 3: The 'Banc Michotte', instrument used in the disc method in Michotte's experiments on perceived causality. In the back, Albert Michotte is arranging the discs, his colleague and former student Gérard de Montpellier is seated in front. Photograph from the Collection of the Library of the Faculteit Psychologie en Pedagogische Wetenschappen, Katholieke Universiteit Leuven, Leuven, Belgium.

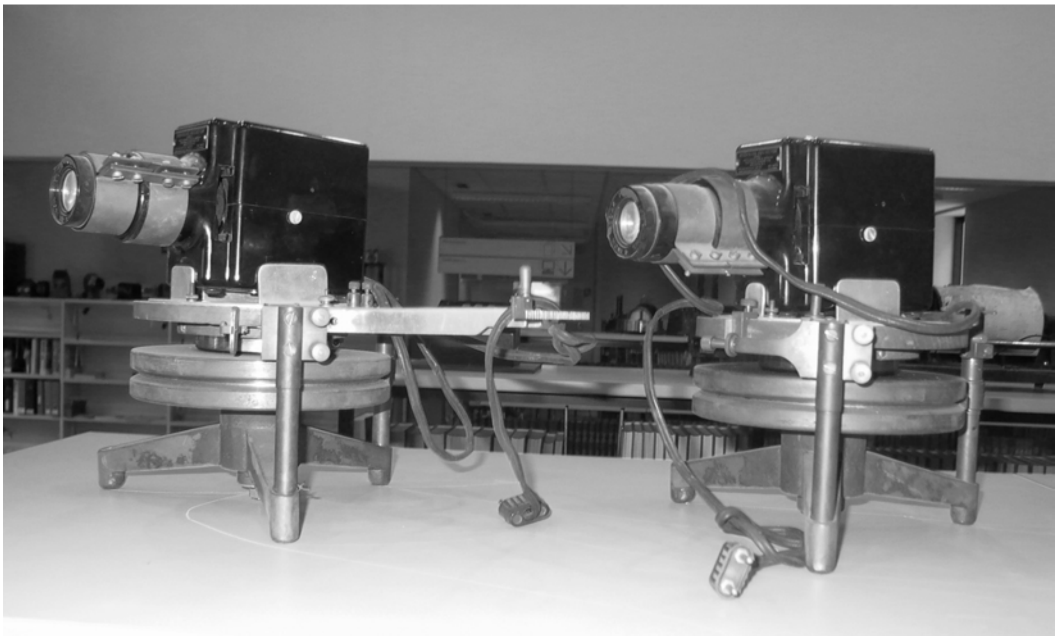

Fig. 4: The projection method, a combination of two connected rotating projectors, used in Michotte's experiments on perceived causality. From: Musée Albert Michotte de la psychologie expérimentale, Faculté de psychologie et des sciences de l'éducation, Université catholique de Louvain, Louvain-la-Neuve, Belgium.

TABLEAU VI.

Mouvements successifs d'un objet. — Répartition des stades d'après les intervalles. (60 cas par intervalle).

Intervalles (ms.).	Continuité.	Discontinuité.	Arrêt.
14	100 %	0	0
28	98 %	2 %	0
42	95 %	5 %	0
56	57 %	43 %	0
70	31 %	68 %	1 %
84	22 %	61 %	17 %
98	9 %	66 %	25 %
112	1 %	45 %	54 %
126	1 %	36 %	63 %
140	1 %	23 %	76 %
154	0	7 %	93 %
168	0	0	100 %
196	0	0	100 %

Fig. 5: Table of the percentages of observers' responses which described either a causal impression (in the form of a direct launching effect or a delayed launching effect) or a succession of movements when a series of intervals was introduced between the arrival of A and the departure of B in the launching case experiment. In: Albert Michotte (1946), *La perception de la causalité*, Louvain: Editions de l'Institut Supérieur de Philosophie, p. 93.

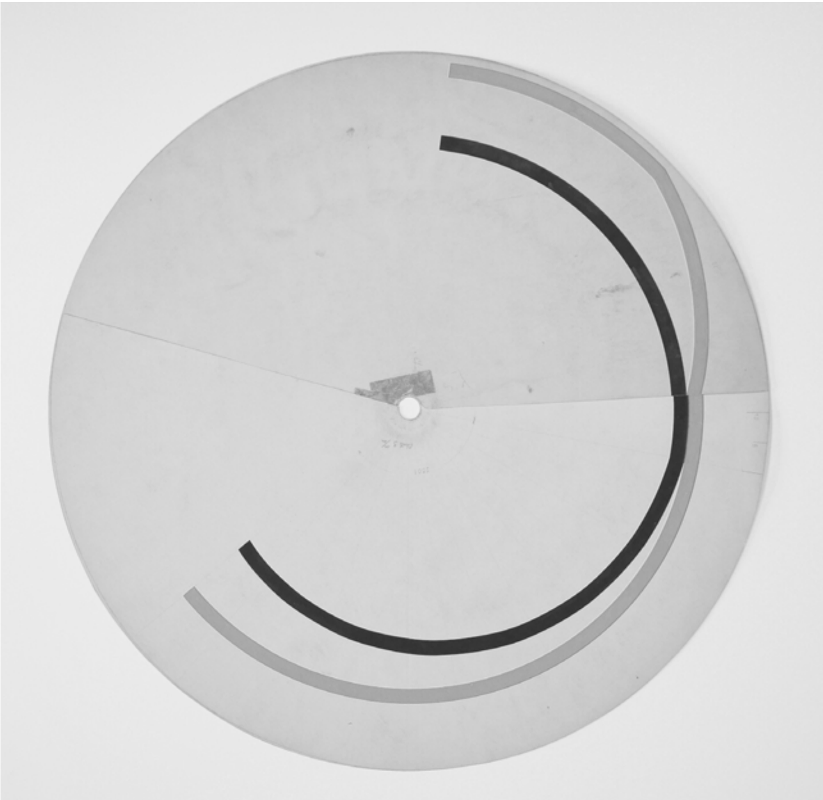

Fig. 6: Disc for the launching effect. When this disc was rotated clockwise, and looked at through a small horizontal slit (along the radius of the circle) in the screen covering it, a red square was seen to go towards a black one, bumping into it and setting it in motion. This adjustable pair of discs can show the launching effect with an interval that corresponds with 0° till 95°. In this photograph, a random interval of 13° is shown, which, at a rotation rate of 1 rotation per second, would amount to an interval of 36ms, which would have presented a clear impression of launching. From the Archive of the Laboratory of Experimental Psychology, Katholieke Universiteit Leuven (Photo Sigrid Leyssen).

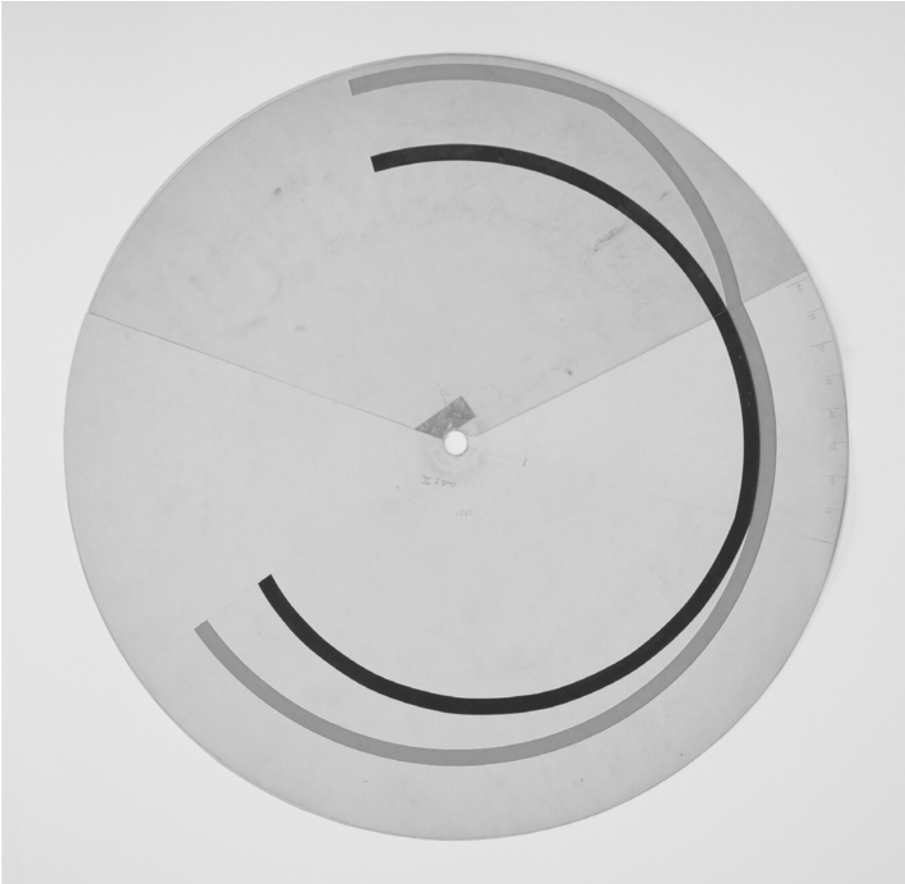

Fig. 7: Disc for the launching effect. This is the same pair of discs as in figure 6, but with a larger interval inserted, one which corresponds with 42°. At one rotation per second, this disc would have presented an interval of 114ms. Here the causal impression is about to disappear, and more than half of the subjects reported that they saw two separate events. From the Archive of the Laboratory of Experimental Psychology, Katholieke Universiteit Leuven (Photo Sigrid Leyssen).

A clear example of a causal impression was then put through many variations. By varying the spatial, temporal and kinematic stimulus conditions, Michotte investigated when the causal impression was retained, altered or disappeared. The experiments showed how very small alterations in the stimulus conditions could make for significant differences in how the events were described by the observers. For example, when introducing a small pause in the launching case, between the moment of A reaching B and B setting off, what at first was described clearly as a causal event of launching, was then described as two separate successive events. The table in figure 5 illustrates such an experiment, where a series of intervals, ranging from 0 to 224 milliseconds in steps of 14 milliseconds, were introduced between the arrival of A and the departure of B in the launching case. The different displays were shown to the subjects in random order. (PC91-95) Varying the interval brought about specific changes in the reported impressions, and although these changes were gradual, fairly clear quantitative boundaries could be drawn indicating where the causal impression disappeared.[13] For example, at 98ms, a broad majority of

[13] The table shows how actually three stages could be clearly distinguished: first the launching effect; then, when the interval approaches one tenth of a second, the launching effect involves some 'time-lag', a 'discontinuous movement', but is still unmistakably described as a causal impression; and then, in a third stage, the causal impression disappears and two separate movements are seen. (PC92)

the subjects still reported a causal impression. At 140ms, and clearly at 154ms, any causal impression had disappeared. (Figure 6 & 7)

In these experiments, the *boundaries between causal and non-causal impressions* were determined. Note that in between the clear cases of causal and non-causal impressions, there always were stimulus configurations which presented borderline cases, grey zones in which some subjects described the events in causal terms, and others not, while some were indecisive about how to describe the events. Instead of clear-cut boundaries, it were rather statistic boundary zones that were found. Nonetheless, Michotte was happily surprised when it became clear from the experiments that the spatiotemporal stimuli conditions needed for the appearance of a causal impression could be so precisely determined. What had seemed at first to be a subjective and elusive phenomenon, previously not even recognised as being perceivable at all, now turned out to be very well quantifiable.

Through hundreds of experiments it could be established that causal responses depended on very specific and narrowly limited stimulus conditions. With the 'concomitant variations'[14], which manifested themselves throughout these many experiments, i.e. the fact that variations in the stimulus conditions were closely followed by variations in the responses, Michotte could prove that there was a systematic dependence of the responses on the stimulus conditions. The causal responses could be shown to be connected to a very specific organization on the perceptual level, which could be clearly distinguished from that which led, for example, to impressions of successive movement. This was also important from a methodological point of view. At the same time, the observers' responses were shown to be fine precision instruments to work with, their contents soundly based in the stimulus conditions. In this way, Michotte could justify them as experimentally obtained materials with which he would work intensively in the next two steps of his method.

In this first step of the experiment, the observers' responses were used only as 'differential reactions'[15] which could indicate about the presence or absence of a certain phenomenon x or aspect of phenomenon x. This was a clear way of answering criticisms advanced by behaviourists that the impressions lived by the subjects were strictly unknowable to the experimenter. In the behaviourist framework, these impressions were incommunicable and words could thus not be used to find out what the experience had been like for the subject. At most, words could indicate that a certain impression was present or not. What happened in this first step of Michotte's experiment – determining the conditions under which a certain phenomenon occurs, whatever it might be 'in itself' – resembles, as Michotte recognised, the method of psychophysics. (PC308) In psychophysics, the task was to determine how strong a stimulus had to be in order to perceived, or how much it had to be altered for a difference to be perceived. Michotte was also determining threshold values, yet of a much less recognised phenomenon: he was determining the threshold values of the causal impression. In this way, he was able to place the causal impression on the same map with other, more typically investigated topics of perception, that is, as a phenomenon with very specific phenomenal characteristics, which could be experimentally investigated.

Second step of the experiment: perceived boundaries differentiated

In order to understand why exactly these stimulus constellations gave rise to causal impressions, a second step[16] in the investigations was needed. What happens here characterises for Michotte his

[14] PC309, 312; Michotte, 1959, p. 21.

[15] Michotte, 1959, p. 17.

[16] In different accounts of his research, Michotte presents his methodology as consisting of different stages or steps which can be methodologically distinguished but which actually take place simultaneously during the experiments. (PC19) This description in three steps follows his account in 'Rôle du langage'

experimental phenomenology of perception in the best way. After determining the boundaries of causal impressions (determining which stimulus conditions produced a causal impression and which made it disappear), Michotte now started to look more profoundly at what happens in clear cases of perceived causality: what happens phenomenally in the course of such an event? Here, we will see, new kinds of boundaries were distinguished.

In this second step, the observers' responses were used in another and to Michotte much more 'rewarding' way. (PC308) He still treated the actual impression as an unknown, but he took the responses seriously as attempts to describe in the best words available what was actually seen. He made use of these verbal materials to build up 'hypotheses as to the structure of the perception'[17], hence to devise a 'schematic reconstruction of the phenomenon.'[18]

Michotte worked with naive subjects, often first year students of the philosophy department in Leuven, who were left unaware of the workings and aims of the experiment. More often, however, he worked with more knowledgeable subjects: he himself, his doctoral students and close colleagues at the laboratory took upon them alternatively the role of experimenter and observer, which was still common methodological practice at the time. (PC40) Sometimes also renowned psychologist colleagues, who were visiting the laboratory, served as subjects.

The kind of observation asked from the observers was specified as ordinary, daily perception. The subjects were asked to 'say what you see in the apparatus', and to give a description 'in as spontaneous a way as possible', 'as one might give when witnessing any sort of event in ordinary life'. (PC305) Sometimes additional questions as 'Could you put it another way? Could you not be more precise?' were posed, but it was carefully avoided to give any suggestions. (PC305-306) They were never asked, Michotte emphasized, 'to adopt the "attitude of introspection"' (PC305) and the requested way of observing stands in stark contrast with a more analytic way of observing, the kind of observation observers in the laboratories of e.g. Wundt, or Titchener, had been trained in, in order to report the raw data of perception. Such a way of observing would, according to Michotte, be completely inadequate for studying causal impressions. Indeed, when looking more analytically at the composing parts of the event alone, no trace of a causal impression is found. It was thus a naive, ordinary way of perceiving that was requested, but one that was as 'perceptive' as possible. (PC8, 23-24)

Often nuanced descriptions were obtained. Certain concepts and turns of phrase proved to be chosen very luckily, grasping the specific character of certain aspects of the event. Sometimes observers spontaneously tried to make their answers more explicit. Sometimes, with the method of 'paired comparison', they were asked to compare e.g. a launching type display with other displays put together for that purpose.[19] Both the work and talent of the observers of choosing words to characterize their impressions, and the work of the experimenter analysing these responses were important.

I want to argue that the main function of these observer responses was to indicate perceived boundaries. Let me explore this by elaborating a number of examples of such descriptions and the kinds of perceived boundaries they allowed to discern.

(1959). In the Appendix 'Theory of Phenomenal Causality. New Perspectives', which was added to the English translation of his book *The Perception of Causality* (1963), and which first appeared in Michotte (1962) *Causalité, permanence, réalité*, he distinguishes seven stages.

17 Michotte, 1959, p. 23.

18 Michotte, 1959, p. 25.

19 Michotte, 1959, p. 22.

'B moves farther than it should have done'

It had been noticed with regard to the entraining effect that the movement of A and B seemed to be of a different character, and that the character of the movement of B was particularly difficult to describe. (Figure 8) Peculiar expressions were used: 'B *is* in movement, but A *has* the movement,' 'B participates in A's movement, but has not itself any movement of its own,' or 'It is A which does everything; B does nothing.' (PC314) This made Michotte recognise that the word 'movement' was used here in two different senses, and that observers tried to differentiate between them in their wordings. He stated that on a phenomenal level, one had to distinguish between *movement* and *displacement*. The physical movement of an object can, on a phenomenal level, sometimes be seen as *movement*, and sometimes as mere *displacement*, i.e., 'a simple change in position without [...] taking on the perceptual (phenomenal) character of movement belonging to the object in question'. (PC314)

Fig. 8: Disc for the entraining effect. A red rectangle A is seen to approach a black rectangle B, and to take B with it. From the Archive of the Laboratory of Experimental Psychology, Katholieke Universiteit Leuven (Photo Sigrid Leyssen).

A second observation was made, here with regard to the launching case, namely that the movement of B itself seemed to change character during its course (PC59): 'subjects remarked that "B went farther than it should have done", [...] that after a certain distance "it was moving of its own accord" and its movement "no longer had anything to do with its having been struck"' (PC53). The displacement of B apparently loses its passivity and seems to become an autonomous movement after a while. To much initial surprise of the experimenters, this change in character of B's movement could be determined experimentally with great precision (PC54). For example, for one subject, B's movement changed character after 59,7 mm, with A and B moving both at 32 cm/sec. This distance diminished considerably at lower speeds, e.g. to 23,1 mm at a speed of 8cm/sec.[20] Something similar applied to the entraining case: when A and B take off together, this is first described as A entraining B. When their movement is prolonged, this sense of A producing B's movement disappears and a pure 'transport effect' appears (cf. below). (PC153)

Thus, where the motion of B changes character, a new perceived boundary is discovered. This made Michotte recognise that instead of two phases (one before and one after the moment of impact), there is yet another, intermediate phase to be distinguished, where a special perceptual structure is established. Instead of one moment of impact, a more extended boundary phase is perceived, which characterises the causal impressions under study.

The observers' responses in their specific phrasings allowed to find out where, in the course of an event described as causal, changes in character could be located. These perceived boundaries were then used to reconstruct the phenomenal structure of the event, dividing it in different phases, the organization of which would be explored more deeply through further experiments.

'But there is only one movement!'

Another insight gained from analysing the observers' responses was that, phenomenally, 'movement may in certain conditions detach itself from the objects' (PC137). Referring to Max Wertheimer's (1880-1943) absolute 'separability' of movement[21], contested at the time, Michotte realised that in order to understand the perceptual structure of causal impressions, the movements and the objects to which these movements 'belong' have to be distinguished each as phenomena sui generis (PC137). In this way, even more subtle perceived boundaries can be discerned, and it turns out that the perceived boundaries of an object and that of its movement do not necessarily coincide.

Perhaps one of the most lucky descriptions of the entraining effect, Michotte indicated, was when someone realized that there really is just one movement. It was uttered in an outburst of surprise, as a sudden discovery, by an observer who had observed already more than hundred of such cases: 'But there is only one movement! It is the movement of A that entrains B!'[22] This description provided a preliminary formulation of the two concepts which Michotte conceived to characterise the main structures of the causal impression: *phenomenal duplication* and *ampliation of movement*.

Phenomenal duplication[23] refers to the fact that the movement performed by object B in the second phase of both the launching and the entraining case has a dual aspect to it: it is seen both as the movement of A and as the displacement of B.

It might seem strange at first, that phenomenally a movement and the object performing it do not always form a closed unity. This is however what happens also in a number of phenomena other than perceived causality. One movement can 'belong' phenomenally to different objects. For example, in a group movement, such as the passing of a formation of planes, where all the objects

[20] See PC55, table I.

[21] On Wertheimer's stroboscopic research on the pure phi phenomenon, where observers saw motion without a moving object, see Wertheimer (1912).

[22] PC151; Michotte, 1959, p. 23.

[23] Michotte borrows this term *phenomenal duplication* from David Katz (1884-1953) and his work on colour perception: the colour of an object seen through a transparent coloured screen has a dual aspect to it. Two distinct impressions, that of the colour of the object and that of the transparent screen itself are simultaneously there. (PC136)

(all seen as active and independent objects) are perceived as co-carriers of what is seen as one movement, namely that of the group.[24] One movement can be extended over different moving objects without belonging to all of them as well. This is the case in what Michotte called the *transport effect.* This phenomenon is known from examples such as a lorry transporting boxes, or from seeing a car passing in which a passenger is seated. The transporting object in one way or another isolates the transported object from the space around it so that the frame of reference of the transported object is the carrier object, while that of the vehicle is the surrounding field. Due to this separation in the system of reference, the movement is perceived as 'belonging' to the carrier object only, not to the transported object. Although the transported object is 'in movement', it is seen as immobile, as being merely displaced and carried by the movement of the transporting object. (PC151)

This structure of one movement being extended over several objects in a hierarchical way, and the phenomenal duplication that goes with it is also recognisable in the entraining effect. The difference between the transport effect and the entraining effect is however that the first is never described in causal terms: it was described as A supporting B, or carrying B, but never as A entraining B, or carrying B off. In the entraining effect, while B is still at rest, A is already moving. In this way, phenomenally, the dominance of A and the pre-existence of its movement is established. The difference with the transport effect lies in the fact that in the entraining effect, the process of A's (pre-existing) movement *passing on* to B can be perceived. (PC154) This is where the concept of *ampliation of movement* comes in. Ampliation of movement refers to 'the *creating* or *establishing* of the extension on to the second object of the already existing movement of a first object, in such a way that this movement brings about the displacement of the second object.' (PC143) This ampliation of movement is perhaps even more stunning in the case of the launching effect. Here, even when A has stopped, it is its movement that makes for the displacement of B. '[A] lthough A has stopped, the movement actually performed by B continues to 'belong' phenomenally to A, i.e. it appears as a *prolongation* of the latter's movement, during the short period of the second phase.' (PC345)

This structure of the *ampliation of movement* is the main characteristic of the perceived causality Michotte is studying and it accounts for the productive aspect of the causal impression, i.e. that A is seen to produce the movement of B. 'It is the actual passing over of the movement from one object to another which gives rise to the casual impression.' (PC 154) It is this *perceived boundary crossing* that explains the causal impression.

Here, yet another perceived boundary, or rather the absence of a perceived boundary is marked out: in the launching case, the movement of A has no boundary where and when object A stops, and for a short while A's movement is extended onto another object B. Phenomenally, the boundaries of the objects prove to be permeable ones with regard to movements.

Third step of the experiment: using perceived boundaries to find perceptual laws

The third step of the investigations was concerned with the question why this specific perceptual structure of the launching or the entraining effect (elaborated in the second step) necessarily resulted from the specific system of stimulations, which was found in the first step. In the second step, phenomenally given boundaries had been distinguished which were then used for reconstructing the phenomenal structure of the causal impression. In the third step, these perceived boundaries are used to help determine the perceptual laws that must have been in effect, in order for these typical structures of ampliation of movement or phenomenal duplication to be in place.

[24] Michotte, 1959, p. 28.

For this purpose, Michotte applied what he called a *genetic analysis*, i.e. considering more simple events and then comparing these with the original complex event. (PC18-19) He set up, for example, an experiment with a display similar to that of the entraining case, yet with its first phase eliminated: A and B are joined and begin to move together in the same direction at the same speed. Here the *principle of common fate* applies. The fact that A and B start moving at the same time, in the same direction and at the same speed makes the objects seem to form a single shape or a closely integrated group, and there appears to be only one movement, performed by this one compounded object.

In the entraining case, these identical stimulus conditions can be found as well, and the principle of common fate could be expected to similarly bring about an integration of the movements and the objects. Yet, here it seems to take a while before the common fate principle comes into full effect. Typical of the entraining case, there is a temporal delay between the integration of the movements and that of the objects.[25] Kinematic integration occurs very quickly in the second phase. The movements of A and B in the second phase are similar in a number of aspects, which encourages an identification of the movements (PC118), and observers described that they saw only one single movement. The two objects, however, remain distinct. In the first phase, the objects had been established as autonomous objects, clearly distinguished by their shape, colour, place and distance from one another, as well as by the difference between their states (moving versus at rest). (PC326) The observers indicated that during the second phase they still saw a duality of shapes after the objects have joined and this at a time at which the stimulus conditions are such that, had there not been a first phase, they would have been seen as a single figure. (PC330) It is only in the third phase that the common fate principle can fully set in.

There seems to be nothing in the stimulus conditions of the second phase to justify these responses of a duality of objects, which is why Michotte concluded that these must have been due to some influence of the first phase. (PC328) Some structures which are established in the first phase – i.e. the segregation of the objects, the movement of A, and the hierarchy of the objects due to the difference in their states (moving vs. at rest) which make object A phenomenally dominant – remain effective for a short while.

The temporary, instable second phase is then understood as a short period in which these previously established structures remain in effect and *conflict* with the common fate principle. A *compromise* structure (PC333) is formed, in which traces of the demands of both laws (common fate, and the persistence of previously established structures, which Michotte called 'phenomenal permanence') can be found, only partially satisfied. It is a necessarily provisional structure, a structure of transition between two more stable phases.

The laws and principles Michotte appealed to are all laws that bring about integrations and segregations. This is perhaps hardly surprising as these laws were meant to explain how the stimulus conditions gave rise to the perceived instances of 'belonging' (e.g. movements belonging or not belonging to objects), fusion, identification, or duplication which together formed the typical perceptual structure of the entraining or the launching effect. *Which* laws could be expected to apply to certain stimulus configurations was known from studies of more stable situations. *When* these laws were actually in effect was deduced from comparing (1) the point where new stimulus conditions were installed and (2) the point where the perceptual structures which, due to known laws, could be expected to follow from these new stimulus conditions were actually perceived. The 'boundaries' given in the stimulus conditions and the perceived ones do not necessarily coincide. Studying the specific character of the interval between both could determine the way in which the demands of a certain perceptual law could take effect, either as

[25] PC326; Michotte, 1959, p. 29.

prevailing principle, or as involved in compromise structures. Here it becomes clear that where, when and which boundaries were perceived or not became a useful tool for deducing the range of interaction of such perceptual laws.

Boundary phenomena

The perceived boundaries involved in the causal impression discussed so far are boundaries that are rather subtle and it took the experimenters and the observers some effort to discover them. Together, these different perceived boundaries were used to explain the specific link between two movements, that of object A and object B, in cases of perceived causality, which I have considered here as a dynamic boundary phenomenon in its own right.

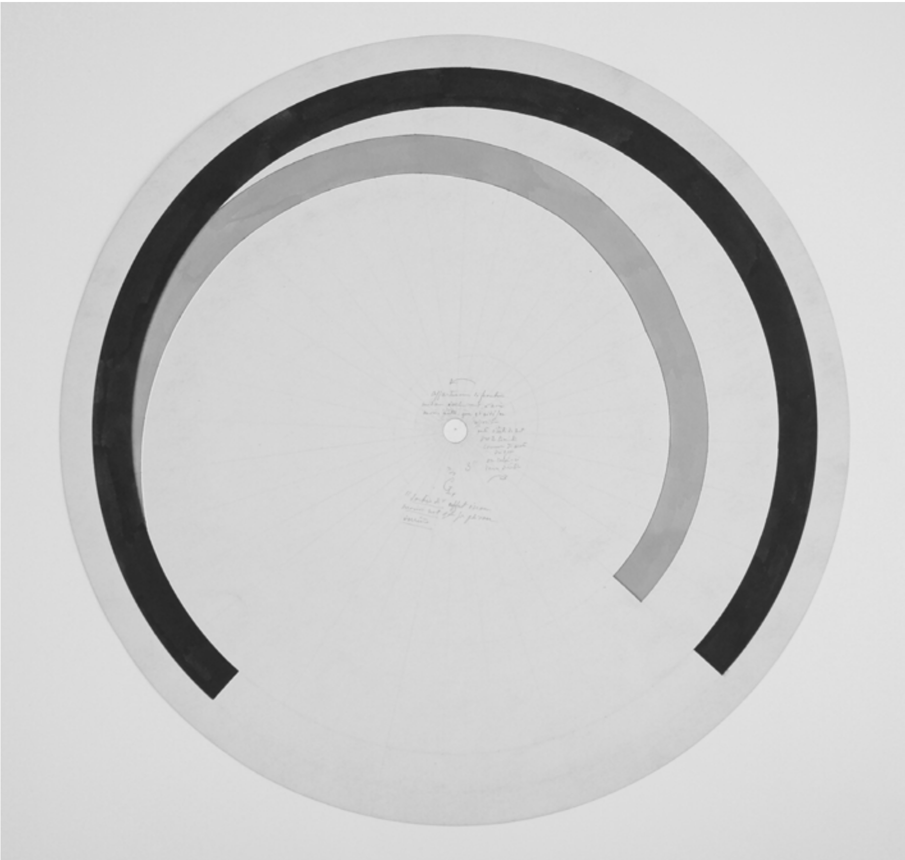

Fig. 9: Disc used for the screen effect of B moving behind A, when rotated counter clockwise, and for the screen effect of B coming out from behind of A, when rotated clockwise. From the Archive of the Laboratory of Experimental Psychology, Katholieke Universiteit Leuven (Photo Sigrid Leyssen).

Michotte, however, also studied phenomena which are considered more explicitly as *boundary phenomena*. A parallel series of investigations at Michotte's laboratory was concerned with the *screen effect* or *tunnel effect*.[26] In an experimental setup similar to that of the causality experiments, a display showed a stationary object A. Another object B starts moving towards A, and gradually becomes shorter when having reached A. (Figure 9) Observers did not have the impression of object B being compressed when running into A, as one could have expected. In contrast, they

[26] This effect was known since Wertheimer's classic study on the seeing of movement, Wertheimer (1912).

described the event as object B moving behind object A, as if it were a screen. The way in which the boundary line between A and B is perceived, determines the perception of this particular event, which is called the screen effect. The tunnel effect refers to similar cases where, however, after a certain interval, B is made to reappear at the other side of A. Subjects described how they *saw* B moving through a tunnel.[27]

This screen effect was explicitly understood as a *boundary phenomenon*.[28] It was considered as Edgar Rubin's figure-ground principle set in motion.[29] This figure-ground principle is well-known from Rubin's faces-vase drawing (Figure 10). Rubin realized that the word 'limit' on the phenomenal plane had a very specific meaning. Phenomenally, the limit or contour of a figure was something that could be dissociated from the surface of that figure.[30] In the case of adjacent figures, a contour can phenomenally 'belong' to one figure only, the other figure then has no visible boundary and seems to extend itself behind the first figure.

Fig. 10: Edgar Rubin's Faces-Vase figure. Here the figure-ground principle is exploited for an illusory effect: ambiguities in the perceptual structure make that the contour can be assigned both to the black or the white regions, making alternatively the one or the other function as background. In: Edgar Rubin (1921) *Visuell wahrgenommene Figuren*, Copenhague: Gyldendalske, Annex Figure 3.

The screen effect was at first understood in similar terms, as a function of the contour. The line which is seen as the boundary between two objects seems to belong to the screen, that is, object A, and not to object B. Now that B phenomenally does not have a boundary in that direction, it seems to extend itself, behind or inside A.[31]

[27] See the work of Michotte's students on phenomenal permanence, all republished in Michotte (1962): amongst others Sampaio (1962) [1943], and Knops (1962) [1947].

[28] 'L'effet Écran lui-même est essentiellement un phénomène de limites' in Knops, 1962, p. 342.

[29] Michotte, 1959, p. 26-28 ; Rubin (1921).

[30] In a similar move, Michotte had considered 'movement' as something that phenomenally could be dissociated from the object performing it.

[31] Sampaio (1962).

In an article a few years later, in 1950, Michotte made the story more complex, by introducing also the *temporal limits* at play.[32] He discusses this by means of an example where the object B starts to come out from behind a fixed object A (the screen). (Figure 9, rotating the same disc in the other direction) Here an object B gradually glides from behind A, producing the perception of a progressive evolution. Yet this evolution does not seem to belong to the *object* B. Spatial conditions (the shared boundary phenomenally belonging to the fixed object A, not to the moving object B), as we have just seen, assure B's extension behind A: B seems to have been behind A already, and there seems to be no internal evolution in the object. The evolution, then, seems to 'belong' only to the movement. Phenomenally, the temporal limit which is seen only belongs to the movement of B, not to the object B. The temporal limits of the movement and of the object performing it do not always coincide. It is in this way that the object can be seen to extend itself beyond this temporal limit, and to give an impression of 'having been there already'.

The screen effect thus indeed has to be understood as a boundary phenomenon, but one whose specific characteristics are understood by studying how both spatial and temporal limits of both objects and movements are perceived. Here the catalogue of different perceived boundaries is differentiated once more. Not only the boundaries of objects versus movements have to be distinguished, but also the spatial boundaries belonging to the respective objects, and the temporal boundaries belonging to the respective movements.

Conclusion

Distinguishing more and different kinds of perceived boundaries was accompanied by moments of surprise: on the side of the observers, when they realized that this was what they actually saw; on the side of the researchers, when it became clear that the perceived boundaries, elusive as they were, could be experimentally determined with much precision. Perceived boundaries, much more subtle and diversified as previously thought, could be discovered and determined in the experimental process.

In this paper, we encountered different types of boundaries. First, there are the boundaries between classes of perceptual phenomena, or, in this case study, the boundaries between causal and non-causal impressions. These boundaries were drawn based on measurements, obtained by comparing the impressions produced by displays of often only slightly differing combinations of movements, varying the relevant stimulus conditions. The boundaries of what was described as 'causal' or 'not causal' proved to be determined by well-defined and narrowly limited stimulus conditions. Second, there are the perceived boundaries within certain perceptual phenomena. On the one hand, looking inside some clear examples of a causal impression such as the launching or the entraining effect, different phases could be distinguished in the course of the impression. Observers' responses indicated where significant changes during the perceptual event were seen, and these perceived boundaries could be experimentally determined just as precisely as those of the impression as a whole. On the other hand, in a closer analysis of the impression, observers' descriptions allowed to distinguish the perceived boundaries both of the *movements* and those of the *objects* performing them. It was exactly the non-coinciding of the perceived boundaries of objects and movements which explained why certain events were perceived as 'causal'. Such perceived boundaries are themselves dynamic, spatiotemporal phenomena. Yet, in some experiments, such as that on the screen effect, temporal and spatial boundaries could be distinguished as well. Here, the perceived temporal boundary was shown to belong to the movement of B only, without belonging to the object B.

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[32] Michotte (1962b) [1950]. On the growing importance of the temporal in Michotte's work, see also Paracchini (2008).

These different perceived boundaries were not studied for their own sake. They were used as important tools to build a theory with which to understand impressions of causality. The perceived boundaries were used to construct a hypothesis of the perceptual structures of the event. They were also called in to help to deduce when a given perceptual law must be in effect, and to deduce the resolution of conflicts between different perceptual laws. For understanding the perception of dynamic events, it was especially important to study such transient structures as that which was established in the second phase of the launching and entraining cases, and to understand the different interactions between perceptual laws occurring there.

This same method was also applied to analyse more straightforward perceptions of boundaries, as in the case of the screen effect. Here, the way in which the boundary line between A and B is perceived clearly *determines* how the interaction is understood: whether B is seen to crush against A when reaching A, or whether B is seen to slide behind A. In this case as well, more subtle and differentiated perceived boundaries were appealed to in order to determine why this perceived boundary is perceived in its specific way.

From a project such as that of Albert Michotte, it becomes clear that the concept of 'boundary' in science is a broad and complex one which needs differentiation. Boundaries should not be understood only in a spatial sense, as a demarcation of an area or an object. Boundaries can be dynamic phenomena and boundaries can be seen at different levels: boundaries of objects, of movements, of movement characteristics, and we even perceive boundaries between *classes* of perceptual phenomena.

Furthermore, boundaries do not have to be 'really there' in order to be perceived. The fact that we perceive a boundary on a particular place and time, can be the result of a whole constellation of stimuli, without there having to be a definite stimulus corresponding to it.[33] There is also the following consideration. The psychological research discussed in this paper is situated almost entirely on the phenomenal plane. Perceptual factors responsible for the impression of causality, and for certain perceived boundaries were discussed. In other texts, Michotte also discussed the relation between what we *see* and what we *know* to be there.[34] Our impressions and beliefs can exist in various combinations, and they do not necessarily coincide. Michotte used the discrepancy between impression and belief in order to study perceptual structures in isolation. Hume had claimed that any sense of causality was added secondarily to the perceptual data, based on what we know. In contrast, Michotte's aim was to show that causality and boundary phenomena were *given in perception*. Michotte's experiments separated as much as possible the impression from the belief, so that he could study the *perception* of causality (and of boundaries) in itself.

In the causality experiments, there is an impression of causality, but no causal impact is physically taking place, and this was not a secret for the observers. These experiments were a kind of 'illusion', but this 'illusion' served to study boundaries that were given in perceptual structures that were very real. Indeed, the stimulus conditions and boundary conditions for these perceptual structures could be experimentally determined with fine-grained precision. Nevertheless, when the boundaries that we perceive depend so much on perceptual structures, this leaves us with the troubling question: when scientists see a boundary, what do they actually see?

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[33] On this aspect, see also Michotte's discussion of different cases of *amodal completion* (Michotte et al. 1964).

[34] See e.g. Michotte (1948a; 1948b; 1960).

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Bodies, Interstices, and the Victorian Imagination

Tina Young Choi

This paper is not part of the electronic version, please see printed volume.

Boundaries of the Living: From *Un-dead* Surfaces to the Politics of Surfaces

The Vitality of Membranes: Artificial Cells as Boundary Objects

Thomas Brandstetter

Of recent – due not least to current developments in biology – the history of artificial life, and especially of artificial cells, has attracted the attention of practitioners as well as historians of science. Typically, these accounts take as their starting point the years around 1900, when, as Evelyn Fox Keller has shown, the term 'synthetic biology' was first coined its meaning encompassing the various methods then in existence geared towards the simulation, as it were, of living processes by means of physical and chemical models. Indeed, some of the narratives assume a linear development leading from these early models to today's research on protocells.[1]

In contrast, in this paper, I want to concentrate on one particular, and much earlier episode in the history of artificial cells: the period from 1840 to the 1860s, and the period during which, in fact, the very first physical and chemical models of cells were constructed. At the same time, I want to advocate a context-sensitive approach to these efforts. Instead of treating them as forerunners of recent developments, I shall examine the contemporary meanings and uses of such models. I do not think there is a universal urge to create life artificially, notwithstanding the age-old reiterations of the myth of Pygmalion. Also, it is by no means evident why, or in which specific contexts, artificial recreations of living beings might have been epistemologically important: After all, as Otto Bütschli, by all means an early authority on artificial creatures, once stated, creating an artificial amoeba would just present us with one more amoeba which we do not understand.[2]

To this end, I shall argue that in the 1840s, work on artificial cells did not represent a genuine research programme. Instead, such entities served as arguments against what has been called the "teleomechanist takeover" of cell theory.[3] What is more, these were not so much discursive arguments but material and performative ones: instead of explaining verbally how an organic process worked, they made evident that an organic process could be imitated by purely mechanical means. In this context, membranes played a crucial role: they were deemed essential parts of living cells; further, as artificial cells were not only able to imitate membranes but also the process of osmosis linked to these, a mechanical, non-teleological view of life could be supported on the grounds of artificial membranes.

1 Martin M. Hanczyc: "The Early History of Protocells: The Search for the Recipe of Life", in: Steen Rasmussen, et. al. (ed.), Protocells. Bridging nonliving and living matter, Cambridge, Mass.: MIT Press 2009, S. 3-18; Evelyn Fox-Keller: Making Sense of Life. Explaining Biological Development with Models, Metaphors and Machines, Cambridge, Mass.: Harvard University Press 2003; Milan Zeleny, Klir, George J., Hufford, Kevin D.: "Precipitation Membranes, Osmotic Growths and Synthetic Biology", in: Christopher G. Langton, Artificial Life, Redwood City, Cal. u.a.: Addison-Wesley Publishing Company 1989, S. 125-139. Cf. also Jessica Riskin (ed.): Genesis Redux. Essays in the History and Philosophy of Artificial Life, Chicago/London: University of Chicago Press 2007.

2 Otto Bütschli: Mechanismus und Vitalismus, Leipzig: Engelmann 1901, p. 94.

3 François Duchesneau: "Vitalism and Anti-Vitalism in Schwann's Program for the Cell Theory", in: Guido Cimino, Duchesneau, François (ed.), Vitalisms from Haller to the cell theory. Proceedings of the Zaragoza symposium, XIXth International Congress of History of Science, 22 – 29 August 1993, Florenz: L. S. Olschki 1997, S. 225-251: 240.

1. Cell Formation and Crystallisation

When Matthias Schleiden and Theodor Schwann proposed their 'theory of cells' in 1839, they defined three features as essential to any cell: the cell nucleus or cytoblast, which was the origin of cell development, the fluid contained in the cell, and the cell wall or membrane. Cells were supposed to originate not by cell division (this became the central dogma not until 1855), but to form in a structureless fluid, the cytoblastema.[4] In this context, Schwann introduced a very influential model: crystallisation. He proposed that the formation of a cell in the cytoblastema could be likened to the formation of a crystal in a homogenous mother lye, and he described the development of molecular layers, which would finally become the membrane, as a type of precipitation.

It is important to note that Schwann was not so much interested in the metabolic function of cells. Rather, he wanted to shed light on the process of cell development, hoping that this would allow to find general laws valid for all living beings, thereby establishing a unified science of biology.

For us, the interesting point is that Schwann did not stop at a general analogy between cell formation and crystallisation. He went one step further and presented a detailed explanatory model that involved a somewhat imaginary entity: the crystal capable of imbibition, that is, a crystal capable of taking up liquid in its inner structure.[5] This, as he admitted, was a paradoxical notion in as much crystals, in contrast to organic matter, were not capable of assimilating liquid. However, it furnished him with a means to sketch a detailed (if hypothetical) explanation of how molecular interactions were sufficient to account for all aspects of cell formation.

The focus for Schwann clearly rested on the explanatory capabilities of the model, particularly on its ability to make the process of organic development comprehensible as a non-teleological, purely mechanical series of molecular events. Or this was the epistemological core of Schwann's theory: an explanation of organic processes with no recourse to vital directive forces.

I have tried to knock together a small diagram of the process as Schwann describes it [Fig. 1].

Fig. 1

In the model, the outmost layer of the crystal (which would eventually become the membrane) represented the functional mechanism that created the difference between the inside and the outside of the cell. According to the model, this layer attracted molecules which then were assimilated into the layer so as to make the surface grow exponentially in relation to its thickness. Once the surface became too extensive, it would detach itself from the body of the crystal (in this case, the nucleus) and thereby create an interstice between the nucleus and the membrane. Crystals capable of imbibition therefore would not form a solid body by just adding layers of molecules around their core; instead, they would form a hollow vesicle.[6] This way, the membrane would be

4 For the history of cell theory, cf. Parnes, Ohad: "The Envisioning of Cells", in: Science in Context 13 (2000), S. 71-92; Jacyna, L.S.: "Romantic Thought and the Origins of Cell Biology", in: Cunningham, Andrew, Jardine, Nicholas (ed.), Romanticism and the sciences, Cambridge: Cambridge University Press 1990, S. 161-168; Duchesneau, François: Genèse de la théorie cellulaire, Montréal, Paris: Bellarmin, Vrin 1987.

5 Schwann, Theodor: Mikroskopische Untersuchungen über die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen, Frankfurt/Main: H. Deutsch 2006, p. 243.

6 ibid., pp. 243-248.

able to perform the metabolic functions of the cell, that is, the chemical transformation of substances and the production of new substances.[7] It is immediately apparent how, at this time, the old notion of 'cellula' as an enclosure still played an important role for the definition of what a cell was – the cell was essentially conceived as a hollow vesicle (this was to change in the 1860s with the rise of protoplasm as the main epistemic object in this connection; the latter implied a shift of attention from the more structural inside/outside dichotomy to the properties inherent in a specific substance).

In Schwann's theory of cells, then, the membrane mainly served the purpose of conceptualising cell formation in terms of precipitation – in other words, on purely physical and chemical grounds. The membrane was a mechanism at the heart of the development of cells. The fact that its layers of molecules did ingest newly arriving molecules instead of just adding them led to the formation of the hollow container that was the cell. In a sense, the membrane also served to identify cells in the first place: the presence of the former signalled the presence of the latter. No wonder the membrane became an important visual signifier of the cell, a visual schema making such entities recognisable under the microscope. This image of the cell as a vesicle defined by clear contours indeed was to become very influential. When in the last third of the 19th century more and more researchers shifted their attention to the cell content, the protoplasm, they even lamented these obstructing layers as an epistemological obstacle.[8]

2. Inventing Artificial Cells

For Schwann, as we have seen, the 'crystal capable of imbibition' was a thought experiment, an imaginary entity not unlike the models Maxwell would present some 15 years later in order to make electromagnetism comprehensible in terms of mechanical contrivances. However, things really got rolling when, in 1840, two researchers claimed to have actually identified such entities.

The first was the Berlin physician Ferdinand Ascherson, who, by mixing protein with oil, had created what he called a "haptogenic membrane".[9] This phenomenon, he stated, shows how "a membranous vesicle or a cell" – note again how the membrane is constitutive for the notion of the cell – could form by purely physical means.[10] Thus he called his products both, "true cells" and "artificial cells", asserting that there could be no doubt that "hymenogeny [that is, the development of membranes, TB] function[ed] in living beings in the same way as it does in the test tube of the chemist."[11]

The second to tinker with artificial cells along such lines was the Dutch physician Pieter Harting, who for his part published a lengthy article on microscopical investigations of diverse precipitations [Fig. 2].[12] In there, he came to the conclusion that such phenomena materially

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7 ibid., p. 255.

8 "We are so captured with the personality of the cell that we habitually draw a boundary-line around it, and question the testimony of our microscopes when we fail to find such an indication of isolation." Charles O. Whitman 1893, cit. after Andrew Reynolds: "The Theory of the Cell State and the Question of Cell Autonomy in Nineteenth and Early Twentieth-Century Biology", in: Science in Context 20 (2007), S. 71-95: 80. Cf. also Max Schultze: "Ueber Muskelkörperchen und das, was man eine Zelle zu nennen habe", in: Archiv für Anatomie, Physiologie und wissenschaftliche Medicin, 1861, S. 1-27.

9 Ferdinand M. Ascherson: "Ueber den physiologischen Nutzen der Fettstoffe und über eine neue auf deren Mitwirkung begründeten und durch mehrere neuen Tatsachen unterstützten Theorie der Zellenbildung", in: Archiv für Anatomie, Physiologie und wissenschaftliche Medicin 7 (1840), S. 44-68.

10 ibid., p. 53.

11 ibid., p. 55.

12 Pieter Harting: "Etude microscopique des précipités et de leurs métamorphoses, appliqué à l'explication

realized the idea of Schleiden and Schwann's, i.e. the idea that cells precipitate from liquid matter like crystals. As some of the objects he investigated showed considerable visual similarity to cells, he argued accordingly that these objects presented evidence that cells also must derive from inorganic precipitations, and that their development could therefore be explained by physical laws.

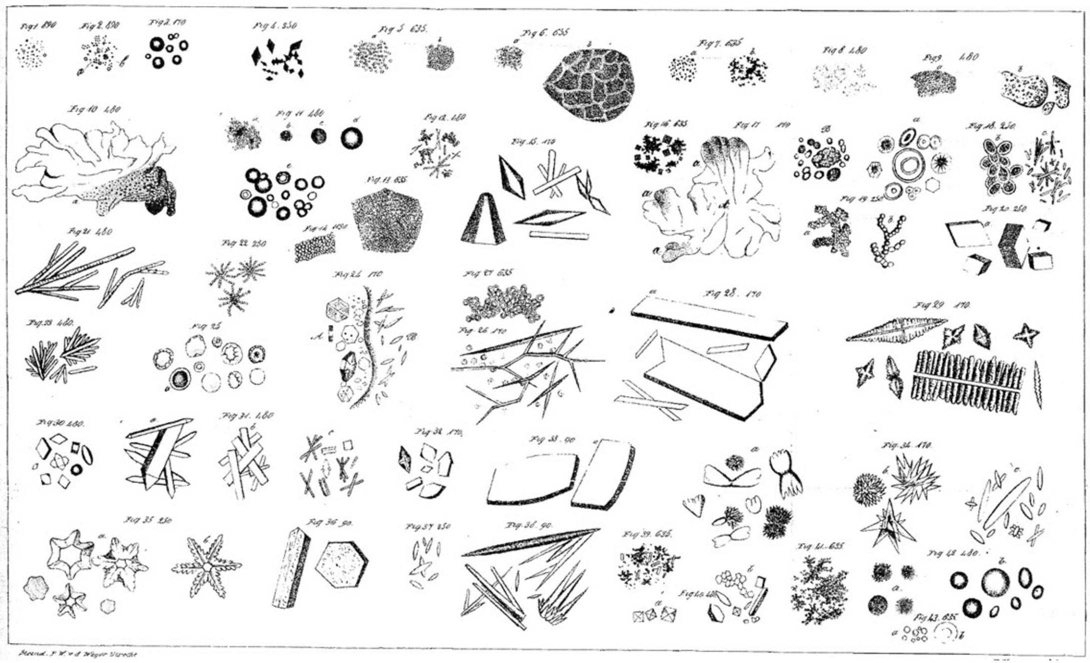

Fig. 2

These two publications created some considerable stir and subsequently led to a lively debate on the epistemological significance of such objects and their possible uses. Figure 3 provides an overview of the debates from 1840 to the 1860s; it lists the authors involved in these experiments as well as some of the authors referring to these experiments either affirmatively or sceptically (marked by + or -). The first thing one notes is that there were not a great many affirmative commentaries; it seems that people did invoke artificial cells only when they conducted experiments themselves. This supports the stipulation above, i.e. the notion that artificial cells were performative arguments. Second, it is readily evident that the few affirmative commentaries that there were came from the camp of radical materialists such as Moleschott and Büchner. And third, it is easy to discern a considerable clustering of critical interventions in the period around 1852-1856. This timing is important – as Timothy Lenoir has shown, the mechanist approach, which by this time was already associated with the organic physicists gathering around du Bois-Reymond and Helmholtz, then came under attack from the last generation of teleomechanists.[13] The explanation of cell development by means of physico-chemical processes was considered one of the weakest points in the reductionist programme and therefore chosen as an easy target in this all-out attack, which aimed not only at specific explanations, but at the very way of doing biological research. Indeed the so-called Materialismusstreit raged in parallel, in which radical materialists like Moleschott, Büchner and Vogt – who advocated a strict mechanist view of nature – were

de divers phenomènes physiques et physiologiques", in: Bulletin des sciences physiques et naturelles en Néerland 3 (1840), S. 287-365.

13 Timothy Lenoir: The strategy of life. Teleology and mechanics in nineteenth century German biology, Dordrecht: Reidel 1982, p. 219.

pitted against reactionary religious scientists (so perceived by the former) such as the anatomist Rudolph Wagner. To what extent these two developments must be seen as having belonged together, must remain subject to a future paper; even so, it should be clear that the performativity of artificial cells unfolded a significance well beyond the confines of the laboratory.

		+	-	?
1840	Ascherson		Henle (1841)	
1841	Hetterschij	Kürschner (1843)	Karsten (1843), Reichert (1843)	Du Bois-Reymond (1843)
1845	Schmidt		Günter (1845)	
1847	Reissek		Robin (1848)	
1850	Wittich			
1850	Melsens		Lotze (1851)	
1851	Lyons			
1852	Panum	Moleschott (1852)	Eschricht (1852), Virchow (1852), Henle (1852), Reichert (1852, 1855, 1856)	
1858	Rainey	Büchner (1861)		
1863	Schultze			

Fig. 3

3. *Artificial Cells as Arguments*

Unfortunately, I cannot here present a detailed chronicle and analysis of the controversies surrounding artificial cells and therefore will restrict myself to offering some glimpses of the way in which these objects were employed as polemical arguments.

To do this, I first have to briefly delineate the reception of the so-called cell theory by its contemporaries. The latter, the cell theory, in general was taken up quite rapidly, and especially so among the teleomechanist biologists around Johannes Müller.[14] This is by no means obvious, as Schwann himself had designed the theory as a counter-argument to the teleomechanist approach. Therefore, teleomechanists had to adopt the theory to fit their own requirements. This was mainly achieved by stressing the integration of cells into the greater whole of the organism. In the process, cells were partly deprived of their autonomy: logically as well as chronologically, they were to become subordinate to the wholeness of the organism. Müller renounced the analogy between cells and crystals, and his pupils Henle and Reichert, in defending the teleological viewpoint, argued that cells and cell development depended upon an organising and directing force that could not possibly be reduced to molecular interactions.

It is easy to see how artificial cells would have worked as arguments in this context. Not least they presented an irritation to the teleomechanist model of the organism – because they showed

[14] Lohff, Brigitte: "Johannes Müllers Rezeption der Zellenlehre in seinem 'Handbuch der Physiologie des Menschen'", in: Medizinhistorisches Journal 13 (1978), S. 247-258; Duchesneau: "Vitalism and Anti-Vitalism"; Lenoir: The strategy of life, pp. 141-153.

how structures that evidently bore a high resemblance to cells could in fact form outside of the organising whole of the organism; indeed, they even exhibited simple functional properties of cells.

One can identify three areas were artificial cells were employed in this way: First, as pertaining to the exogenous origin of cells; the idea, voiced by Schwann and Schleiden, that was, that cells formed in a homogenous, physico-chemical liquid. This, of course, opened the door to spontaneous generation – an implication that was duly exploited by the likes of Ludwig Büchner.[15] Second, as related to the continuous transition from crystals to cells, or from the inorganic to the organic: This argument was directed against the notion of an ontological gap between the living and the non-living and hence, against the very notion of a vital or organising force. And third, in connection with the phenomenon of osmosis: As this is the most important for the context of this workshop, I would like to dwell on it shortly.

Osmosis was discovered in France in the 1820s by Henri Dutrochet, presented as a solution to the problem of absorption in tissues.[16] Until Adolf Fick came to the fore with his ground-breaking investigations on diffusion in 1855, osmosis was conceived as a phenomenon that presupposed a membrane through which the exchange of liquids takes place. It became a most promising fields of research at any rate, and in particular for those biologists keen on accounting for physiological processes in terms of physical and chemical laws – physiologists such as Carl Ludwig and Ernst Brücke, for instance, directed their early efforts to this problem. The phenomenon of osmosis was fascinating not least because it promised proof that one of the central properties of tissues and cells, traditionally identified as a vital property, could be explained in physical terms: the cell's ability to select substances from its environment, to incorporate them, and to emit other substances from its body. Consequently, it was perceived as another blow against the teleomechanist and vitalistic approaches when Ascherson and other experimenters claimed that their artificial cells actually exhibited osmosis. These artificial structures, in other words, did not only imitate the morphological characteristics of cells, but also their physiological, functional properties. In fact, the consequences drawn from this even reached beyond a mere criticism of teleomechanism. Let me illustrate this with an example:

In his 1852 book Der Kreislauf des Lebens, the Dutch physiologist Jakob Moleschott presented a strict, materialist and mechanist world-view.[17] His main arguments concerned the ontological identity of matter and force as well as the indestructibility of matter. The result was a world where matter continuously circulated through the different realms of nature, from the earth to the organisms and back again. Stoffwechsel – metabolism – became the central property of living beings, and cells the main agents of metabolic functions. Cell formation, however, could be explained merely in terms of physical and chemical processes, and Moleschott referred to the haptogenic membrane of Ascherson when pushing for a purely mechanical account of the coming-into-being of cells.[18] What is more, metabolism itself, so Moleschott, could be explained by osmosis; the latter, as we know, was also exhibited by artificial membranes, and therefore was similarly accessible to physical and chemical explanations.

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[15] Büchner, Ludwig: Physiologische Bilder. Erster Band, Leipzig: Th. Thomas 1861, p. 233f.

[16] Cf. John V. Pickstone: "Absorption and osmosis: French physiology and physics in the early nineteenth century", in: Physiologist 20 (1977), S. 30-37; Paul S. Agutter, Malone, P. Colm, Wheatley, Denys N.: "Diffusion Theory in Biology: A Relic of Mechanistic Materialism", in: Journal of the History of Biology 33 (2000), S. 71-111.

[17] Jakob Moleschott: "Der Kreislauf des Lebens. Physiologische Antworten auf Liebig's Chemische Briefe", in: Dieter Wittich (Hg.), Schriften zum kleinbürgerlichen Materialismus in Deutschland. Erster Band, Berlin: Akademie-Verlag 1971.

[18] ibid., p. 61.

Moleschott here presented a grand schema of the universe which was, in defiance of its radical rhetoric, hardly devoid of metaphysical surplus. In this schema, cell membranes represented the relays connecting the organism with the outside world, the living with the non-living, and the organised with the disorganised. At the same time, they represented the agency of life. However, this agency was conceived of in terms of strict mechanical processes that could be imitated in the laboratory – as could be seen in the artificial cells of Ascherson and others. The "most secret and primordial machinery of metabolism",[19]as Moleschott called it, was constituted by a tiny barrier. This membrane established and managed all by itself – and without any recourse to vital forces or directing agents – a difference between the inside and the outside. It was this difference that defined an individual (the cell, the organism) and, at the same time, embedded it into the great whole of the cosmos, linking it to the perpetual flow of matter.

4. The Meaning of Membranes

What, then, was the meaning of membranes in the context of the story I have just told?

I have shown that artificial membranes were introduced as arguments against the teleomechanist takeover of cell theory. As presented by Ferdinand Ascherson, Pieter Harting and others, they functioned as performative and material arguments, a kind of proof-of-principle for the feasibility of a mechanical explanation of properties that were traditionally conceived as unique to organised and living beings.

Indeed, as we have seen in the case of Moleschott, they were able to transcend this use as local and specific arguments in a technical debate and turn into models embodying a whole world-view; in this case, the idea that metabolism is the key agent of life.

In both cases, artificial membranes were conceived as mechanical agents of exchange and control. They allowed modelling the agency of organic structures without recourse to vital forces or teleological principles. This feature points to a more general meaning of artificial cells: I would thus argue that such entities helped to make sense of life in a way that avoided the pitfalls of traditional mechanical models – that is, machines – without embracing a vitalist framework. In a way, they represented an idea of self-organisation avant la lettre. They offered a mechanism without machines, a mechanism that, in spite of everything that had been brought forward against mechanical models of life since the 17th century, could actually form itself. If the traditional argument against mechanical models involved pointing to the fact that machines do not reproduce, grow, or form by themselves, experimenters now could always refer to artificial cells: entities that, although evidently explainable by physical and chemical laws alone, nevertheless exhibited key features of life.

This, and with this last hint I will close my presentation, points to a tradition of mechanistic thinking and modelling that is much more clever than most accounts, contemporary as well as historical, suggest.[20]

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[19] ibid., p. 62.

[20] For another case study in which I elaborate on this idea, cf. Brandstetter, Thomas: "Leben im Modus des Als-Ob. Spielräume eines alternativen Mechanismus um 1900", in: Avanessian, Armen, Menninghaus, Winfried, Völker, Jan (Hg.), Vita Aesthetica. Szenarien ästhetischer Lebendigkeit, Zürich: diaphanes 2009, S. 237-249.

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Surface Politics and the Transformation of Waste into Energy

Lisa M. Cockburn

> *If some might object that the book deals too much with mere appearances, with the surface of things, and fails to engage and reveal the patterns of unifying relationships that many believe form the true and underlying reality of existence, I can only reply that I am content with surfaces, with appearances. I know nothing about underlying reality, having never encountered any….* Edward Abbey, *Desert Solitaire*, preface to the 1998 edition.

Introduction

Common wisdom tells us not to judge a book by its cover, as it is what is beneath the surface that matters most. Yet when it comes to relations and interactions and everyday experience, the surface may be all that is known to the other. Through an exploration of the relational politics of surfaces that occur at various levels of bioenergy production in the transformation of waste into energy, I will argue that surfaces are political because they are sites of relation, and relations are always political. The forms of politics exercised at the surfaces of things often differ from typical conceptions of human politics. Yet these more-than-human politics matter a great deal, as it is through them that reality emerges. In transforming municipal waste into useable forms of energy, surfaces are generative both conceptually and physically, on scales ranging from the micro to the macro, from microbial relations to societal and planetary politics.

In developing what Latour (2005) has termed a "more-than-human" sociology that examines the relations among all actors, human and otherwise, with equal interest, I aim to find ways of imagining and understanding the nonhuman world beyond the human-nonhuman dualism. I argue that reality exceeds the conflation of the infinite complexity of the universe that is other-than-human into a category that means little more than "not us": the "nonhuman". In so doing, I join a growing trend across the social sciences and humanities toward accounting for the material world when studying the social (e.g. Whatmore 2002; Murphy 2004; Haraway 2008; Cerulo 2009; Bennett 2010). In his seminal 1993 work We Have Never Been Modern, Bruno Latour argues that the modern divide of the human/cultural from the natural/nonhuman in fact misapprehends the reality of the world, in which human and physical systems are inseparably entangled. In this paper I draw mainly from Karen Barad's agential realism (Barad 2003; Barad 2007) and Graham Harman's reading of Latour's actor-network-theory (Harman 2009) to discuss how surfaces matter in the creation, understanding and transformation of municipal waste into usable forms of energy. Although Barad, Latour and Harman do not specifically discuss surfaces in this manner, my aim is to show how surfaces function in their work as intensely and even ontologically political zones, where things come into being through their surface intra-actions and relations. In discussing bioenergy, I begin with a brief foray into the metaphysical significance of surfaces. I then discuss some key material and biological surfaces in waste and bioenergy production from waste. Different types of relations occur at these surfaces, and I end with some broader political implications of engaging in these relations.

Metaphysical Surfaces

As Latour observes, "nothing can be reduced to anything else, nothing can be deduced from anything else, everything may be allied to everything else" (1988: 163). In other words, although nothing is entirely explainable by a single cause, at the same time there is always the potential for mediation and translation. Tackling a similar paradox, Harman argues that although "a thing's reality is dependent on its relations with other things", at the same time "objects are irreducible to their relations with other things" (2009: 187), in that objects are "never replaceable by any sum total of qualities or effects" (Harman 2009: 188). Connection and interdependence abound, but nothing is determinate. Philosophers of biology Dupré and O'Malley (2009) note that membranes play an important role not only in the origin and maintenance of life but also function as boundaries in both theoretical and experimental biological science. However, this "epistemological necessity" does not require that these boundaries be "uniquely and unequivocally identifiable" (Dupré and O'Malley 2009: 25). Indeterminacy is also a key concept in Barad's agential realism. In a classic thought experiment from quantum physics, the position and momentum of an electron cannot be measured simultaneously, and accuracy in measuring one is inversely dependent on the other (Barad 2007: 112). Although this problem has typically been understood as one of uncertainty, a limit to what can be known (popularly known as the Heisenberg Uncertainty Principle), Barad identifies it as one of indeterminacy, in that "the values of complementary variables (such as position and momentum) are not simultaneously determinate" (Barad 2007: 118). Indeterminacy refers not to an epistemic limitation as uncertainty does, but to an ontological one, an inherent property of the world. Not just the unknown, this is the unknowable, or even "what can be said to simultaneously exist" (Barad 2007: 118). While uncertainty is ultimately human-centric, concerned with what we do or do not know, indeterminacy is posthuman; it is not about humans or nonhumans, but about something beyond, from which these distinctions arise.

In an indeterminate world, surfaces are where differences are articulated, emerging through relations. Agency is what underlies all existence, and relations themselves are primary to that which relates (Barad 2007: 136). Thus, the lines between subject-object and cause-effect come forth through, not before, "*intra*-action" that produces phenomena, the primary epistemological (and, ontological) unit (Barad 2003: 815). Similarly for Latour, "actors are events" (Harman 2009: 47) akin to phenomena, whereby "an actant is not a privileged inner kernel encrusted with peripheral accidents and relations" (Harman 2009: 14) but rather exists because of its relations. The phenomenon includes the object, subject, and the "apparatus" that resolves the ambiguity between the two by providing the "conditions of possibility and impossibility of mattering" (Barad 2007: 148). The apparatus enacts a local and contingent "agential separation" of object and subject that Barad terms an "agential cut" (2007: 148). What 'is', is not determined until specific agential cuts are made. While an apparatus is most obviously understood as the physical equipment necessary to measure or produce a certain phenomenon (such as anaerobic bioreactors), it can also include the role of scientists and technicians, social relations, and even extend to the theories and beliefs that underlie the choice to (intra)act in a specific way. Accordingly, science creates knowledge, not through (exclusively human) social construction, but through an ongoing dance of human and nonhuman agency (Pickering 1995). Objectivity is possible in agential realism through careful description of the apparatus: exactly what is done to draw the boundaries by and about which knowledge is obtained.

It is therefore through agential cuts that surfaces are collectively determined. A surface cannot exist in isolation, but must border something else in order to be ontologically meaningful. In fact, material objects could be said to only exist because of surfaces that act to distinguish and bound their existence. Yet key to Harman's object-oriented philosophy is the assertion that nothing can ever come directly into contact, and that objects "always hold something in reserve" from the relations in which they are involved (2009: 187).

> Assuming that the real tree and I are able to enter into some sort of genuine relation, we form a new integral reality that is something over and above both of us, and also something deeper than any external view that might be taken on that reality. And that is what 'object' really means, despite the usual prejudice that objects must be physical entities or at least relatively durable (Harman 2009: 211).

This is similar to Barad's "phenomena", whereby each intra-action forms a new object. Each object comprises the history of objects by which it was formed, creating "a chain of objects descending without limit, each of them with a molten internal space where new encounters can arise" (Harman 2009: 211). At the core of any object-relation is agency. Yet always these objects are necessarily defined by their surfaces, their interior remaining mysterious. Surfaces are a site of emergence, articulation and demarcation of objects; they intra-act their distinction, and traces are left as lasting objects. Surfaces are therefore both indeterminate and subjective – different things will find different surfaces meaningful depending on the relations – and the resulting agential cuts – that they are caught up in and created by. Different agential cuts will create different surfaces. The subjective nature of surfaces can be understood as there being "no absolute inside or absolute outside" but only "exteriority within, that is, agential separability" (Barad 2007: 377). For instance, at the micro-ontological scale many fruitful insights emerge and challenge our basic anthropocentric notions of reality: from the indeterminacy and wave-particle duality of the quantum level (Barad 2007) to the boundary-defying tendencies of bacteria, whose exchange of genetic material even between seemingly disparate groups calls into question the relevance of the very concept of species (O'Malley and Dupré 2007). Boundaries that appear fixed and unquestionable at one ontological scale may disappear, blur, shift or become permeable at another, as different apparatuses produce different agential cuts. Boundaries delineate not definite separation but rather concentrations of difference made knowable through intra-action; surfaces are the manifestation of these local distinctions. This is why it is so important to clearly articulate the apparatus used in knowledge-making, and as far as we are able, be "response-able" for the agential cuts we make (Barad 2007).

Surfaces, Waste and Energy

In understanding waste and bioenergy, surfaces act as sites of local articulation and mediation between useful and problematic, living and nonliving, material and immaterial, matter and energy. 'Waste' is only waste when it is demarcated as useless, spent, or undesired. This means that what is waste today can become a valuable resource tomorrow, as increasingly seen in waste-to-energy production systems. Waste and energy are more closely related than one might first suspect. Of course energy is not an object in the classical sense: it has no material surfaces. Yet it too comes into onto-epistemological existence through intra-action, and it is surfaces that are the site of the transformation, release or capture of energy; energy is experienced and understood through its effects on matter. Working on the surfaces of things, energy has material effects, transforms matter and conveys meaning. Bioenergy is energy stored in the chemical bonds of recently synthesized organic molecules (thus differentiating it from energy derived from the organic bonds in petrochemicals). Household waste is already teeming with microbes busy metabolizing its carbohydrates, proteins and lipids, using the released energy for growth while producing their own waste products that in turn act as necessary input for other organisms in the collaborative metabolism of biomass. Because the status of waste is indeterminate, I argue it can be thought of as what Susan Leigh Star (2010) terms a "boundary object." Here the meaning of boundary is not "edge or periphery" but rather "a shared space, where exactly that sense of here and there are confounded" (Star 2010: 602-603). The materiality of boundary objects is derived from action; moving between different zones or groups, they are "something people act toward and with" (Star 2010: 603). In Canada, poorly managed waste biomass accounts for 10% of greenhouse gas emissions yet holds the potential of supplying 22% of the country's energy needs through conversion to bioenergy (Levin et al. 2007). This same cycling of matter forms the basis of ecosystems and nutrient cycles; life itself can be thought of as a planetary system of constantly recycling matter, of which humans are part whether we act the part or not. Bioenergy can be produced by microorganisms in various ways. Two types of bioenergy production I will focus on are biogas – methane production from methanogenic microbial communities (Appels et al. 2008) – and cellulosic ethanol production through fermentation by yeasts or bacteria (Lynd et al. 2002).

As mentioned above, methanogenic communities are found all around us, already present in the organic matter of our garbage. Given the appropriate anaerobic and moisture conditions, these microbial communities will metabolize most organic compounds to methane, leaving behind matter high in mineral nutrients that can be used as fertilizer. Methanogenesis occurs through syntrophic metabolism, a tightly coupled mutualistic interaction in which organisms of one trophic group utilize the metabolites and waste products of another (Schink 1997). Anaerobic environments are energy-limited, meaning the availability of reducing equivalents (mainly H_2) depends on the bioenergetic yield of non-oxidative catabolism such as fermentation and methanogenesis (Lynd et al. 2002). Complex organic matter is therefore converted to methane through multiple stages performed by different groups of microbes: hydrolysis, fermentation, acetogenesis, and finally methanogenesis. As syntrophic metabolism can occur only with energy input, metabolism effectively becomes shared between multiple organisms that must function together in order to consume a single food source. This provides a particularly good example of Dupré and O'Malley's (2009, 13) argument that "metabolism is typically a collaborative activity" of multiple entities. Dupré and O'Malley present a revised concept of life in which combined lineage and metabolic functions must overlap but need not be discretely bound within what we would conventionally call an organism (Dupré and O'Malley 2009). This relates to the idea of surfaces as boundary zones in a direct way: "the boundaries of a plant and animal are precisely the sites where complex interactions occur between entities generally considered distinct" (Dupré and O'Malley 2009, 13). In other words, it is on surfaces that life functions, through intra-action between matter; different intra-actions result in agential cuts that can create different 'organisms'.

Because syntrophic populations rely on each other for required inputs or disposal of waste products, they are usually found in close proximity, even physically attached in biofilms or macroscopic granules. These communities of microbes spatially organize themselves relative to the organic matter on which they are feeding to create a productive surface at which methanogenesis can occur more effectively. Granulation occurs in liquid substrates through the self-immobilization and aggregation of organisms from multiple trophic groups into 0.5–2 mm diameter granules (O'Reilly et al. 2009). Although granulation is an essential ecological process in many industrial bioreactors, it remains poorly understood. The growth of granules in liquid is thought to proceed similarly to the formation of biofilms on solid surfaces, but no consensus exists as to the mechanism triggering granule formation. It is currently explained by various theories including: i) physical theories based on flow velocity, suspended solids and inert seed particles; ii) thermodynamic mechanisms involving physicochemical interactions between cell walls, substrate surfaces and the energetics of adhesion; or iii) microbial theories, which may be based on physiology, growth, or ecology, for example the formation of multi-layered syntrophic granules in which methanogens form a nucleus surrounded by acetogenic and acidogenic microbial layers (Hulshoff et al. 2004).

The spatial and intra-active organization of the syntrophic microbes involved in methanogenesis illustrates the importance of surfaces in bioenergy production.

Fermentation typically involves simple sugars and starches, but the majority of human waste is composed of plant cell walls (Bayer et al. 2007). Besides providing plants with their structure, the multiple protected surfaces of plant cells, composed of cellulose wrapped in hemicellulose and lignin, resist intra-action with bioenergy-producing microbes in various ways. Cellulose poses little challenge to biogas production, as methanogenic microbial communities include organisms capable of cellulose metabolism. In contrast, cellulose is a major challenge in the production of ethanol, as organisms capable of hydrolyzing cellulose are not typically the same organisms that ferment alcohol (Bayer et al. 2007). Various chemical, physical and biological pretreatments are used on waste feedstocks to increase microbial access to cellulose surfaces by breaking open cell walls and removing lignin (Sun and Cheng 2002; Stephanopoulos 2007). After pretreatment, extracellular enzymes called cellulases extracted from fungi are used to hydrolyze hemicellulose and cellulose into five- and six-carbon sugars (Wackett 2008). Traditionally, hydrolysis is completed prior to and separately from fermentation. Consolidated bioprocessing aims to combine cellulase production, hydrolysis and fermentation within a single cell; this can be accomplished either through strain selection to improve ethanol yield of native cellulolytic organisms or by genetically engineering cellulose-utilization pathways into an organism with high ethanol yield such as yeasts (Lynd et al. 2005). An important development in cellulosic ethanol production involves the use of cellulosomes, enzymatic nanofactories found on the surface of the cells of some bacteria. Cellulosomes consist of cellulases, carbohydrate binding domains, and structural dockerins and cohesins (Bayer 2004). Researchers aiming for consolidated bioprocessing are currently looking for ways of genetically engineering cellulosomes into strains of fermenting yeasts (Bayer et al. 2007).

Relating at the Surface

Various types of intra-relation can take place on surfaces. Surface politics range from control to flow, unilateral to reciprocal, imposition to collaboration. In the intra-actions that generate life, collaboration includes a continuum of cooperative and competitive activities (Dupré and O'Malley 2009), providing a useful framework for understanding intra-actions between entities. Using the concept of collaboration as a starting point, two distinct approaches are evident within bioenergy production, which seem to reflect or at least reinforce very different relationships with nature (Figure 1). What I call the organism approach uses various forms of genetic modification, focusing on single-species isolation and manipulation. The organism approach currently dominates research on cellulosic ethanol, seen for example in cellulosome engineering. This includes strain selection and genetic engineering as well as evolutionary and physiological engineering. Evolutionary engineering is a form of forced evolution, where specific genes are targeted for induced selection pressure towards a desired phenotype (Koffas and del Cardayre 2005). Physiological engineering is a systems biology approach that extends conventional genetic engineering to include various metabolic and cellular functions (Zhang et al. 2009). However, these approaches struggle with genetic instability of engineered strains, and loss of beneficial population and community effects.

The organism approach suffers from the limitations of the small genome size of microbial collaborators, which may ultimately prove prohibitory to a single species producing all the enzymes required to break the various chemical bonds in plant cell walls and ferment the resulting sugars (Maheshwari 2008).

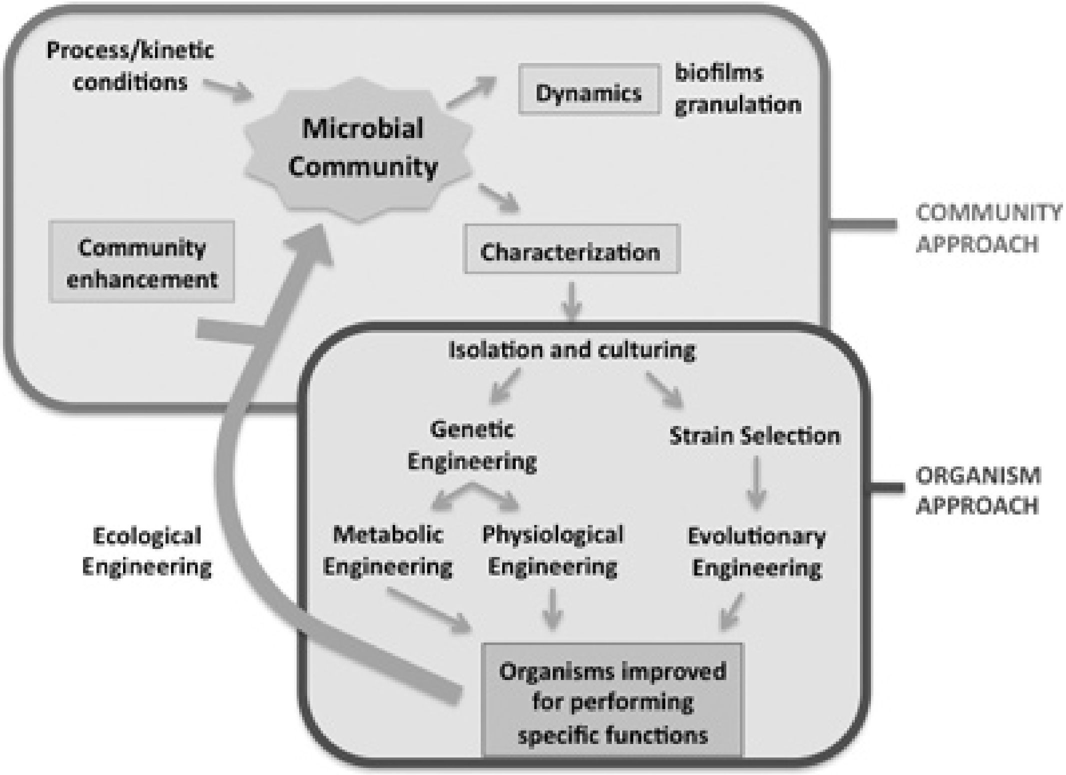

Figure 1: Organism and Community Approaches to Improving Bioenergy Production. The upper box shows the community approach and the lower box the engineered-organism approach. The organism approach may be incorporated into the community approach through ecological engineering. However, the organism approach may be pursued independently of the community approach, and the community approach need not include the organism approach, and may in fact resist it. The community approach regulates production either by modifying conditions or working with community structure and dynamics. Cellulosic ethanol production can proceed using either the engineered-organism approach or take a community approach using biofilms. Methane production requires a mixed-organism community approach but can incorporate elements of the engineered-organism approach through community enhancement. Community enhancement can involve either engineered or unmodified organisms.

Furthermore, the more heterogeneous the substrate, the more challenging it will be to engineer a single organism (Kleerebezem and van Loosdrecht 2007); this is significant to waste-to-energy initiatives as municipal solid waste is a highly heterogenous substrate. These difficulties have led to increased interest in ethanol fermentation using mixed-organism cultures, representing a potential shift toward the community approach. An alternative to consolidated bioprocessing in a single cell is to consolidate all the steps temporally, if not microbially, in a single community capable of performing all steps. Biofilms are spatially-organized multi-species communities of microbes in which metabolism is shared collaboratively. Biofilm cultures are a more efficient way of achieving cellulose hydrolysis. This is evidenced by the shorter retention time and ten-fold higher efficiency of digestion accomplished in the gut of rumens, compared with a conventional commercial digester. Biofilms are surfaces that are productive, exhibiting higher ethanol tolerances and yields, increased reaction rates, and allowing a more efficient continuous system that includes separation and extraction in one step (Wang and Chen 2009). The confined microenvironment of the biofilm mediates inhibition, promotes enzyme–microbe synergy (Lu et al. 2006), and selectively rewards cells with better phenotypes (Wang and Chen 2009). Thus, moving to the community approach could lead to more robust, energy efficient, cost-effective and versatile ethanol production systems.

The community approach is characterized by mixed-species systems in which principles of ecology come into play. It allows for collaboration between organisms and at least potentially accepts indeterminacy and flow. However, whether the collaborative character of the community approach extends beyond the microorganism level to include human collaborators remains debatable. Because complex organic matter is converted to methane through multiple stages performed by at least three distinct trophic levels of microbes, optimization of methane production has necessarily followed the community approach, although a push toward greater control, increasingly incorporating elements of the organism approach, is evident. Although much is known about the basic chemical kinetics of microbial methane production, less is known about the actual microbes responsible for these processes (Weiland 2010). Thus, biogas production is usually regulated through temperature, mixing and pH of the bioreactor, rather than modifications of organism metabolism (Appels et al. 2008). Community characterization using various DNA screening methods is a major focus of current research, and attempts to isolate specific microbes often follow. Syntrophic organisms are understandably difficult to grow in isolation, but this can sometimes be overcome through co-culturing with an appropriate syntrophic partner. If a suitable candidate organism for a desired function can be targeted and cultured, it may then be added to a bioreactor to increase biogas production. Organism addition may, however, alter community dynamics in unexpected ways. This has lead some biologists to call for ecological engineering: interdisciplinary cooperation between ecology, engineering and microbiology, and the transformation of microbial ecology from a descriptive into a quantitative predictive discipline (McMahon et al. 2007). Here, overlap between the community and organism approaches becomes evident.

Surface Politics

The question of whether our microbial relations are rooted in cooperation or control is relevant on scales larger than that of the scientific laboratory. Society tends to model human relations and justify human behaviours based on what we think we see in 'nature' (Everndon 1992). When Charles Darwin's influential *On the Origin of Species* was first published, Karl Marx noted in a personal letter to Friedrich Engels: "It is remarkable how Darwin recognizes among beast and plants his English society" (as quoted in Bell 1998: 222). In other words, human characterizations of what is 'natural' may provide a better reflection of current societal norms than an accurate depiction of what the rest of existence is doing. As Engels replied to Marx, "the whole Darwinist teaching of the struggle for existence is simply a transference from society to nature...the same theories are [then] transferred back again from organic nature into history and it is now claimed that their validity as eternal laws of human society has been proven" (as quoted in Bell 1998: 223). This dialogue between society, science and nonhuman nature deserves critical attention, as there are many parallels between the prevailing societal worldview and prominent biological theories. Competition and predation continue to dominate ecological understandings of community dynamics today, weighting them heavily toward the negative. Yet for over 40 years some ecologists have suggested that positive interactions may be much more important in ecosystems than is broadly recognized (Vandermeer 1980; Ulanowicz and Puccia 1990). In mathematical ecosystem modelling, network synergism emerges when the net sum of all parts of a model is positive; this means the net result of any biological system from the cellular to the ecosystem level is ultimately synergistic, regardless of the size or complexity of the system (Fath and Patten 1998). This agrees with ecologist David Sloan Wilson's (1976) conjecture that in a spatially heterogeneous environment, the effects of individuals on their community will eventually feed back, leading to selection for positive interactions such as mutualisms. Still, negative interactions remain the focus of most ecological theory.

In microbiology, however, the difference in scale makes community relations and mutualisms easier to detect and study, and a great deal of research has been done on various microbial symbionts, mutualisms, and cooperation (Hird 2009). By starting from the microbial up, we may be able to rethink and rework the politics of our surface relations. Although competition has been the focus of most evolutionary, particularly neo-Darwinian, theory (epitomized by Richard Dawkins' selfish gene hypothesis), alternative theories emphasizing cooperation and shared interests over selfishness provide evidence for social selection and symbiogenic evolution (Margulis 1993). Evolutionary biologist Joan Roughgarden argues against the conflation of evolutionary success with selfishness, challenging not only current evolutionary theory but also the very core of the "scientific validity of a world view that naturalizes selfishness" (2009: 4).

Staying at the Surface of Things

I end with some tentative conclusions that I hope will open up exploratory space in the interstices of energy, humans, waste, microbes and technology. Surfaces are in many ways, both materially and discursively, the site of creation and transformation of waste and energy. If bioenergy production effectively engages humans in a collaborative relationship with existing collaborations of microbes, the pressing question is, how do scientists and consumers of bioenergy engage and interact with their microbial collaborators? Does the collaboration lean toward cooperation or control? Joining Barad in her use of the double meaning of "matter", how we think about waste and energy matters a great deal. How we intra-act at the surface is deeply important both ethically and ontologically, for it is through these intra-actions that reality emerges from potential. Beyond the obvious surface effects on municipal waste management, how we relate with our waste may have ripple effects that spread to other human-nonhuman relations. How do different waste management practices materialize and stabilize broader human-nature relations? What models of the future are embedded within the transformation of waste into energy? Other technologies, which I have not discussed here, further complicate this picture: these include thermal gasification of either organic or inorganic matter into syngas, a mixture of carbon monoxide and hydrogen which can then be transformed into various biofuels. The juxtaposition of alternative technologies by which we might transform our waste into energy, one – bioenergy production – relying on life and the other thermally circumventing it, invites further questioning into what the role of life in biotechnologies is, and whether it draws us into relations that tend toward flow or control. Our waste management intra-actions also physically create new entities through intra-action, articulating the agency of the universe in the participatory emergence of phenomena. Methane, ethanol, syngas, fertilizer, as well as bioreactors, new strains of microbes and multiple other object-actors, all come into being through these intra-actions. Our world is constantly being created through the intra-actions of multiple agencies, human and otherwise. Waste shifts, both ontologically and materially, from a buried and potentially environmentally toxic mess to an input into living systems of nutrient cycling. How local surface relations between municipal waste, scientific laboratories, human actors, bioenergy-producing microbes, and waste management technologies form and transform networks of waste and energy at multiple scales does matter. It matters to how human society imagines the human-nonhuman relationship, and intra-acts in the politics of the more-than-human community.

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From the Atomic Age to Anti-Aging: Shaping the Surface between the Environment and the Organism

Alexander von Schwerin

In 2006, the cosmetic brand Nivea launched a new product series which came with both, a most striking name and appearance: "DNAge" was surely a peculiar name for a cosmetic product; and the iconic DNA double helix, conspicuous on DNAge's packaging and in the advertisement campaign accompanying the introduction of the series, reminiscent of the exciting press and radio coverage at the time of the international Genome Project and the human DNA, was clearly calculated to remind potential customers of its profoundly scientific character (see figure 1).

Brand name and the icon chosen for the DNAge product series indeed were not coincidental, as was impressed to the some 250 "beauty journalists" who joined the international press conference for the launch of "Nivea Visage DNAge" in May 2006. The opening gala centered on "the wishes and needs of women over 40." However, the important people of the day were Beiersdorf's scientists. They entered the limelight presenting their new results on human skin research after which Nivea Visage DNAge® was finally revealed to the public – as a "revolution" and the "first product of its kind worldwide."[1] Nivea announced its intent to showcase a new type of beauty care. The active agents of DNAge would not only strengthen the skin's structure, but also prove to be effective on the level of molecular regulation. Lest there be any doubt, the suffix "age" in the name "DNAge" would usefully bring home to beauty house clients that this new item was an anti-aging product.

In 2006, perhaps none of this came as a great surprise. Nivea, for one, had long been the most prestigious trademark of the Beiersdorf company: headquartered in Hamburg, it is one of the smaller German chemical companies, with an annual turnover of some 5.8 billion Euros (a bit more, for comparison, than the tenth of the turnover of the chemical concern BASF).[2] Beiersdorf is specialized in cosmetics in general, and in skin care in particular. As with most cosmetic companies, biomedical research has become increasingly important to Beiersdorf's strategy in the past few years. Thus, the so-called Skin Research Center was opened in 2004, prominently featuring a Laboratory for Molecular Biology. The molecular laboratory in turn was crucial in the development of the new crème's formula. Most anti-aging products target structural changes in the connective tissue. Not so Beiersdorf. Beiersdorf researchers recognized a different threat to the vitality of the skin: the alteration of the structure and molecular conformation of the DNA. As Beiersdorf's Head of Product Development, Dr. Martin Rudolph explains: "We know that damage, through UV light for example, can penetrate to [sic] the DNA and contribute to skin aging."[3] And that, of course, is why the Beiersdorf molecular biologists sought to deliver effective agents to the skin cells which would then intervene in the skin's molecular processes, ostensibly preventing aging processes at its source. Or this is what DNAge claims to do: to activate the capacities of the skin to resist damaging environmental influences.

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1 Beiersdorf Aktiengesellschaft, Corporate Identity & Information (ed.), Annual Report 2006, Hamburg 2007, p. 34.

2 IKB Branchenbericht, Chemische Industrie, April 2010, p. 8.

3 Martin Rudolph, We worked to achieve a milestone right from the start, in: Beiersdorf Aktiengesellschaft, Corporate Identity & Information (ed.), Annual Report 2006, Hamburg 2007, p. 34.

Fig. 1: A new type of beauty care “works where skin-aging starts: at the DNA.” The protection of the DNA becomes the aim of anti-aging luxuries such as crèmes for face care.
Source: Nivea print advertisement from 2006.

The project was, accordingly, identified as “important” at an early stage: “We worked to achieve a milestone right from the start.”[4] At any rate, it was not long until Beiersdorf’s molecular biologists got promising results, tests showing that folic acid – also known as vitamin B9 – significantly influences the ability of skin cell nuclei to repair themselves. On the way to DNAge, this was only

[4] Ebd.

a first, if major, step. Because folic acid is extremely sensitive to heat and light, and because it is also water-soluble, the main difficulty was to actually develop a stable cream. But this, of course, was a problem to be now handed over to the colleagues at the department for product development.

What interests me here though, is the other, first step (or more precisely, its historical circumstances): The short story of DNAge indeed turns longer and rather more interesting if one asks how the Beiersdorf researchers were able to identify an anti-aging active substance in such a short time. The search for answers, as we shall see, turns the story of DNAge into a story of science and politics beyond wrinkles; a story to be found in the folds of modern biopolitics and body concepts – concepts of bodily boundaries. In the following I shall roll the tape back to the "Atomic Age" therefore, an era in which the skin was clearly not a molecular and active surface. From there, I will move forward in time to trace the genealogy of DNAge's activation strategy. Not least, this will illuminate the kind of activation DNAge offers: the enhancement of DNA repair mechanisms. This latter scientific object, I shall argue, grounds the story of DNAge in the period from the 1960s onward, when the molecularization of biological knowledge changed ideas of how the environment can cause distress to the body and its genetic constitution. It was at this time as well that a new species of researchers was born at the cross-roads of radiation biology, genetics, toxicology and molecular biology, eager to try out new ways of enhancing the body: "repairologists" (Friedberg).[5] First, however, a few more remarks on Beiersdorf's target, or matter of concern, are in order: genetic mutations and their significance for the body and the history of its surface.

1. Intersections of the Environment and the Organism

Worries about damage to human DNA increased with the growing economic prosperity and technical progress in the "Golden Age" (Hobsbawn) of the welfare states, especially in the 1950s to the 1970s. Then, nuclear technologies and the oil-based, chemical industries catering the age of mass consumption multiplied the number of agents circulating in the human environment, even if nobody knew much about their toxic impact. Environmentalism since the 1950s came along with a growing awareness however that many of these substances where mutagenic and/or carcinogenic.

The history of mutations not least highlights the specific problems involved in the regulation of toxic substances. Science and politics had to deal with a changing environment that changed the conditions of human life. Were worker's bodies able to cope with inhaling toxic vapor? To what degrees could it be tolerated when a normal white male body came into contact with pesticides that were, and still are, ubiquitously used? Were there synthetic food additives that had to be considered toxic? And if so, how much of it? Consequently, scientists, politicians and environmental activists – all those who became enrolled, or enrolled themselves, in the regulation of the pitfalls of technical progress – had to deal with this increasingly precarious relation between the environment and citizens' bodies.

Hence, it was not the body as such but a relationship that determined the new problems of risk politics. Body history has lucidly described the inscription of the social order and values into concepts of the body, or changing body metaphors as changing ideas about health, identity etc. The case of risk politics shows that it is not always, or in the first instance, the idea of the body in isolation that is at stake in practice: it is certainly a connection that has become precarious, and most obviously perhaps in the case of the scientific evaluation of toxic substances and political

[5] Errol C. Friedberg, The discovery that xeroderma pigmentosum (XP) results from defective nucleotide excision repair, *DNA Repair (Amst)* 3 (2004), pp. 183, 195, here: p. 195.

considerations on regulative measurements. There are no bodies when it comes to these practical problems of science, as we shall see, only relations between bodies and environments.

Hygiene and preventive politics naturally have to rely on knowledge of the bodies endangered as well as its endangering environs. That is why knowledge that deals with the relationship between the body and the environment – a type of knowledge linking the body's autonomy and vulnerability with the influences of the environment – is at the same time scientific and political. Moreover, the hazards and risks at stake themselves are a matter of contestation and negotiation – as are the concepts brought to bear on constructing a given problem in terms of dangerousness, urgency, social impact, and regulation.

The epistemic reconfiguration of the relation of environment and body, then, is one crucial moment in the emergence of a new problematization of social problems. The regulatory knowledge that is implied of course is not something that is "just" discovered but is the outcome of a historical process within which this very relation between body and environment was problematized.[6] Politics and risk episteme mobilize each other.[7] Mutation research is a case in point: at stake were the biological and biomedical concepts of how the organism relates to environmental hazards. Traditionally, this was a sub-area at best of research in classical genetics. Crucial to the following story is the point when mutation research met up with molecular biology in the late 1960s. When researchers identified DNA repair mechanisms in the 1960s, they opened the way to a new interpretation of mutagenesis in biological and molecular terms.[8] This became the starting point for a crucial reconfiguration of the models of mutagenesis; that is, for how the environment was seen to act upon the biological substance of the organism (thereby inducing mutations). The organism's surface – the skin – accordingly was at the center of this toxicogenetic reconfiguration of the environmental body. It provided the epistemic conjunction to negotiate the relation between the human body and the environment. The re-shaping of what was at the time a skinny boundary regime may then also point to a general reconfiguration in the problematization of the body's capacities to draw boundaries.

2. The Tortoise Strategy of the "Atomic Age"

This story of human skin as that of a remarkably thin surface with remarkable capacities begins, in many respects, in the 1950s. This, the proverbial Atomic Age, was a time when many were indeed thrilled by the prospects of nuclear technology. Countries all over the world aspired to adopt atomic energy as an innovative key technology to modernize energy production, industrial production generally, and not least, medicine and biomedical research. For much the same reasons,

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6 For regulatory knowledge (Regulierungswissen) see Beat Bächi, Carsten Reinhardt, Einleitung: Zur Geschichte des Regulierungswissens. Grenzen der Erkenntnis und Möglichkeiten des Handelns, *Berichte zur Wissenschaftsgeschichte* 33 (2010), pp. 347-350.

7 For risk episteme see Alexander von Schwerin, Low Dose Intoxication and a Crisis of Regulatory Models. Chemical Mutagens in the Deutsche Forschungsgemeinschaft (DFG), 1963–1973, *Berichte zur Wissenschaftsgeschichte* 33 (2010), pp. 401-418.

8 Actually, the history of the DNA repair begins earlier, but I will refer here only to one important point when DNA repair emerged as new well-defined research field between (molecular) mutation research and toxicogenetics. For further historical readings see Doogab Yi, The coming of reversibility: The discovery of DNA repair between the atomic age and the information age, *Historical Studies in the Physical and Biological Sciences* 37 (2007), pp. 35-72, also from a scientist's perspective R. F. Kimball, The development of ideas about the effect of DNA repair on the induction of gene mutations and chromosomal aberrations by radiation and by chemicals, *Mutation research* 186 (1987), pp. 1-34; Errol C. Friedberg, *Correcting the Blueprint of Life. An historical Account of the Discovery of DNA Repair Mechanisms,* Cold Spring Harbor 1997.

it was an era of fear; a time when above-ground atomic testing became omnipresent in the news and rumours spread that atomic fallout might travel from far-away testing grounds to rain down onto distant cities or the regions meant to provide the population's food supply. Clearly, here was an environmental threat, invisible and man-made, that came through the air: soundless, ubiquitous, undetectable by unaided, human senses, but with features making it fatal in terms of individual health (and collective consequences). No wonder that the radioactive threat became the object of professional investigation, drawing together biophysicists, physicians, radiation biologists, and geneticists. It was in their hands that radioactive pollution re-defined the environment as an object of regulation and risk politics.

From the start, aspects of this early environmental debate focused on the genetic effects of radiation. It was well known that radiation easily penetrates the body, passing through human cells and causing genetic damage. Geneticists increasingly drew attention to the mutagenic effects of radiation on germ cells and pointed out that the detrimental genetic effects would not show up in the harmed individuals but in the next generation when mutated germ cells became the origin of new life. Indeed, the fears of the "Atomic Age" tended to focus on these problems in terms of generational effects, broadly construing radioactive hazards as a dysgenic problem. Politics had to contend with the daunting prospect of a society facing degeneration on a truly massive scale.

There are quite a number of works that have described this research on radiation biology and genetics, the role of scientists in the efforts to objectify the radioactive danger or the regulatory measures that were being drawn up for radiation protection.[9] But my question here is different, I'm interested in how the worries about radioactive effects were related to concepts of the biological organism. How did the biological model of hazardous effects guide the debate and rationale of radiation protection?

Put simply, the model of the Atomic Age was radiation as a bullet – a bullet hitting the cell and inducing damage in the genetic make-up. To be sure, no geneticist or biophysicist really spoke of "bullets," but the genetic model of radiation-induced mutations came close to this idea as it was formulated in physical terms. Following that model, the radiation waves penetrated cells, induced changes in cell structures that – in accordance with the principles of quantum physics – contingently resulted in changes of the conformation of genes and chromosomes. Because this model assumed very focal initial changes at the target site of the cell structure, biophysicists called it target concept or target theory. The target model was based on terms and concepts that (famously) dated back to the 1920s, when biophysicists first entered the field of biological radiation research which they had dominated ever since.[10]

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9 For a selection, see Robert W. Seidel, A home for big science: The Atomic Energy Commission's laboratory system, *Historical Studies in the Physical and Biological Sciences* 16 (1986), pp. 135-175; John Beatty, Genetics in the Atomic Age: The Atomic Bomb Casualty Commission, 1947-1956, in: Keith R. Benson, Jane Maienschein and Ronald Rainger (Hrsg.), *The Expansion of American Biology*, New Brunswick, London 1991, S. 284-324; J. Samuel Walker, The Atomic Energy Commission and the Politics of Radiation Protection, 1967-1971, *ISIS* 85 (1994), pp. 57-78; Karen A. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955,* Princeton 2004; Angela N. H. Creager, Maria Jesus Santesmases, Radiobiology in the Atomic Age: Changing Research Practices and Policies in Comparative Perspective, *Journal of the History of Biology* 39 (2006), S. 637-647.

10 There is some work on the history of the target theory, but no one on the implication for body concepts. Alexander von Schwerin, Der gefährdete Organismus. Biologie und Regierung der Gefahren am Übergang vom „Atomzeitalter" zur Umweltpolitik (1950-1970), in: Florence Vienne and Christina Brandt (eds.), *Wissensobjekt Mensch. Humanwissenschaftliche Praktiken im 20. Jahrhundert,* Berlin 2009, pp. 187-214, here: pp. 195-198.

The physical model of mutagenesis evoked a clear idea of the role of the penetrated biological substance: it was passive. The biological organism in turn appeared to be at the mercy of natural laws. Whether the radiation changed genetic cell structures or not was only a matter of probabilistic laws of quantum mechanics. It was a model, in other words, of external forces, violence inflicted and brutal damages. It victimized the cell and the body: an external agent passed easily through the body's surface, penetrated the cells and hit their very heart, the (now) DNA. In this schema, a weak, almost naked, body emerged as the passive object of environmental effects.

Since genetic damage was not reversible, the helplessness of the body in fact was even more dramatic than that. The traditional laws of toxicology had implied that the body could recover from (minor) physiological effects. The message of genetic toxicology was clearly different, stipulating, as we have seen, a genetic body that was a great deal more vulnerable to environmental hazards than was the physiological body: mutations were irreversible. The impacts of this message at any rate were far-reaching. Mutagenesis as such, so much seemed clear, constituted a completely new problem because there was absolutely no dose of radiation that was not genetically detrimental. This conclusions derived from this belonged to the incessantly rehearsed warnings then issued by geneticists: The body had no protection that could handle genetic damage. The only protection against this kind of environmental influence was the skin that was at least a passive physical barrier. The skin, in other words, acted like a screen that could absorb sunlight and ultraviolet radiation to a certain degree but was a rather imperfect protection when it came to more energetic radiation.

As such, this model of a passive organism would also shape regulatory rationales. Figure 2 shows stills from the movie "Duck and Cover," an official United States Civil Defense Film produced in co-operation with the Federal Civil Defense Administration. Produced in the 1950s, the movie was screened in schools, instructing youthful Americans in methods of self-protection. In the beginning, a cartoon clip shows a tortoise hiding in its shell, a bar of dynamite exploding right next to it: The tortoise character performed the basic technique of self-protection in the Atomic Age: ducking and covering would offer negligible protection against the intense heat, shock waves, and radiation following a nuclear explosion. Though, in fact, the technique would offer only slight protection against radioactive fallout, it would at least reduce exposure to the gamma rays.

One of the next scenes is set in a classroom. It is a civil defense lesson. The teacher points to the blackboard with a self-made sketch of a turtle on it, and the male voice-over – a trustful guarantor of officially – and scientifically – attested knowledge – utters the words into her mouth. Imagine a tortoise with a thick shell on its back: obviously, such a shell offers better shelter in case of an atomic bomb explosion than the skin of a human. "Now, you and I do not have shells in the quality that turtles have, so, we have to cover up in our own way."[11] The message was clear: humans have to seek a substitute in order to defend against atomic bomb blasts. The basic sheltering substitute was so simple that children could exercise it right away in the classroom: duck and cover under a desk. In essence, one only needed one's arms to protect one's head. But the children also learned what proper atomic shelters look like: you needed hard and thick things that can protect the smooth body against the penetrating forces that emanated from the bomb. Concrete shelter, of course, was the best way to protect oneself. The tortoise's shell indeed was operative inside and outside of the class-rooms: the concrete cover of a reactor, the concrete building in the high-energy therapy laboratory or the lead apron the radiologist used in X-ray therapy were analogue devices, each one of them involving a whole set of knowledges about the relation of the

[11] Federal Civil Defense Administration, Duck and Cover (1951), in: Bill Geerhart, Ken Sitz (eds.), *Atomic Platters: cold war music from the golden age of homeland security* [different media], volume 6, Holste 2005.

external agent and the human organism. The precarious point in this rationale was the body surface, the exceptional thin and weak boundary between the air – the medium that transported the shock waves, the heat and gamma rays – and the organism.

Fig. 2: The tortoise strategy of protection. Stills from "Duck and Cover" (1951), an American Civil Defense movie that illustrates the basic protection strategy in the "Atomic Age."
Source: Bill Geerhart, Ken Sitz (eds.), *Atomic Platters: cold war music from the golden age of homeland security* (different media), volume 6, Holste: Bear-Family-Records 2005.

3. Transition Phase And Crises of Protection

In 1963, the atomic nations agreed to halt above-ground atomic testing. However, the problems of the Atomic Age were not so easily halted; they were followed by new problems that came in connection with the emergence of a vast oil-driven industry and the armada of novel products issuing from the chemical industries. Some 1,000 new compounds were now invented by the latter – per annum. The so-called agricultural revolution too was based on the large-scale usage of pesticides; the industrial production of food meanwhile became dependent on the massive use of chemical additives like preservatives and colorings. Moreover, new habits and styles of living went along with increases in the consumption of drugs and the consumption of (synthetic) products in general. The world of colorful little plastic things became a new power that structured reality. As The sociologist Ernest Zahn diagnosed in his "Sociology of Prosperity" (1964): These things had turned into the "necessary means of communication of social life;" society would "not only be a growing number of people but growing number of things as well."[12]

[12] Cited in Andrea Westermann, *Plastik und politische Kultur in Westdeutschland,* Zürich 2007, p. 19 (trans. AS).

The circulation of this growing number of things, of course, emerged as a problem in and of itself. In the USA, biologist Rachel Carson scandalized the extensive use of pesticides. Pesticides turned out to be everywhere: once released into the environment they were absorbed by plants, passed on to caterpillars that feed on plants, and eventually birds – only to kill the birds that ate the caterpillars. That was why the spring became more and more silent. And this was only the beginning, according to Carson. One environmental problem replaced the next. Indeed, as the problems of mass consumer societies began to eclipse the concerns raised by the atomic age, many a scientists who had been concerned with radioactive fallout and hazards shifted his or her attention to chemical pollution and chemical toxicity. As Creager puts it, "research into chemical carcinogens followed the tracks – conceptual, experimental, and institutional – of radiation genetics" and radiation biology.[13] Principally, geneticists knew that chemical substances could induce mutations just like radiation. Little fantasy was required to imagine the new genetic threats posed by pesticides, food additives, and pharmaceuticals. The so-called consumer democracy, with its chemical-technical infrastructure, might lead straight into a genetic catastrophe.

Yet, not until the early 1970s did these early warnings of chemical mutagens result in regulatory action. Shielding then emerged as the metaphor of choice, and from the 1970s onward, progressive figures within science and politics would demand from the state to shoulder the burden of protecting vulnerable individuals from these damaging environmental influences. Scientists like the biochemist John Cairns claimed: the most promising approach to the control of mutagenic hazards was to "identify" those very factors in the environment and to "eliminate" them.[14] These demands – demands to regulate chemical hazards – gave rise to the U.S. Toxic Substances Control Act (TSCA), which was passed in 1976 and made mutagenicity testing for new chemicals obligatory.[15] In West Germany, the Chemicals Law ('Chemikaliengesetz') was passed a few years later, in 1980.

The protective regime that was instituted when toxicologists and state administrators stepped forward to meet the challenges of the new era was broadly in line with the general regulatory rationale which had already been established by occupational and social hygiene programs. Regulation was perfectly suited to a technocratic understanding of science-based decision-making in an age of planning euphoria. But more importantly here, the protective approach of hygiene was one that implied giving shelter to vulnerable citizens; it was in this respect quite analogous to the tortoise strategy crafted at the height of the Cold War. Sheltering, then, continued to inform the approach taken toward the risks presented by chemicals.

The tremendous difficulties in practice and limits of this strategy became increasing clear, however. Sheltering prevention meant to keep hazardous influences away by any means. Who, for instance, was the one to tell chemical industry to stop the production of crucial intermediates in the production line of chemical synthesis? Also, regulatory efforts were quickly overtaxed as the development of substances accelerated. In contrast to risk research on radioactivity, testing of mutagenic substances faced the problem of big numbers. There were thousands of substances to be tested, but this task also required tremendous resources. These constraints soon became evident. For instance, although West German scientists had been among the pioneers in the field of

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[13] Creager 2010, p. 299; Frickel 2004, p. 42; see also Alexander von Schwerin, The Hollaender Legacy. Mutagens and a new Problematisation of the Consumer Society, 1954-1970, in: Soraya Boudia and Nathalie Jas (eds.), *Carcinogens, Mutagens, Reproductive Toxicants: The Politics of Limit Values and Low Doses in the Twentieth and Twenty-first centuries (Book of papers of an International Conference, 29-31 March 2010 Strasbourg)*, Strasbourg 2010, pp. 109-120.

[14] John Cairns, The Cancer Problem, *Scientific American* (November 1975), pp. 64-78, here: 64.

[15] Scott Frickel, *Chemical Consequences. Environmental Mutagens, Scientist Activism, and the Rise of Genetic Toxicology*, New Brunswick 2004, p. 62.

mutagenesis testing and established a large-scale laboratory for mutagenicity testing in the late 1960s, it quickly emerged that this effort was ridiculously small in face of the new threats.[16] 1,000 to 3,000 new chemical compounds newly went into production each year in West Germany around 1975, not to mention those substances already in use, which nevertheless required testing.[17] Toxicologists, geneticists and biochemists worked in dozens of laboratories around the world on genetic toxicology, but there was little agreement on the right test measures that would provide the regulatory bodies with definite proof of the mutagenicity or non-mutagenicity of a substance.[18]

To summarize: The efforts to regulate chemical mutagens faced a series of epistemic, technical and political problems. The new chemical danger, consequently, was much vaguer, more diverse and unpredictable than the danger of the "Atomic Age." Chemicals came with the new styles of living of mass consumption; therefore, the threat was even more ubiquitous than radioactivity. Now too, the old preventive strategy and its sheltering techniques had seemingly reached its limits. As yet, few, or none, of the scientific experts, politicians, environmentalists and activists involved derived explicit conclusions from, instead they proceeded with arguing and fighting about legislation. Nevertheless, this gap was to become a driving force for new horizons that were about to dawn in the backyards of mainstream risk discourse.

4. A Conjunction: Mutation and Cancer – the Case of Xeroderma Pigmentosum

The early 1970s witnessed a crucial scientific reconfiguration, when cancer became a major target of health and research politics. In 1971, President Nixon, finally having confessed that Vietnam was lost, launched the "War on Cancer." In effect, state money was redirected to combat this other enemy. Cancer quickly became a magnet for all kinds of research. In retrospect, there is no doubt that this "war" was not very successful either as the cancer incidence did not decrease.[19] However, in one respect the run for cancer money turned out to be effective, as it encouraged collaboration across different fields of research. Cancer research had done so before, when, in the 1950s, it had led biochemists into the era of molecular biology.[20] Now, cancer research introduced new phenomena into mutation research. Up to then, the whole experimental configuration of mutation research was built around the concept of *germ line* mutation: Mutation research had been a spin off of 'classical' transmission genetics and the methods of mutation research were designed to show the transmission of phenotypical traits from generation to generation. Hence, genetic mutations were related to a mutated phenotype that was transmissible through generations. Mutations in regular body cells and *somatic* mutagenesis had also been neglected because it were germ line mutations which had been identified as the most urgent *eugenic* problem.

Nevertheless, scientists had long suspected that somatic mutations could be related to carcinogenesis. The hunch was that mutations somehow were transforming body cells into cancer cells. But only from the early 1970s onward, did the attention of mutation research shift from germ line to somatic mutation. Both environmental health and cancer research now became

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[16] Schwerin 2010, p. 412.

[17] Westermann 2007, p. 292.

[18] Angela Creager, The Political Life of Mutagens: A History of the Ames Test, in: Luis Campos and Alexander von Schwerin (Hrsg.), *Making Mutations: Objects, Practices, Contexts. Cultural History of Heredity Workshop at the Max Planck Institute for the History of Science, Berlin, 13-15 January 2009*, Berlin 2010, pp. 285-306; Schwerin 2010.

[19] Robert N. Proctor, *Cancer Wars. How Politics Shapes What We Know and Don't Know About Cancer,* New York 1995, p. 252.

[20] Hans-Jörg Rheinberger, *Toward a History of Epistemic Things. Synthesizing Proteins in the Test Tube,* Stanford 1997, pp. 5-6.

important fields in which experimental biologists could demonstrate the utility of their knowledge. Angela Creager illustrates this three-fold conjunction in the case of the American biochemist Bruce Ames.

> In 1973, "Ames published an overview of his method in Environmental Health Perspectives that put the new premise succinctly, 'Carcinogens are Somatic Mutagens.' Here and in his other publications, Ames drew on recent work on both the genetic code and on the chemical nature of DNA damage, much of which had been funded through the AEC."[21]

The conjunction of carcinogenesis and mutagenesis meant a major shift that affected both research fields. Of course, it also heightened the sense of urgency that one needed to come to terms with mutagenesis. As the *Scientific American* put it: "Almost all cancers appear to be caused by exposure to factors in the environment," –among them environmental mutagens were the most suspicious factors.[22]

It was this very conjunction which now accelerated the raise of a new *biological* model of mutagenesis that went beyond the physical models of the past. Of crucial importance in this unfolding story was a rare skin disease which greatly helped merge the skills of the Atomic Age and cancer research. The story begins with James Cleaver. Cleaver was a biophysicist who, in 1966, joined the Laboratory of Radiobiology at the University of California, San Francisco (UCSF), to study irradiated rodent cell lines. The aim was to figure out whether the progress in mutation research that was made almost exclusively using bacteria had any validity in mammals.[23] In late 1967, he had an inspiring moment when he read a newspaper report in the San Francisco Chronicle that described a study of a familial skin cancer induced by sunlight called xeroderma pigmentosum (XP).[24] Cleaver was excited because XP seemed to be a clue for his problems. The peculiarity of this perfidious cancer was that small doses of ordinary sunlight were enough to induce miniscule tumors in the exposed skin loci. Up until this point, for the most part only dermatologists had been interested in that rare form of cancer, although human geneticists had also expressed some curiosity owing to the fact that it was known to be hereditary since 1925. Cleaver began research into xeroderma pigmentosum, collaborating with the dermatological clinic in San Francisco.

Within a year, Cleaver and his colleagues had revealed the secret of xeroderma pigmentosum: patients were more vulnerable to sunlight than healthy persons because their skin cells missed crucial capacities to cope with the detrimental influence of UV radiation. At the same time, Cleaver's data showed that normal human skin cells behaved similar to bacteria cells. Bacteria could cope with UV radiation because they were able to repair UV-induced genetic damage.[25] Repair phenomena were only a little side field of mutation research at that time, but the new findings were a strong clue that these phenomena could become an important research field. The radiobiologist Errol Friedberg recalls the excitement at that time about the publications: "Thank God, we've now got a disease."[26] These sentiments expressed the expectation that mutation research would profit from these results on genetic repair in human cancer, and especially research in DNA

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21 Creager 2010, pp. 295-296.

22 John Cairns, The Cancer Problem, *Scientific American* (November 1975), pp. 64-78, here: p. 64.

23 For biographical notes see James E. Cleaver, Richard B. Setlow, a commentary on seminal contributions and scientific controversies, *Environmental and Molecular Mutagenesis* 38 (2001), pp. 122-131, here: pp. 122-124; James E. Cleaver, Mending human genes: a job for a lifetime, *DNA Repair (Amst)* 4 (2005), pp. 635-638, here: pp. 635-636.

24 Friedberg 1997, pp. 98-99; Cleaver 2005, p. 636.

25 For details see also Friedberg 1997, chapter 3.

26 Friedberg 2004, p. 195.

repair, because "the DNA repair field could now legitimately lay claim to some of the millions of research dollars then available for cancer research."[27] Actually, the list of institutions that would fund Cleaver's research in the next years attest perfectly to the contemporary linkage between radiobiology, genetics, and cancer research, since it included the Atomic Energy Commission, the National Cancer Institute, and the American Cancer Society.

The XP story had not only resonance in mutation research. The link of mutation genetics and cancer came just at a turning point when popular sentiments about the impact of molecular biology grew. Joshua Lederberg, then Chairman of the Department of Genetics at Stanford University, wrote an editorial for the Washington Post in June 1968 celebrating the findings of Cleaver concerning xeroderma pigmentosum in order to argue against critics of the rising molecular biology:

> Excitement about the new biology of DNA has tended to provoke either of two reactions: that little men would soon be synthesized and come swarming out of the laboratories or that the whole study of molecular biology was mainly of academic importance and we would see little practical impact of it within our lifetime. More sensible observers have suggested that we would see ever deeper insights into cancer and other cell pathology over the coming years and decades.[28]

When Lederberg linked mutation research, cancer research and molecular biology, he argued that the new findings would have an impact on a molecular understanding of carcinogenesis like the example of xeroderma pigmentosum and DNA repair had then shown.

5. The Sensitivity of the Skin: DNA Repair and Mutation as Biological Process

Xeroderma pigmentosum had been at first a link between genetics and cancer, but now it became a link between mutation and cancer research with the added element of molecular biology. The mutation researchers were not that much interested in the well-known fact that XP was hereditary *per se*, but claimed that XP would reveal insights into the molecular mechanisms of how mutations are induced – not only in the germ line but generally. Also, XP would highlight the impact of mutagenesis for the understanding of diseases in man in general. So why did mutationists think these claims to be true, and what was exactly the secret of XP? Did it also hold the secret to DNA repair?

When Cleaver came to San Francisco there was only a small community of mutationists that worked on repair phenomena they had observed mainly in bacteria. In 1964, two groups had shown independently that the recovery of the bacterium *E. coli* from UV inactivation was based on enzymatic activities that included the excision of mutated DNA elements and the subsequent repair of the damaged locus of the DNA.[29] These papers on "excision repair" became the starting point for much more research on the molecular basis of DNA repair phenomena. Especially, it was unclear whether excision repair was a cellular capacity restricted only to bacteria. So, James Cleaver decided an important effort would be to try to discover DNA repair in mammalian cells, just as it had been done in *E. coli*. When Cleaver read about the characteristics of xeroderma pigmentosum, in 1967, he was quite familiar with the previous research in DNA repair and concluded right away that DNA repair could solve the puzzle of how UV rays could easily be induced in the skin of XP patients. It was the right newspaper article at the right time in the mind of a repairologist working with human cell culture. Cleaver's assumption was that the skin cells of

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[27] Friedberg 2004, p. 195.
[28] Cited in Friedberg 1997, p. 99.
[29] Friedberg 1997, p. 95.

XP patients lacked the ability to repair mutations in the DNA; these mutations would accumulate and trigger cancer.

The crucial point in Cleaver's considerations was that XP was not just hereditary cancer, but a *disposition* for getting cancer. Let's hear Cleaver in an interview recalling his thoughts when he first heard of XP: "I thought to myself. 'My word, here are God-given *UV-sensitive* mutants.'"[30] The emphasis was on "sensitive," meaning that the XP patients got skin cancer because their skin was hypersensitive to the damaging effects of UV light. Therefore, patients with XP were more likely to get it because their skin was more sensitive to the damaging effects of sunlight than the skin of healthy people. Strictly speaking, not the skin cancer was hereditary, but the genetic *disposition* to it. In physiological or biochemical terms, the skin cells of XP patients were more sensitive than normal cells because they were not able to avert the mutagenic effects of UV light as effective as normal cells. In other words, there were hereditary mechanisms that regulate skin's sensitivity to UV light.

Cleaver went in to the laboratory in order to show that UV rays induce more mutations in cell cultures of XP patient's than in normal skin. If so, then it was very likely that A) there was a DNA repair mechanism in the skin and B) that this repair mechanism was not working in XP cells: "We worked through the Dermatology Department at UCSF and got skin biopsies and cultures of XP patients. We got three cultures from three different patients with XP. [My colleague Robert B.] Painter and I used the methods we had developed at that time for unscheduled DNA synthesis and Hanawalt's technique for repair replication. The results came up on each cell line right away."[31] Within the year Cleaver's group knew that XP cells were defective in excision repair. "The first two papers, in Nature and PNAS, established the discovery."[32] The scenario was then this:

> At noontime on a sunny day the flow of ultraviolet radiation that reaches the earth is strong enough to generate pyrimidine dimers in the DNA of exposed cells by linking two laterally adjacent thymines or a thymine and a cytosine. Most such DNA lesions in skin cells are repaired in normal people by an excision process strikingly similar to the 'cut and patch' repair process in bacteria exposed to the same radiation. In xeroderma patients, however, the lesions go unrepaired, and their accumulation appears to bring on cell transformation; DNA damage breeds cancer.

In this description Scientific American took XP as a prime example illustrating the significance of DNA repair for life.[33]

DNA repair was not restricted to special cases in bacteria, but a biological mechanism that was a basic feature in mammals and humans, too. Consequently, it called into question the assumptions of the old physical model of mutagenesis. What was at stake became clear in the introductory remarks at a Ciba Foundation Symposium in 1969 that tackled exactly this: "Mutation as a Cellular Process." The symposium assembled mutation biologists, geneticists and molecular biologist. Most of them worked with cellular systems and therefore shared the experience that "the mutagenic treatments we give initiate a process which only after an appreciable time results in our detection of mutations" – an experience that supported the general conviction that "mutation is not just an immediate *event*, a quantum event, or a simple chemical reaction, but a *process* in

30 Ebd. (emphasis by AS).

31 Interview with Cleaver in Friedberg 1997, pp. 97-98.

32 Cleaver 2001, p. 123.

33 Raymond Devoret, Bacterial Tests for Potential Cancerogens, *Scientific American* 241 (2), 1979, pp. 28-37, here: p. 30.

which cellular functions are intimately involved."[34] The main evidence of this conviction was that repair phenomena in different specimen occur under different circumstances.

To cut a long story short: The papers by Cleaver and other groups on xeroderma pigmentosum were the first to reveal the importance of DNA repair in human health. Obviously, human skin cells possessed an enzymatic reaction system to avert danger. This system was able to repair damage to DNA in a similar way as it had been described in bacterial systems before. Excision repairs were found to be one of the fundamental DNA-repair processes and it was effective in eliminating not only pyrimidine dimers but also other kinds of damage to the DNA. What is more, this repair system functioned equally in case of radiation or chemical-induced mutations. The diagram in figure 3 from a popular magazine depicts this process; a process that is inactive in the skin of patients with xeroderma pigmentosum. The mechanism involves particular genes and a system of enzymes (not shown in the schema).

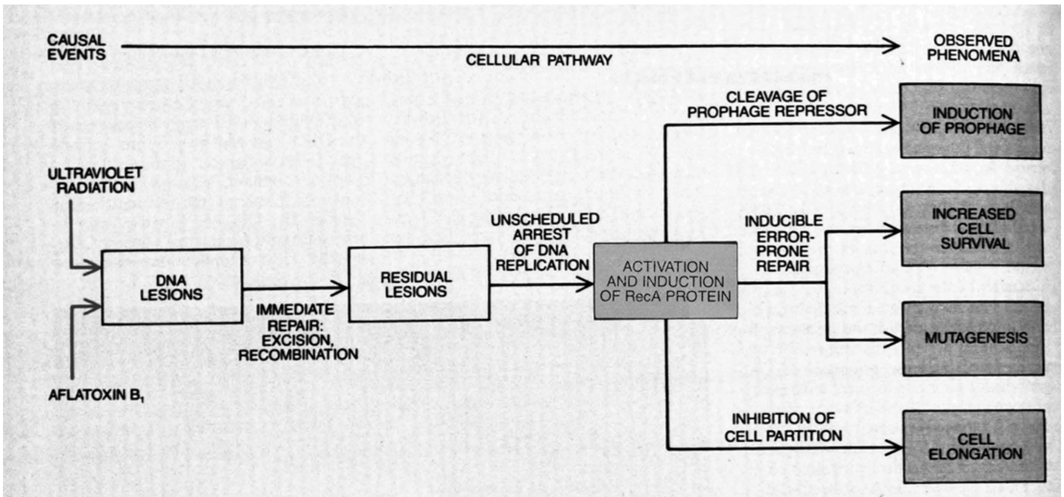

Fig. 3: Mutagenesis as process: Sequence of cellular events follows DNA damage imposed by ultraviolet radiation or by the chemical carcinogens – here in an E. coli bacterium. The original caption says: "Immediate repair processes mend most of the lesions but leave residual damage, causing an unscheduled arrest of DNA replication. This threat to cell survival activates the RecA protein to become a protein-cleaving enzyme and also induces the synthesis of more RecA. Various forms of the protein are involved in three processes that give rise to the four observed phenomena shown at right."
Source: Raymond Devoret, Bacterial Tests for Potential Carcinogens, *Scientific American* 241 (2), 1979, pp. 28-37, here: p. 33.

The diagram illustrates mutagenesis not as a physical event, but rather as a biological process. At the left, radiation or chemical substances have an effect on the DNA. However, this does not mean that the effect automatically ends in a mutation. The question of whether the radiation or chemical stimulus results in a mutation, or whether it is biologically processed, emerges in the course of several steps and bifurcations. The DNA repair mechanism, then, fundamentally changed the concept of mutagenesis. Again, the physical model of radiation genetics had long been that of a bullet. Now, the detrimental influence was more similar to a stimulus that changed the molecular configuration of DNA. This stimulus became a biochemical signal triggering a cascade of molecular reactions. As a consequence, the dogma of mutation genetics – mutations are irreversible – did not hold any more; mutations in principle were reversible.

[34] R. F. Kimball, Chairman's Opening Remarks, in: G. E. W. Wolstenholme, Maeve O'Connor (eds.), *Mutation as Cellular Process. A Ciba Foundation Symposium,* London 1969, p. 1 (emphasis by AS).

By much the same token, the repair model of mutagenesis implied a shift with respect to how bodies were construed in this problematic. The human body's thin protection was not as passive as it once had been imagined. The organism was not a ready-made victim of fateful radioactive or genotoxic disturbances; instead, here was an organism which was built to handle those environmental factors. As it turned out, not only was it in possession of this single repair system, but there were several molecular repair systems as well, active in each cell of an organism and repairing damages around the clock. In 1974, there were already four major repair processes recognized, and many more were to come during the next decades.[35]

On top of that, disturbances of the molecular DNA repair system in Xeroderma pigmentosum increasingly drew attention to the reflationary character of diseases. In the context of mutation research the radiation-sensitive skin of dermatologists changed into a repair-deficient mutant. Xeroderma pigmentosum was now no longer a human genetic and dermatological disease in the first respect, but a disease of the DNA repair system. ('Thank God, we've now got a disease,' to recall the reaction of repairologists.) XP changed into a perfect link between genetics and the environment because, although the defect of the repair system was located in the DNA, the induction of the disease was decided in the skin of the somatic individual exposed to environmental influence. Xeroderma pigmentosum was neither a hereditary cancer nor a UV-driven chronic skin disease, but a disturbance in the relation of the organism and its hazardous environment. XP demonstrated that disease was not about "nature vs. nurture" but about a relation. In that sense, it was a paradigmatic disease of the environmental age to come.

The molecularization of mutagenesis and environmental effects on the organism made a big difference in toxicology. One the one hand, mutagenesis did not differ from toxicological effects any more when it became reversible. On the other hand, DNA repair of UV-induced mutations in skin cell DNA was not the same as physiological recovery from sunburn. Paul Howard-Flanders, one of the pioneers of DNA repair research, put it like this:

> DNA repair was about molecular activation and regulation and self-regulation of the DNA. Both DNA's vulnerability to damage and its susceptibility to repair are implicit in its architecture.[36]

6. New Strategies: Self-repair, Chemoprevention, and Anti-Factors

Mutations were not the fate of an organism or a population, but a disease of the DNA. The German popular magazine "Bild der Wissenschaft" responded to this new insight of the intersection of mutagenesis and molecular biology and declared "Cured Mutations!" (Figure 4) The message was that DNA appeared to be equipped with the cellular capacities to heal itself. This was dramatic news, as the eugenic scenarios and politics of protective prevention at the time rested on the assumption that mutations were not curable. Eugenics was not dismissed (or revived) nor did the general direction in the control of environmental mutagens change; however, self-repair was turning into a new and powerful trope within the discourse on environmental hazards and regulation opening prospects on strategies of governing the body.

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[35] Kimball 1987, p. 13.

[36] Paul Howard-Flanders, Inducible Repair of DNA, *Scientific American* (November 1981), pp. 56-64, here p. 56.

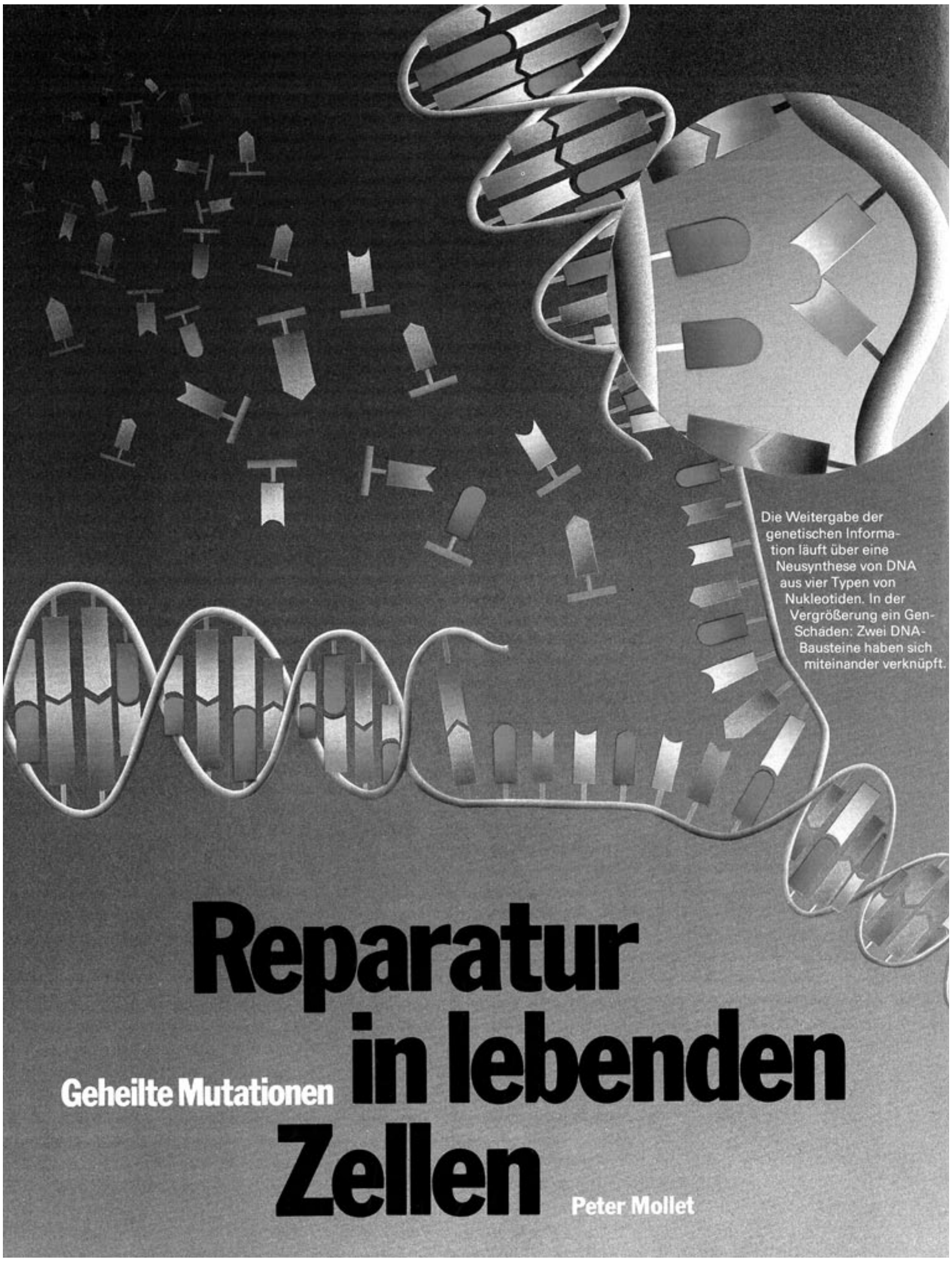

Fig. 4: Mutation as disease of the DNA.
Source: Peter Mollet, Reparatur in lebenden Zellen, *Bild der Wissenschaft* (4), 1977, pp. 69-78, here: p. 69.

There were two main reactions to DNA repair and its impact on the relationship between our bodies and the environment. The first made the strong point that DNA repair, and hence mutations, were natural phenomena. This kind of naturalization of mutagenesis was congenial, especially to those parties who shelved interests in less-than-strict regulation: namely, the tobacco and chemical industry. They argued that there were not only synthetic mutagens but natural mutagens too; hence, evolution had balanced the mutagenic threat and the body's capacities to handle it.[37] A different approach was to take DNA repair seriously in terms of intervention. Obviously, the

[37] Proctor 1995, pp. 133-152 and 158-161; Creager 2010, pp. 297-299.

molecular system of genetic self-repair was still imperfect; so, why not intervene in order to enhance this system? The idea emerged that there would be means in the future to repair mutations. There were already hints that this was a realistic vision. Radiobiologists reported successes in this direction in the late 1970s, deploying radioactivity. Experiments with plants showed that very small doses of radiation stimulated the repair system, so that the plants were more prepared to react to more detrimental radiation doses.[38] This biopositive effect became know as radiation hormesis.

Indeed, during the 1980s a new branch of research would develop at the edge of mutation and cancer research that focused on a new biochemical-molecular research object: "antimutagenesis" and "anticarcinogenesis." The aim was to find the anti-mechanisms and anti-factors that would counteract mutagenesis and carcinogenesis, respectively. Researchers looked especially for substances that were able to stimulate, activate and strengthen the cellular repair system.[39] This research program was not about therapy or repair but the preventive enhancement of molecular capacities and, especially, the capacities of self-repair. During the 1980s, "chemoprevention" became a common slogan for this preventive strategy.[40]

Chemoprevention differed from the preventive rationale of environmental politics. The identification (risk assessment) and subsequent removal (risk management) of risk factors represented the common approach to primary prevention of mutation and cancer. The politics of chemical control included many control techniques such as sheltering techniques and limit values. All approaches were aimed at the environment in order to preempt the vulnerable body from coming into contact with poisonous substances in the first place. 'Surface' here was considered to be a wall and the knowledge was concerning how dense this wall was. The molecular prevention of anti-factors aimed at the single organism instead. The defense was refocused onto the individual – and it became a matter of individual responsibility: from self-repair of the DNA to the self-repair of one one's self.

The proponents of the new research strategy were quite well aware of this difference. They did not doubt that identification and removal of risk factors provided an unreplaceable tool for warding off certain causes of disease. However, they emphasized the chronic problems of that approach: there were too many chemicals to be tested; the predictive value of mutagenic tests was dubious; there were too many mutagens and carcinogens generated by environmental pollution from man-made sources and by 'natural' processes; and finally, many of the artificial risk factors were so tightly connected with the demands and benefits of modern life that their removal was impracticable from economic and social standpoints. The influential hygiene professor Silvio De Flora, Head of the Laboratory of Environmental Genomics and Prevention of Chronic Degenerative Diseases at the University Genoa and member of the International Agency for Research on Cancer (IARC), summarized the health care rationale of anti-factor research in 1988:

[38] Schwerin forthcoming.

[39] The number of approaches on antimutagenesis and anticarcinogenesis and publications increased since the early 1970s. The self-confidence of the field became obvious with the International Conferences on Mechanisms of Antimutagenesis and Anticarcinogenesis held since 1985.

[40] There is another line of chemoprevention reaching back into the Atomic Age: chemical radiation protection. Schwerin 2009, pp. 199-201. This line has to be studied in more detail. One guess is that the term "chemoprevention" was first introduced in the mid-1970s in the context of cancer prevention. Peter Greenwald, Chemoprevention of Cancer, *Scientific American* (September 1996), pp. 96-99, here: p. 96.

> All these problems have contributed to an evident switch of a part of the interest of the scientific community from 'environmental' (in a broad sense) risk factors to anti-risk factors, *acting in the host organism*.[41]

7. From Nutrition to Cosmetics

The molecular strategy of anti-factors pushed the environment of intervention inwards. Up to this point, the dangerous environment and the sites of intervention had started beyond the organism's boundary. This boundary was moved analogously to artificial body walls and skin layers, but still, the principle was to differentiate the outside and inside of the body whether the boundary layer was as skinny as the skin or impenetrable like a lead apron. The new rationale of intervention moved the boundary between the environment and the body *into* the body – as well as the sites of intervention that became the metabolism, the biochemistry of the cells and the molecular regulation of the DNA. But how was it possible to reach these sites? This was not possible without the collaboration of the individuals concerned. Fortunately, the strategy of molecular intervention and prevention fit neatly into a general trend in public health starting the 1970s that demanded the active collaboration of healthy people to achieve prevention. The preventive self's fitness program soon included the right nutrition, too.

In 1982 the National Research Council, an arm of the U.S. National Academy of Sciences, issued provisional guidelines intended to reduce the risk of cancer. They reviewed the role of food in carcinogenesis and recommended to change food habits. The main recommendations were to lower the intake of fat from the current U.S. average of 40 percent of total calories to 30 percent; to eat more fiber, fruits and vegetables; to increase the consumption of complex carbohydrates (such as the starch in flour and potatoes), and more. It was the first official – and subsequently controversial – document to advise that cancer risk might be reduced by adhering to dietary guidelines.[42] Similarly, the WHO Study Group on Diet, Nutrition and Prevention of Noncommunicable Diseases met in 1989 to make recommendations regarding the prevention of chronic diseases and the reduction of their impact. The experts reviewed epidemiological data relating regional diet habits and disease incidence from all over the world. An anticarcinogenic diet included the increase of micronutrients such as vitamins C, E, A, beta-carotene and other carotenoids, and folic acid.[43] Consequently, one recommendation was that every person should consume 400 grams of fruits and vegetables daily.[44]

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[41] Silvio De Flora, Problems and prospects in antimutagenesis and anticarcinogenesis, *Mutation Research/ Fundamental and Molecular Mechanisms of Mutagenesis* 202 (1988), pp. 279-283, here: p. 281 (emphasis AS).

[42] Leonhard A. Cohen, Diet and Cancer, *Scientific American* 257 (November 1987), pp. 42-48, here: p. 42; Council on Scientific Affairs, American Medical Association, Report of the Council on Scientific Affairs. Diet and cancer: where do matters stand?, *Archives of Internal Medicine* 153 (1993), pp. 50-56, p. 50.

[43] Council 1993, p. 53; E. B. Feldman, Dietary intervention and chemoprevention – 1992 perspective, *Preventive Medicine* 22 (1993), pp. 661-666.

[44] WHO Study Group of Diet, Nutrition and Prevention of Noncommunicable Diseases, *Diet, nutrition, and the prevention of chronic diseases: report of a WHO Study Group*, Geneva 1990 (WHO Technical Report Series, No. 797), p. 112-113.

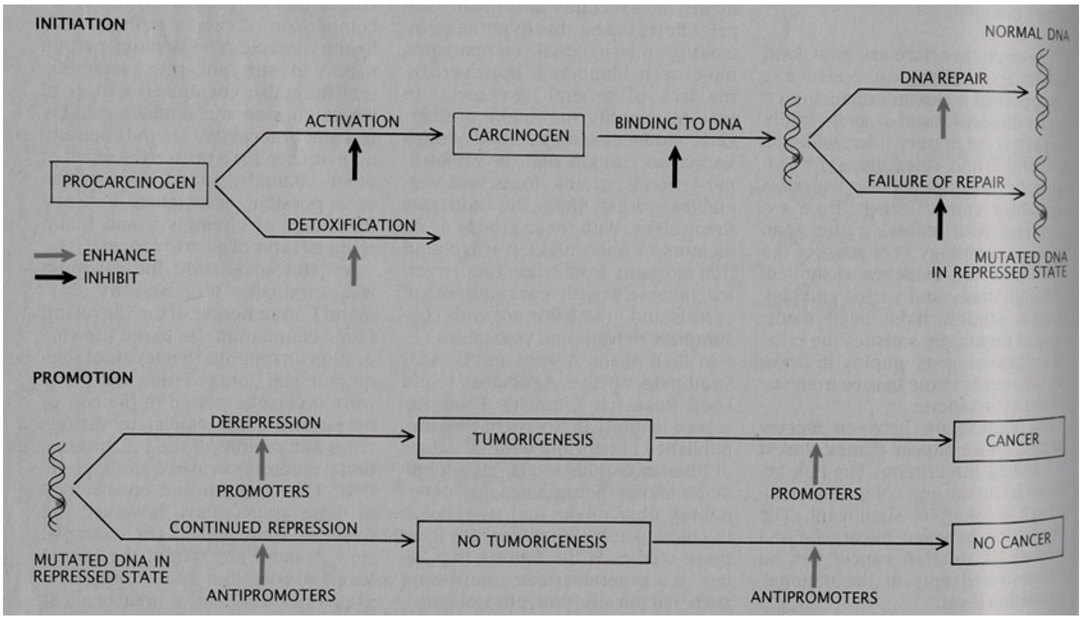

Fig. 5: Intervention by molecular prevention. The targets of molecular prevention are not sites in the environment, but the molecular regulation within the body cells. The figure illustrates how the new model of mutagenesis – a biological process, rather then a physical event – opened up a range of sites for intervention (vertical black and grey arrows). It shows the influence of dietary factors on the initiation and promotion of carcinogenesis that begins with a mutation in the DNA. The mutation may or may or may not be repaired. Steps in activation, binding and DNA repair may be enhanced (grey arrow) or inhibited (black) by "such dietary components as vitamins A, C and E, indoles from cruciferous vegetables and the trace element selenium."
Source: Leonhard A. Cohen, Diet and Cancer, *Scientific American* 257 (November 1987), pp. 42-48, here: p. 46.

These goals met perfectly the results of increasing research efforts in anti-mutagenesis as well as subsequent considerations of how to target it in practice. For the time being and for practical reasons, de Flora and his colleagues judged that the entry of chemoprevention in the general population was limited to dietary suggestions.[45] By the 1980s, there was increasing evidence that food ingredients were crucial not only in the prevention of cancer but also to counteract mutations (see figure 5). Toxicogeneticists like Bruce Ames advocated the ingestion of vitamins and nutrient-rich foods to counteract mutagenicity in foods and chemicals.[46] However, the experimental data on the impact of concrete substances for the enhancement of DNA repair was small, and the antimutagenic effect of micronutrients would become clearer in terms of molecular interaction not until the 1990s.[47]

Actually, one of the outcomes in the next years showed that folic acid or Vitamin 9 – one of the WHO recommended micronutrients – was one essential micronutrient essential in DNA synthesis and repair. First, studies showed that DNA instability was increased by folic acid depletion in human cell cultures in vitro. Also, in an epidemiological sense, a diminished folate status appeared

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[45] Silvio De Flora, Alberto Izzotti, Carlo Bennicelli, Mechanisms of antimutagenesis and anticarcinogenesis: role in primary prevention, *Basic Life Sciences* 61 (1993), pp. 1-16, here: pp. 2-4.

[46] Creager 2010, p. 298.

[47] Silvio De Flora, Claes Ramel, Mechanisms of inhibitors of mutagenesis and carcinogenesis. Classification and overview, *Mutation Research/Fundamental and Molecular Mechanisms of Mutagenesis* 202 (1988), pp. 285-306, here: pp. 299-300; Yukaiki Kuroda, Tadashi Inoue, Antimutagenesis by factors affecting DNA repair in bacteria, *Mutation Research/Fundamental and Molecular Mechanisms of Mutagenesis* 202 (1988), pp. 387-391.

to promote colonic carcinogenesis.[48] In 1998, researchers from the Vitamin Bioavailability Laboratory at the Jean Mayer USDA Human Nutrition Research Center on Aging in Boston announced that folate deficiency impairs DNA excision repair in rat colonic mucosa in vivo.[49]

And here, the story comes full circle. In 2000, the scientists at the Beiersdorf Skin Research Center started to discuss the latest findings on DNA repair and folic acid intake in the light of future skin care strategies, knowing that excision repair was one fundamental mechanism protecting skin against UV driven damages.[50] The young molecular biologists Stefan Gallinat who had joined the Beiersdorf product development only in 2000 – and in 2002 was appointed head of the Molecular Biology Laboratory – pushed ahead with the idea right up to the launch of DNAge.[51] The researchers showed that skin cells increase their folic acid uptake in response to UV exposure, the main factor contributing to premature skin.[52] The idea, that is, was to enhance the cutaneous supply of this vitamin in human skin. They argued that effective DNA repair and normal cell proliferation in aging skin can "only be accomplished and maintained with a sufficient cellular folic acid supply."[53] This consideration became the basis for the DNAge formula handed over for patenting and production in 2006.[54]

Shortly after the launch of DNAge, the Beiersdorf scientists published their studies, in which they used "a commercially available formulation distributed by Beiersdorf AG (Germany)," Nivea Visage DNAge® with 0.03% folic acid and 0.2% creatine.[55] The quantification of UV-induced DNA damage in epidermal keratinocytes showed in vitro that UV-induced DNA lesions decreased after application of the test formulation; further, in vivo tests showed a decrease in wrinkle volume, an increase in skin firmness, and an allover improvement in clinical grading. Based on these studies the Beiserdorf scientists conclude that the DNAge formula skin care regimen counteracted a variety of clinical manifestations of aging skin.[56]

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48 S. J. Duthie, Folic acid deficiency and cancer: mechanisms of DNA instability, *British medical bulletin* 55 (1999), pp. 578-592.

49 S. W. Choi, Y. I. Kim, J. N. Weitzel, J. B. Mason, Folate depletion impairs DNA excision repair in the colon of the rat, *Gut* 43 (1998), pp. 93-99.

50 Interview with Dr. Stefan Gallinat, R&D – Research Skin Care, Beiersdorf AG, Hamburg, 26.11.2010.

51 Beiersdorf, Pressemitteilung from 11.9.2006: Fitmacher für die gestresste Haut (www.beiersdorf.de/Press_Media_Relations/Pressemitteilungen..., seen on 11.11. 2010).

52 Anja Knott, Heiko Mielke, Urte Koop, Rainer Wolber, Thorsten Burkhardt, Jens-Peter Vietzke, Franz Stab, Horst Wenck, Stefan Gallinat, Folic acid: cellular uptake and penetration into human skin, *The Journal of Investigative Dermatology* 127 (2007), S. 2463-2466, here: p. 2465.

53 Anja Knott, Urte Koop, Heiko Mielke, Katja Reuschlein, Nils Peters, Gesa-Meike Muhr, Holger Lenz, Ursula Wensorra, Soren Jaspers, Ludger Kolbe, Thomas Raschke, Franz Stab, Horst Wenck, Stefan Gallinat, A novel treatment option for photoaged skin, *Journal of Cosmetic Dermatology* 7 (2008), pp. 15-22, p. 21.

54 Rudolph 2007, p. 34.

55 Knott et al. 2008, p. 21.

56 Knott et al. 2008, p. 21.

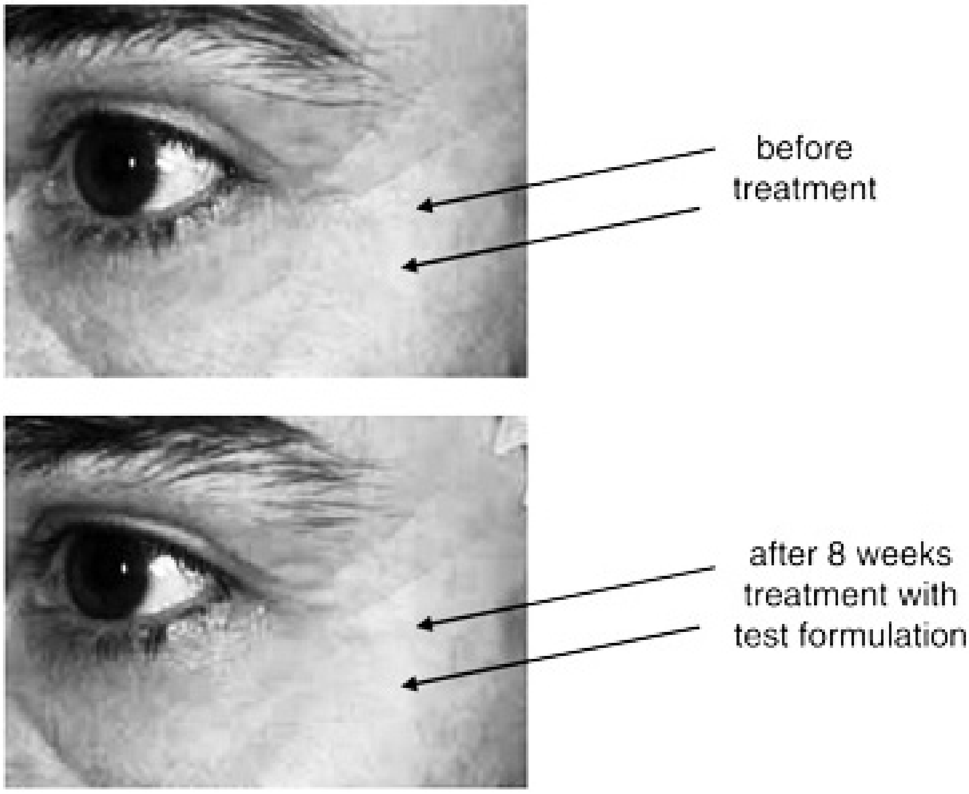

Fig. 6: DNA care as new strategy of face skin care research and production. Clinical monitoring included many parameters such as fine and coarse wrinkles, overall wrinkles, tactile roughness, laxity, and radiance of facial skin. The photos show "a volunteer at baseline (above) and after 8 weeks of treatment with the test formulation (below)."
Source: Knott et al.: A novel treatment option for photoaged skin, *Journal of Cosmetic Dermatology* 7 (2008), pp. 15-22, here: p. 21.

8. Concluding Speculations on DNA (Skin) Care, Self Repair Selfs and Nutrition

Beiersdorf's anti-factor strategy turned out to be successful not only in terms of test results. The competitors were keen not to lose contact with this new field of DNA care since "the total number of cosmetic procedures augmented by 846% from 1992 until 2006, with Americans spending approximately $11.4 billion in 2006 and facial procedures were among the fastest growing in both men and women aiming to combat the signs of aging."[57] Since academic research in DNA repair and antimutagenesis revealed some dozen molecules, the field is open. Lancôme – a brand of L'Oréal – was first in March 2009 with the new luxury trademark Génifique suggesting "sleep well – your genes are working." In the same year, Estée Lauder "updated" its 25 year-old prestigious anti-aging product Advanced Night Repair with "revolutionary technology" and a "revolutionary formula" claiming to offer the "No. 1 repair serum no woman in the whole world would like to do without." L'Oréal Paris launched in January 2010 "after 10 years of gene research" Youth Code™ and proclaimed in print ads "the secret of young skins are its genes." Also, L'Oréal started Skin Vivo under its brand label Biotherm: an innovation with "two-fold Anti-aging-effect: DNA + GENE." Lancaster at once redesigned its company image, now spelling "the DNA of your beauty," and promises that it "repairs the DNA of your skin" with 365 Cellular Elixir Intense.

57 Knott et al. 2008, p. 16.

The significance of DNA care products such as DNAge in turn resides not least in making plastic the long trajectory which it forms a part of: Conceptualizing the cosmetic as a molecular surface, DNAge works on a precarious boundary of the environment and the body – a boundary that has now been moved into the body, its cells, the intersections of molecular environmental effects and the cell's economy of the DNA. In a sense, then, DNAge is the commodity expression of our DNA era. However, in the perspective of the epistemological genealogy of this strategy of molecular prevention, eras and ages have long become intermingled – DNA repair mechanisms, for one, prominently features the stamps of the atomic dispositive.[58] The origin of DNA repair lay within the Atomic Age, as Yi has shown with respect to the crisis of target theory and it meant a "major shift" to the radiobiological community: "from target theory in addressing the problem of protection and recovery from irradiation damage."[59] This shift went hand in hand with a "shift in emphasis from work on ionizing to work on ultraviolet radiation and from the biophysics of radiation action to the chemistry and biology of it."[60]

It was no coincidence either that it should have been the skin which revealed the importance of DNA repair in human health in the transgression period from the Atomic Age of radiation policies to the environmental concerns of societies of mass consumption. Basic knowledge of DNA repair processes came primarily from radiobiological experiments with bacterial cells exposed to UV radiation.[61] UV translated the setting of the radiation research, especially, the aspiring DNA repair research field, the interests of cancer research in the miniscule tumors of the skin of XP patients, and the call of molecular biologists for research money. Skin was a political object in as much it had always been an object of scrutiny for those who involved in the research on environmental threats and their regulation in both the Atomic Age and mass consumerism. Also, UV bridged the distance between mutation research in the Atomic Age and the DNA cosmetics of skin aging that is mostly concerned with the aging effects of sunlight exposition.

The skin of the Atomic Age was essentially a mechanical cover that functioned like a shield. The value of this shielding could be compared with other mechanical means of radiation protection – be it the lead apron in a radiological clinic, the concrete box of an atomic pile, or merely the desk to "duck and cover" behind in the event of an atomic blast. The DNA repair concept changed this view from the early 1970s onward when the radiobiological concept of DNA repair was reformulated in terms of molecular biology. According to the DNA repair mechanism, the skin was not only a layer and cover, but also an active interface between the organism and the environment. DNA repair was, roughly speaking, not an answer to the political and social questions of the Atomic Age, but to the age of mass consumerism: myriads of chemicals in circulation, crises in toxicological control rampart, and new forms of liberalism (or techniques of the self) in the air.[62] The skin became a translator between molecular concepts of cellular regulation and political strategies. The tortoise strategy of the Atomic Age conceptualized the organism as a passive victim of environmental hazards, such as radioactive fallout. The strategy of chemoprevention belonged to a new environmental era that begun in the 1970s and 1980s and placed new expectations on citizens. On a political level, prevention appealed to the "preventive self."[63] The relation between the individual person and its environment was now characterized by

[58] For the close linkage of DNA repair and radiation genetics, see Yi 2007, p. 45; Schwerin 2009, pp. 202-204. However, the field of DNA repair research established as a hybrid field in between radiation biology and molecular biology. Schwerin 2009, p. 202.

[59] Yi 1997, p. 45.

[60] Yi 1997, p. 45.

[61] Schwerin 2009, p. 201-203.

[62] See also Schwerin 2009, pp. 209-210.

[63] Martin Lengwiler, Jeannette Madarász (eds.), *Das präventive Selbst. Eine Kulturgeschichte moderner*

steady work and balancing. On an epistemic level, self-active prevention built upon shifts in conceptions of the relation between organism and environment: this was now about molecular balancing of self-repairing capacities of the cells in the example of DNA repair. And no longer was skin the decisive boundary, but one surface of the body among others where the environment meets the body.

The new epistemicopolitical configuration met in techniques such as individual balancing of nutritional anti-factors, for instance, by eating fruits and slowly cooked vegetables. This message certainly appealed to ecological movements and self-help groups. But what about those who preferred to increase their social capital at the solarium? The beauty industry has since filled that gap. Nivea's DNAge crèmes contain micronutrients such as vitamins. As DNAge claims to activate the self-repair capacities of the skin it helps to democratize the nutritional self-government. Suffice it to say here that these strategies of molecular prevention plus accompanying "shifts in the presuppositions about human beings" – and one has to add: in relation to their environment – are at the conceptual core of the "mutations in personhood" and a new bourgeois lifestyle we have now been witnessing for some time.[64] Socially, this Lifestyle of Health and Sustainability (LoHaS) comprises the ideas of grass-root self-help groups and the enthusiasm of body purists. As such, skin is an accomplice of the entrepreneurial self that tends to dissolve in networks; a flexible self that works around the clock to compensate for exposure to altered environments. The active subject is determined not only to be active as entrepreneur but also active to repair the damages of its activities and, hence, to ameliorate its repair capacities – e.g. with products from Lancôme that counteract "lifestyle-damage."

The DNA repair-activated organism is handling hazards as much as the active subject of post-disciplinary society is busy handling risk factors.[65] Hence, the new environmentalism does not mean that "nurture" is victorious, but that genetics and physiology work together. DNA repair shows how it works: by the involution of the environment into the body cells. Indeed, the molecularization of the environment also in at stake in above is the complementary dimension to what Landecker has described in the case of epigenetics.[66] Nutrition becomes a general feature of the self-repair rationale. Hence, the succession from nutrition to cosmetics – as described here – is not really a succession. Cosmetics become just a special case of nutrition. This trend became striking visible when the industry launched one of its latest products. "Eat beauty products" (Herbalife) is the new message that guides product strategies and marketing campaigns. Biotherm launched in 2011 Skin Ergetic, a "repair-concentrate" that contains the "power of 150 young broccoli shoots." Lancôme's Génifique Nutrics is "inspired by gene research" and simulates the repair of the skin, while Vichy and Estée Lauder call their new product series NutriExtra and Re-Nutriv, respectively. Food trust Nestlé developed, in partnership with L'Oréal, the product series "Innéov" – for cosmetic food supplementation.[67] In the future, skin care will be feeding the skin.

Gesundheitspolitik, Bielefeld 2010, p. 24.

[64] Citation from Nikolas Rose, *The Politics of Life Itself. Biomedicine, Power, and Subjectivity in the Twenty-First Century,* Princeton 2007, p. 106.

[65] For the active subject handling risks, see Paul Rabinow, *Anthropologie der Vernunft. Studien zur Wissenschaft und Lebensführung,* Frankfurt 2004, pp. 140-141.

[66] Hannah Landecker, Nahrung als Exposition. Epigenetik der Ernährung und die Molekularisierung der Umwelt, in: Susanne Bauer, Christine Bischof, Stephan Gabriel Haufe and Stefan Beck (eds.), *Essen in Europa: kulturelle „Rückstände" in Nahrung und Körper*, Bielefeld 2010, pp. 135-159, here: pp. 158-159. See also Susanne Bauer, Krankheit im Raster des Umweltgenomprojektes. Koordination, Lokalisation und Fakten auf der Flucht, in: Tanja Nusser and Elisabeth Strowick (eds.), *Rasterfahndungen. Darstellungstechniken, Normierungsverfahren, Wahrnehmungskonstitution*, Bielefeld 2003, pp. 199-218, here pp. 202 and 207.

[67] Websites of the brands, on 5.3.2011; see also http://www.eatbeautyproducts.com/.

Fig. 7: The merging trend of DNA care and skin feeding.
Source: Ads from diverse magazines in 2010 and 2011.

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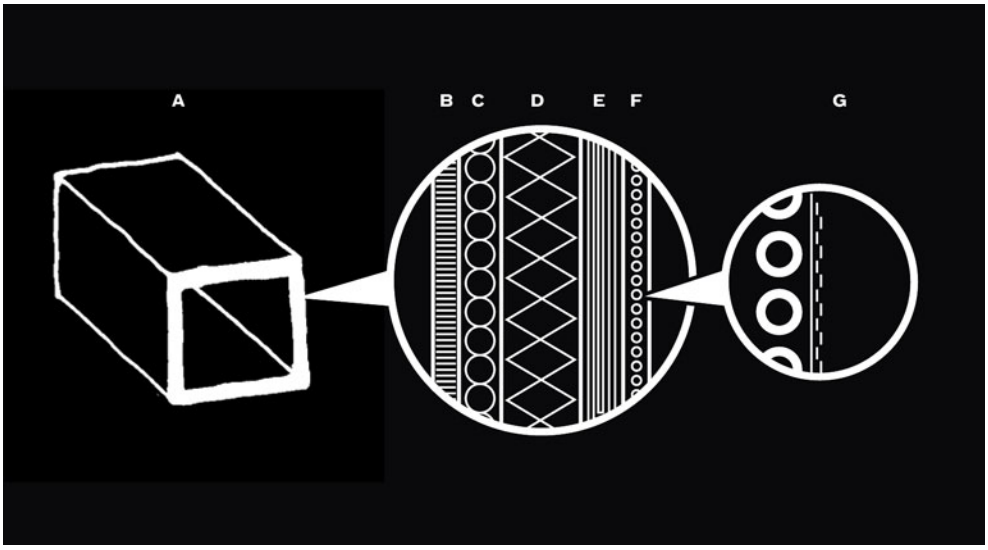
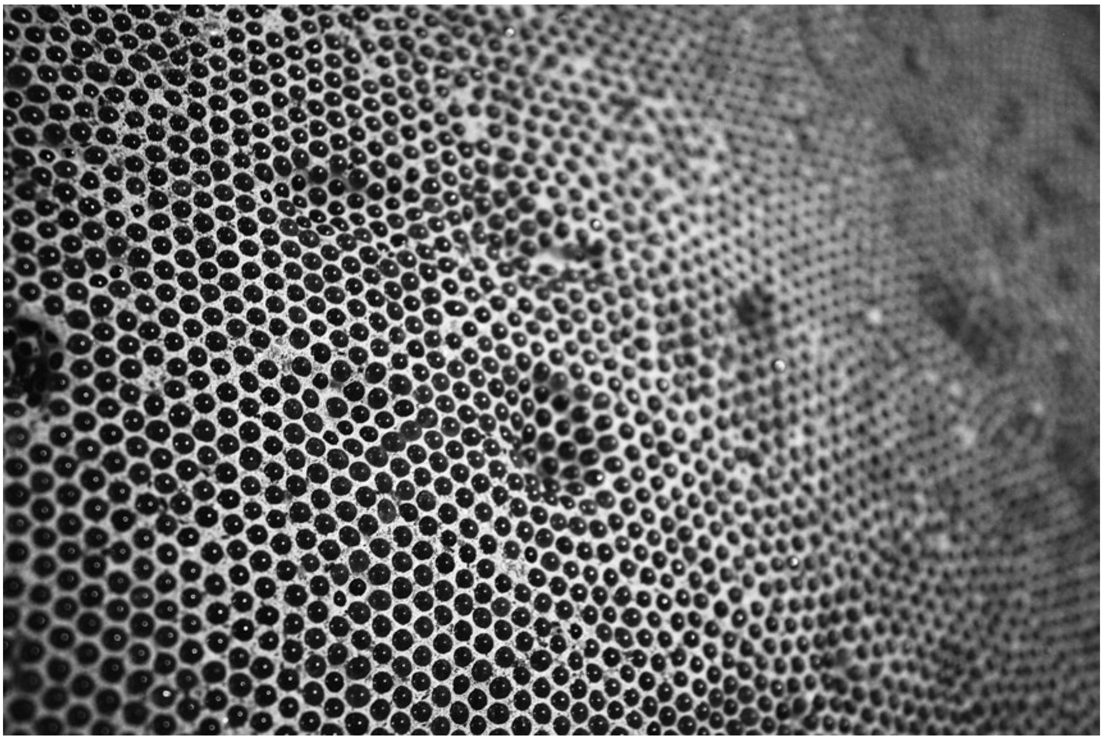
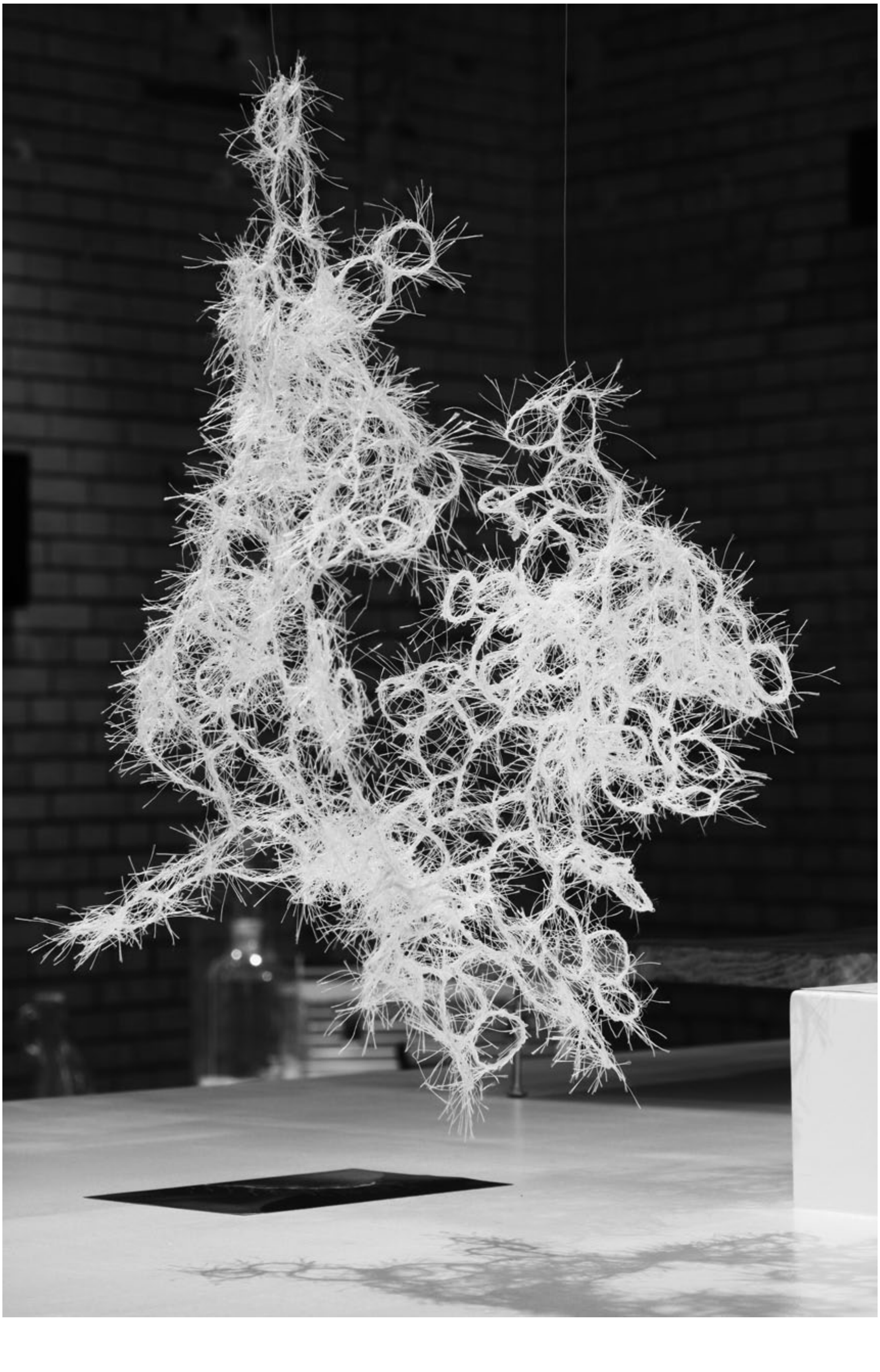
Membranes, Surfaces, Boundaries. Creating Interstices

Architketurforum Aedes am Pfefferberg, Berlin
October 8th – November 11th, 2010








INSTRUCTIONS
HERE!

The exhibition was conceived jointly by the workshop organizers with designer and artist Susanna Hertrich, architect Thorsten Klooster, artist Heike Klussmann, and designer Clemens Winkler. Resonating with the workshop themes, the presented works demonstrated the relation between theoretical and material manifestations of membranes, surfaces, and boundaries, as well as their transitional stages between everyday objects, technologies, and aesthetic production. The project was kindly supported by the Schering Foundation, Berlin.

Figure 1: Layer principle (SMART SURFACES Linoprints / Thorsten Klooster, Heike Klussmann with onlab). For design and the arts, surface design could be considered as a "strategy of enlivenment" or as a change of focus from the appearance of a material to the performance of surfaces: "As structural, chemical and computational properties are integrated at nano-, micro- and macro-scales, even the most traditional material might become more dynamic."

Figure 2: BlingCrete – light reflecting concrete, close-up view of "poured distribution" mock-up (Heike Klussmann, Thorsten Klooster), created for the Exhibition "Membranes, Surfaces, Boundaries – Creating Interstices" at at Aedes Architecture Forum, Berlin, October 08 – November 11, 2010.

Figure 3: BlingCrete – light reflecting concrete, mock-ups (Heike Klussmann, Thorsten Klooster), created for the "Membranes, Surfaces, Boundaries – Creating Interstices" at Aedes Architecture Forum, Berlin, October 08 – November 11, 2010. Above, from right to left: anthracite grid element / 220 x 150 x 3 cm, dished white grid element / 220 x 65 x 3 cm and green "poured distribution" element / 220 x 150 x 3 cm.

Figure 4: Magnetic patterning of concrete (Heike Klussmann, Thorsten Klooster). In the nano-dimension, magnetic patterning caters to the transport and targeted arrangement of particles. With it, methods of exploiting magnetic effects are developed and used to influence particles in a matrix (concrete) in the macro-dimension.

Figure 5: BlingCrete – light reflecting concrete, macro mock-up (Heike Klussmann, Thorsten Klooster) Bling Crete represents a new genre of materials with its own logic of effect that cannot be described simply in terms of the usual categories of heavy and light, or form, construction, and surface. The material combines the positive characteristics of concrete with those of retroreflection. Retroreflecting surfaces send incoming rays of light (sunlight or artificial light) back precisely in the direction of the light source.

Figure 6: BlingCrete – light reflecting concrete (Heike Klussmann, Thorsten Klooster), reflection performance of dished white grid element / 220 x 65 x 3 cm.

Figure 7: Electrifying pencil (Clemens Winkler). Desk object at the exhibition "Membranes, Surfaces, Boundaries – Creating Interstices" at Aedes Architecture Forum, Berlin, October 08 – November 11, 2010. The pencil enhances the surface of objects by adding a graphite layer. The enhanced surface allows to play music with others through the object.

Figure 8: Tensed up (Clemens Winkler). Textile object created for the exhibition "Membranes, Surfaces, Boundaries – Creating Interstices" at Aedes Architecture Forum, Berlin, October 08 – November 11, 2010. The textile can be considered as a knitted sensor – it 'feels', detects and indicates electricity. It uses and gathers electrical energy from its surroundings via various influences and sources – from human activity as well as from nearby electric fields. The material behaviour enables communication and raises specific questions with regard to growing fields such as electronic and human perception technologies.

Figure 9: Electrifying pencil (Clemens Winkler). With the use of some standard electronic components (speaker, battery, chip etc.), a customary pencil is turned into an Actor Sensor Interface (ASI). Users create sounds by drawing lines on paper, i.e. they are designing electric circuits composed of graphite and the human body as decisive parts. The act of drawing is instantaneously turned into a synergic experience.

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