Boundaries of regulatory science: Eco/toxicology and aquatic hazards of chemicals
Halffman, W.

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Chapter 2: Boundaries

So I beheld some shadows borne my way,
Driven on the blast and uttering wail on wailing;
Wherefore I said: "O Master, art thou able
To name these spirits thrashed by the black wind's flailing?"
Inferno, V:48-51, 2nd Circle, the Lustful.

1 Cages or seamless networks?

1.1 The challenge

Among social theories of science, there are two powerful but diametrically opposed models of the boundaries of science. The first one is what could be called the ‘cage model’. This model draws sharp analytic boundaries around science – or indeed around any major institution in society. It does so in order to study what happens within or across these boundaries. One can visualise its topology of society as a cage containing a set of cages, each containing a set of cages, and so on. The second model is the model of the ‘seamless network’. This model sees science as an activity that is intricately enmeshed in other activities in society. Rather than to assume an essential difference or a uniqueness of the ‘region’ called science, it uses a topology of society as a complex weave of relations. In fact, it questions oppositions such as ‘science and society’ or ‘science and politics’, since these are seen to obscure the actual connectedness of such activities.

These two models take diametrically opposed positions with respect to boundaries of science. For the cage model, boundaries were produced by the specific path of the history of modernity, but are nevertheless sharply drawn and clearly identifiable. The network model denies the existence of such boundaries or, at best, sees them as epiphenomenal by-products: chimeras and misrepresentations produced by old-fashioned sociologists. Based on these two traditions it seems as if we have a binary choice: either boundaries are ‘there’, clear and sharp; or they are nowhere, apart from in the misguided sociological imagination.

I will argue that both models do not provide us with adequate theoretical tools for understanding the divisions of expert labour of regulatory regimes. I will not gratuitously argue that ‘in reality’ the world is more complex. (Why should anyone put up with social scientists that only want to show the irresolvable contingencies of life? Novelists like Thomas Pynchon or Paul Auster do a much better job at that.) Rather, I want to question the opportunity of the specific reductions of the complexities of science made in the cage and seamless network models, and contribute to the development of an alternative
notion of boundaries. Drawing on alternative research on boundaries, I claim we can both better understand what goes on in regulatory regimes and come up with an understanding that offers possibilities for a dialogue with the people that work in or with these regulatory regimes. That is the challenge. But firstly, let us examine these two models more closely, for their differences are more than just a matter of topology.

1.2 The cage model.

The cage model divides the social world into sharply distinct domains, containing specific sub-sets of social life. One of those cages is ‘science’. Next to it typically stands ‘politics’, ‘religion’, or ‘the economy’. The cages are sharply demarcated from each other in a society that is ‘functionally differentiated’, the hallmark of sociological modernism. For example, in traditional structural-functionalism, the demarcation of science was grounded in the uniqueness of science’s normative structure. To Robert Merton the unique combination of the cudos norms (communalism, universalism, disinterestedness, and organised scepticism) were what distinguished science from other institutions in society. Likewise, Talcott Parsons identified a unique normative pattern in science. For the more recent functionalist theorist Niklas Luhmann, it is communication in specific ‘codes’ that demarcates the subsystems of society: science communicates in terms of ‘true or false’; politics communicates in terms of ‘power’.

For thinkers that follow the cage model, the difference between science and non-science is cut clearly, with the sharpness of an analytic razor. (Hence a preference for Venn diagrams or a set of nested squares as typical schematic representations.) Once science has been demarcated analytically, the research

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1 The tendency to build theory on the basis of sharply separated social spheres is to be found most clearly in the tradition of (structural) functionalism, viz. Parsons, Merton, Luhmann, and their functionalist reading of Weber. Examples referred to below focus on their conceptualisations of boundaries and obviously do not do justice to the rich qualities of this theoretical tradition. The clearest examples of the approach to define boundaries first and then use these analytic boundaries as a resource for theorising are to be found in systems theory. E.g. classic systems theory: Dubin, Theory Building, pp. 126-46, Ch.5 ‘Boundaries’, where theoretical boundaries are based on a binary logic; or more recent systems theory: Luhmann, The Differentiation of Society: ‘(...) [for the] purpose to use the distinction between system and environment for developing a theory (...)’ (229, my emphasis), i.e. rather than to problematise such distinctions. See also Lilienfeld, The Rise of Systems Theory, p. 247 et seq. For a more reflexive contribution to systems theory, working with dynamic boundaries that are considered part of the representation of social theory rather than ‘really existing’, see Leydesdorff, “The 'Post-Institutional' Perspective”: ‘If a system can no longer be identified, then its boundaries can no longer be considered as given’ (at p. 9).
agenda of this tradition asks how the scientific sub-system of society exchanges money or knowledge with other subsystems; or how the science system further evolves or differentiates in the context of an already functionally differentiated society. (The vocabularies and processes vary between various authors.)² In other words, by identifying cages, their boundaries are used as explanatory resources for what goes on in or between them.

From an historical point of view, the classification of social life in the cage model is that of a genealogy. It resembles the taxonomies of traditional natural history: all the different species are neatly classified in a tree-like structure.³ Primitive society is the stem and modern society a cross-section of the branched-out top of the tree. In theories of functional differentiation, as in natural history, sociologists will try to historically pinpoint the bifurcations of the branches of the tree. The ‘scientific revolution’ is therefore of crucial theoretical importance in a cage model. It is where the cage called science originates, the ‘mutation’ that generated a code that would prove to be evolutionary superior; or a normative structure that would self-reproduce the accumulation of knowledge. Although social theories working with cage models therefore never deny the historical construction of these distinctions, this process of differentiation anchors the division of labour in society and, from there on, creates separated frames of reference in largely irreversible

² Luhmann, *Die Wissenschaft der Gesellschaft*, p. 167 et seq. analyses the demarcation of systems in terms of communications, after signalling the problem of Parsons to adequately attribute every form of action to a specific social system (Luhmann, *The Differentiation of Society*, p. 53; cf. Parsons, *The Social System*; Münch, “Parsonian Theory Today”; Leydesdorff, “Luhmann’s Sociological Theory”, p. 285). This only reruns the problem, since classifying communications is equally problematic as classifying acts, if not for actors, not for theorists, then at least for empirical researchers: just like old-style functionalism, an analytic and unequivocal criterion for what does and does not belong to science is assumed theoretically, but problematic in the practice of research (on this problem for the case of Luhmann’s sociology of law, see: Kargl, “Kommunikation Kommuniziert?”). For classic functionalism on science see Merton, *The Sociology of Science*; Barber and Hirsch, eds., *The Sociology of Science*; or Storer, *The Social System of Science*. The counterpart of these sociologies are demarcationist philosophies of science, which try to find an essential criterion to distinguish ‘science’ from ‘non-science’, Popper being the most noteworthy and Kuhn being no exception. See Pinch, “Kuhn - the Conservative and Radical Interpretations”; and Gieryn, “Boundaries of Science”, pp. 394-404, for an excellent discussion of essentialist demarcations of science.

³ Especially Parsons developed this classification approach almost to the extreme, based on his analytic classification of social life in terms of four functions Cf. Bourdieu, “Het Denken van Grenzen” for the taxonomy parallel, see also Turner, *The Structure of Sociological Theory*. 
ways. Generally, a higher degree of complexity is seen as evolutionary superior. The cage model has a strong preference for biological metaphors and evolutionary types of analysis (along variation/selection schemes).

The construction of further sub-cages can then be followed historically, as the semantics for the analytical distinctions become available, or as the distinctions are embedded in society’s institutions. Within every cage, there is

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4 There is disagreement on the autonomy of the developments within the (sub)systems. Luhmann’s position has radicalised to the view that law, politics, economics, or science as self-organising systems are radically autonomous, i.e. they cannot be systematically influenced in any meaningful way by each other. (Luhmann, *The Differentiation of Society*, p. 142, on the ‘relative autonomy of the political system’; cf. Luhmann, *Die Wissenschaft der Gesellschaft*, p. 290: ‘Der Begriff des autopoetischen Systems (...) schließt damit den unklaren Begriff der ‘relativen’ Autonomie aus.’, and note 30: ‘Ich habe früher selbst diesen Begriff gebraucht und muß das aufgeben.’ [‘The notion of an autopoetic system hence excludes the unclear notion of ‘relative’ autonomy. (...) I have used this notion myself before and now have to give it up.’ My translation.] No more ‘relatively autonomous systems’, the notion is now considered meaningless and replaced with radical autonomy. The theoretical move by which this is accomplished, is to expand the system until everything that operates along its typical code is enveloped, as if a fine slice of communication is cut out of society with extreme analytic precision (cf. ibid., p.291, n.31). This manoeuvre is not new: ‘In this view, a system with an open boundary is one that can be translated into a system with a closed boundary by enlarging the domain through the addition to the system of one or more units with which the formerly included units interact.’ (Dubin, *Theory Building*, p. 128, in 1969!) The obvious ideological consequences of Luhmann’s version are ecological issues that are declared irresolvable in principle, or a market that has absolute autonomy (almost per definition), cf. Kargl, “Kommunikation Kommuniziert?”.

5 Although biological metaphors are used frequently, current systems theorists stress the qualitative difference between a biological system and society as a system, given the importance of communication and reflexive re-ordering of systems (Leydesdorff, “Luhmann, Habermas, and the Theory of Communication”; Leydesdorff, “The ‘Post-Institutional’ Perspective”). Nevertheless, if not in substance, then at least in form, an analytic style is copied from biology, which seems to confer some of the ‘hardness’ of the natural sciences to the social sciences. The irony of history is that Darwin himself imported the metaphor of evolution into biology from political economy (Young, *Darwin’s Metaphor*).

6 The later Luhmann stresses the importance of historical development of semantics. Differentiation in terms of ‘codes’ are seen to guide the structural organisation of society, following the German idealist tradition. He argues that a functionalist interpretation is more enveloping and powerful and seems to suggest it can incorporate structural (mertonian) notions of differentiation (Luhmann, *Die Wissenschaft der Gesellschaft*, pp. 286-87). However, he also states specific structural preconditions may be necessary for this differentiation to take place. In any case, the discrete nature of the boundaries involved is a strong conviction of
room for more cages: science has sub-differentiations. A first kind consists of disciplines, specialities, research fields. The cage model taxonomists find the distinction of disciplines a complex, but not hopeless task. Disciplines are defined by a typical substantial object of study, such as the study of the effects of substances on animal physiology in toxicology. Alternatively, disciplines have typical approaches that distinguish them from their neighbours, also known as ‘formal objects’. For example, toxicologists can be seen to have different methods than ecologists, use different research strategies, and refer to different intellectual traditions, even if they study the same polluted pond. As such ‘paradigms’ ripen, the ruthless differentiation process continues and the reversion of the process quickly becomes unlikely. The paradigms live parallel to each other and fail to communicate in any sensible way. They quickly become incommensurable and mutually exclusive. Hence a cage model is very sceptical of the possibility of interdisciplinarity.

A second kind of sub-differentiation of science can consist of different modes of operation of research, such as applied versus basic, or ‘mode 1’ versus ‘mode 2’, possibly forming new sub-systems on, for example, the boundary between science and the economy. Such new activities along boundaries form their own cage or disappear. To follow my allegorical comparison with biology: by definition, different species do not mix in modernist taxonomies. Hybrids are infertile and will be crushed by the iron logic of evolution, unless if they manage to stabilise as a new species, a new cage.

Just as a typical cage model stresses the difficulty of communication across paradigms, it also stresses the difficulty of science to communicate with politics. Doing science and doing politics are considered to be two entirely different things, radically different ‘rationalities’, disjunctive discourses, or

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Luhmann: ‘Scientific truth (in contrast to the modest truths that function in the everyday life world) is a category that first becomes possible through the emergence of a discrete scientific system. It presupposes strict impartiality toward the distinction between truth and untruth. That is to say, it permits no extrascientific commitment to the truth of specific hypotheses’ (Luhmann, The Differentiation of Society, p. 361).

7 Luhmann, Die Wissenschaft der Gesellschaft, p. 446 et seq.
9 For a radical critique of this type of classification of disciplines see De Wilde, Discipline en Legende, p. 16 seq.
10 Stichweh, “The Sociology of Scientific Disciplines”.
11 Cf. Phillips, “Paradigms and Incommensurability”, pointing at sharp boundaries of paradigms and the affinity of this view with functionalist sociology.
12 Cf. Luhmann, Die Wissenschaft der Gesellschaft, 459 seq.
14 Leydesdorff, “The 'Post-Institutional' Perspective”.

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different modes of communication. Based on a strict distinction of fact and value or truth and power, science at best provides objective knowledge for the policy process, at worst irrelevant or unintelligible communication.\(^\text{15}\) Politics at best takes this knowledge as a basis for action and a reason to provide support for the production of more knowledge. At worst, politics tries to influence the kind of knowledge produced or ignores the truths provided by science, which are both seen as recipes for disaster. The rise of Lysenko due to the political support of Stalin or Nazi science are seen as the emblems of how political intrusion destroys science; a disruption of scientific autonomy that strikes at the heart of the achievements of modernity.\(^\text{16}\) Inversely, when science imposes itself on politics, the result is technocracy: political or normative choice is thwarted by instrumental rationality.\(^\text{17}\) Even when scientists act as experts in the policy process, the cage model holds that their contributions can always be attributed to either science or politics. This is done either by a classification of their actions (in a Parsonian action system) or a classification of their communications (possibly ex post, in a Luhmannian communication system).

In other words: even expertise is always either science or non-science (and then politics, religion, or art, but always something defined).

In conclusion, the cage model proposes to reduce the complexity of society in a typical way: it suggests that we clearly distinguish the cages and sub-cages of sub-cages that together make up society. There are considerable differences between various theorists as to how these cages interact with each other. For some, they are open: things like money float from one cage to the next freely (the Parsons variety). For others, the cages are opaque or closed: they can only ‘irritate’ or ‘disturb’ each other. There is, per definition, no politics in science (for then it would not be science) and political initiatives in science are nothing but a meaningless disturbance, a footprint of a third dimension in two-dimensional Flatland (the Luhmann variety). What they have in common, is their first gesture of theory building: it is to draw a line, a

\(^{15}\) As such, the cage model is at the basis of the conceptions of the science-policy boundary in traditional political science, as mentioned in the previous chapter.


\(^{17}\) This is the critical undercurrent of the cage model, going back on the more pessimistic thoughts of Max Weber, as he saw the ‘iron cage’ of instrumental rationality gradually take over. (Weber, *Economy and Society*; Ritzer, *The McDonaldisation Thesis*) This idea lived on in the critique of modernity of the Frankfurt School, as well as in some critical political theories, e.g. Fischer, *Technocracy and the Politics of Expertise*. 

boundary that we are asked to assume as a basis for further analysis,\textsuperscript{18} legitimated in a symbolic moment of bifurcation in history.\textsuperscript{19}

1.3 The seamless network\textsuperscript{20}

The opposite approach to boundaries of science sees no such cages. In this approach, most clearly found in Actor-Network Theory (ANT), science is intimately related to technology and both are intertwined with politics and society. No single act or thing, no communication in our world can or should be uniquely classified by analysts as either ‘science’ or ‘politics’. No knowledge claim or instrument belongs to any clearly distinct research field. Any effort to force things into cages with analytic scholastics or factor analysis, will naively miss the real heterogeneity of the world, or lead to atrocious imperialism as the false cages of modernism are imposed. Whereas the cage model sees a corruption of the boundaries of science as a dangerous relapse into pre-modern dark ages, the seamless network sees the rigid demarcations of the cage model as the epitome of modernist oppression.\textsuperscript{21}

The model of the seamless network abhors the strict taxonomy and the tree-like genealogy of the cage model.\textsuperscript{22} In the messy taxonomy of the seamless network species \textit{do} mix. They form genetically engineered hybrids, cyborgs that defy all traditional classification – mice with human ears on their backs, cows with human genes, pesticide resistant soy, bionic people with pacemakers.\textsuperscript{23} In a triumphant ecstasy, the advocates of the seamless network

\begin{itemize}
  \item\textsuperscript{18} Lyotard, \textit{Het Postmoderne Weten}, p. 50 et seq.; Giddens, \textit{The Constitution of Society}, pp. 227 et seq., ch.5.
  \item\textsuperscript{19} Cf. Shapin, \textit{The Scientific Revolution}.
  \item\textsuperscript{20} Many alternative terms could be used here: heterogeneous network, cosmic web (Hayles, \textit{The Cosmic Web}), seamless web, rizome,... In as far as boundaries are concerned, I think they all express the same holistic continuity over the demarcationism of cage models.
  \item\textsuperscript{21} Law, \textit{Organising Modernity}.
  \item\textsuperscript{22} Nowhere else has this been expressed earlier and more lucidly than in Deleuze and Guatarri’s rizome manifest. For them, the structure of the tree is the icon of all that is modern and oppressive. From Freud to Chomsky, its binary logic, its focus on structure over process, its reductionist genealogy; everything about the tree-model of modernity has to be replaced by the rizome model: a heterogeneous network, de-centred, multiple, messy, ambivalent (Deleuze and Guatarri, \textit{Rizoom}).
  \item\textsuperscript{23} Haraway’s cyborg concept is precisely the attempt to escape the imperialism of modernist (repressive) categories (Haraway, \textit{Simians, Cyborgs and Women}). Latour has pointed at similar motivations for his approach, though based on a concern for the consequences of essentialist, ethnocentric notions of rationality (with science as its culmination) for non-western societies, rather than a feminist position (Latour, “De Antropologisering van het Wereldbeeld”).
\end{itemize}
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declare that there are no boundaries.\textsuperscript{24} ‘Science’ and ‘politics’, ‘ecology’ and ‘toxicology’ are meaningless categories that draw boundaries where there are none. The defenders of the seamless network model either claim we have been deluded by our own analytic mystifications (a-modernists),\textsuperscript{25} or claim that the multiplication of n-th order overlapping cages into a kaleidoscope of differentiation requires an entirely different social theory (post-modernists).\textsuperscript{26} Such a new theory would start from the heterogeneous ensembles of whatever elements have managed to concoct an alliance in the seamless, borderless ‘cosmic web’.\textsuperscript{27}

\textsuperscript{24} I cannot resist the temptation to quote Latour's mystic 'revelation', on the way from Dijon to Gray, in 1972, at the height of 'the sixties'. "Le ciel était d'hiver et très bleu. Il ne demandait plus que je le fonde sur une cosmologie (...). Il s'ajoutait aux autres cieux, n'en reduisant aucun autre et ne s'y reduissant pas. Il prenait ses distances, s'enfuyait et s'établissait quelque part où il définissait tout seul, comme un grand, sa place et ses buts, ni connaissable, ni inconnaisable, moi et lui, eux et nous, nous nous entredéfinissions, et, pour la première fois de ma vie, j'ai vu les choses irréduites et féériées." [The winter sky was very blue. It did not ask me to found it on a cosmology. (...) It added itself to other skies, not reducing one to another and not reducing itself. It took its distances, fled and established itself somewhere where it defined itself all by itself, like a grown-up, with its place and its goals, not knowable, nor unknowable, me and him, them and us, we mutually defined each other, and, for the first time in my life, I saw things unreduced and festive.] (Latour, Les Microbes, p. 182 (Irréductions), my translation.) As Shapin comments: "There is much to be said in favour of monistic impulses and the close inspection of seams, but there is little to be said from within a seamless web. Ultimately, those that truly inhabit the seamless web can say nothing intelligible about its nature, even, if they are consistent, that it is seamless and that it is a web." (Shapin, "Following Scientists Around", p. 547) Even more difficult is it to compare two networks, such as two regulatory regimes (cf. Bijsterveld, "De Natuurlijke Ouderdom", p. 77).

\textsuperscript{25} Latour, Nous N'avons Jamais Étés Modernes; Latour, "The Impact of Science Studies on Political Philosophy". Entirely opposite to the cage model, the Enlightenment is not considered a radical change that constituted modernity, but a misleading construct of dated ideologists. "(...) nous accordons nous-mêmes au 'monde moderne' une puissance qu'il n'a jamais eue." [We ourselves attribute to the 'modern world' a power which it never had.] (Latour, Les Microbes, pp. 322 (Irréductions), my translation.) "(...) 'les sciences' n'existent pas plus que les langues (...) ou le monde moderne (...)." [The sciences do not exist any more than languages or the modern world.] (Latour, Les Microbes, pp. 241 (Irréductions), my translation.) (See also Harbers, "Van Mensen en Dingen"; Pels, "Liberale Asymmetrie".)

\textsuperscript{26} E.g. Lyotard, Het Postmoderne Weten.

\textsuperscript{27} Cf. Hayles, The Cosmic Web.
The boundaries drawn by the sociologists of the cage model can no longer be used as explanatory resources for the thinkers of the seamless network. Rather than to assume analytic boundaries in order to use them for explanation of something else, only the very construction of such boundaries must be studied. The activities of scientists cannot be explained because they behave according to the norms of science (Merton’s cage model) or methodology. Rather, we must study the construction of such criteria, analyse why activities that are quite mundane at closer look, come to be known as ‘scientific’. Even more so, the advocates of a seamless network want to undermine the cage model and its foundations in essentialist schemes, such as object/subject, fact/value, or science/politics.

Whereas the cage model will stress the disjunctive relation between science and politics, as they are separated by an almost absolute boundary, the seamless network model will stress the intimate connections between science and politics. Scientists lobby politicians all the time, trying to interest them for their research projects, while they let themselves be interested in political projects. Technoscientific projects thus consist of long networks of artefacts, laboratories, political alliances, scientists, audiences, clients, financial sources,... It is a heterogeneous collection that radically cuts through all the boundaries of the cage model, strung together by skilful tinkering and bricolage. The worst thing that could happen is not the intrusion of politics into science, but the very illusion of the cage model that there is no politics in science. It is under such an illusion that science obtains the guise of absoluteness, of monolithic truth and necessity. Therefore, our research must follow the threads of the seamless network, fearless and oblivious of

28 “From now on, the name of the game will be to leave the boundaries open and to close them only when the people we follow close them.” (Latour, *Science in Action*, p. 175) Although Latour states that the issue is to trace variable and empirical boundaries, the approach is to analyse their construction, not consequences: “(...) the internal/external division becomes the provisional outcome (...) With each step along the path the constitution of what is ‘inside’ and what is ‘outside’ alters.” (ibid, p.159) “Thus, Latour follows the work that precipitates the recognised boundaries dividing ‘science’ from ‘society’ and ‘politics’, while denying those boundaries any analytic legitimacy.” (Shapin, “Following Scientists Around”, p. 539.)


Boundaries, for they are merely illusions. Similarly, interdisciplinarity is seen as the natural state of affairs and disciplinary boundaries are to be mistrusted as misrepresentations by scientists involved in their own network-building.

In conclusion, the model of the seamless network takes an opposite strategy for reducing the complexities of social life: boundaries are considered as non-existent or as inconsequential by-products. At most, they are actors’ ideologies that are to be distrusted and never to be taken too seriously. Representations of the seamless network in terms of boundaries or regional metaphors are to be strictly avoided.

1.4 Where both models fall short

The cage and the seamless network model both form the backbones of much more complex theoretical traditions, both in social theory in general and in science studies in specific. This brief and slightly caricatured description of them does not do justice to the enormous complexity and finesse of both traditions, nor to aspects in which they resemble each other.31 I have merely wanted to indicate that their conceptions of boundaries tend to flip-flop between two extremes: either boundaries are strict and fundamental, or they disappear into the epiphenomenal. Either we assume boundaries to be ‘there’ (really or analytically) and move on to use them for explanatory purposes, or we study their construction and accord them no explanatory value at all. Either they are the result of the long-term societal development of differentiating societies; or they are ephemeral and episodic, here today and gone tomorrow.

The cage and seamless network models have their own specific ways of discussing boundaries when they are forced to do so. Both have ‘in principle’ answers. In principle, the seamless network model may allow for constructed boundaries, but never as explanatory categories. Whether these boundaries hold is always a matter of empirical observation. Maybe they do, maybe they don’t. All it takes is one creative actor to upset the entire network again. Even though networks may organise themselves in region-like structures, the use of regional metaphors to describe them is still to be avoided.32 Similarly, the cage model

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31 Some have argued that, in other important respects, my opposition between Luhmann and Latour (or Actor Network Theory in general) makes way for strong similarities, especially from the perspective of self-organisation (e.g. Blauwhof, The Non-Linear Dynamics of Technological Developments). For example, on a very formal level, Leibniz' Monadologie seems to be a common point of reference both for Latour and for systems theory. (Latour, Les Microbes, especially in the Irréductions, contains several comments critical of, but nevertheless engaged with Leibniz; cf. Hagendijk, Wetenschap, Constructivisme en Cultuur). At some points, authors of ANT also have toyed with self-organisation notions (e.g. the end of Latour and Woolgar, Laboratory Life).

32 Hence the standard neutralising response to the accusation of lack of attention for boundaries: ‘For sure, networks can be transformed into more or less stable
may accept 'variable boundaries' in principle, but it thereby undermines the key strength of its explanatory scheme.\textsuperscript{33} Alternatively, the cage models invoke a higher level of complexity, an extra sub-cage that will clarify a boundary that only \textit{seems} undefined.\textsuperscript{34}

A study of boundaries of regulatory science, of the science/policy boundary and the eco/toxicology boundary, is problematic from the perspective of both the cage and seamless network models. Firstly, what are we to make of boundaries drawn by regulatory actors? In the introduction, I mentioned the sharp distinction between risk assessment and risk management that was advocated by the US National Research Council in 1983. These concepts were introduced to separate more sharply scientific and political aspects of regulatory decision making. As these concepts guided regulatory policy of regulatory agencies such as the US Environmental Protection Agency, they became embedded in its organisational structure and decision-making protocols. The implementation of this scheme and a higher reliance on outside sources of expertise offered American regulatory agencies a new stronghold against their critics. It broke through the regulatory deadlock of the early Reagan years.\textsuperscript{35} As regulatory actors understood and presented their work in terms of this scheme, closely following a separation of science and politics, they altered the structure of regulatory decision making, with concrete consequences for how chemicals were evaluated as they passed through the regulatory process. The seamless network model tells us that we must treat this boundary as a misrepresentation by the actors, but what if that representation has concrete consequences? What if actors change the structure of their 'network' on the basis of such schemes? How are we, from the point of view of the cage model, to understand the renegotiations that regulators later gradually introduced into the risk assessment/risk management scheme to accommodate

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\item[\textsuperscript{33}] Cambrosio, Limoges, and Hoffman, "Expertise as a Network".
\item[\textsuperscript{34}] This more common reply to a criticism of lack of attention for the changes and reconstructions of boundaries of cage models suggests that this problem can be resolved by identifying an encompassing cage of a higher order of complexity, in a system-subsystem relation. The systems theory problem of 'interpenetration' of systems is thus redefined as a problem of different forms of coupling between subsystems or the generation of 'new subsystems at the interface'. In this approach, messy boundaries are then defined away.
\item[\textsuperscript{35}] Jasanoff, "Science, Politics, and the Negotiation of Expertise at EPA".
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it to the impracticality of too sharp a separation? From the point of view of the network model, these boundary-drawing activities of regulators are meaningless; from the point of view of the cage model the details of these negotiations are irrelevant.

Secondly, the development and change of such a boundary pose problems for both extreme models of boundaries. If we understand this reorganisation of relations between scientists and regulators as an irreversible effect of the differentiation of science from politics that started in the 17th century, then it becomes hard to understand why such matters this still needed to be negotiated in 1983. It becomes even harder to understand why the sharp separation was renegotiated again in the 1990s. If we understand this science/policy boundary as an inconsequential by-product of the seamless network, then how are we to make sense of the fact that it guided regulatory policy for years and became a popular concept for organising regulatory regimes even outside of the US? Similar problems occur when we look at the debates between ecologists and toxicologists over the definitions of their fields, over the nature of ‘ecotoxicology’ as an interdisciplinary field, or if we look at the division of labour between environmental toxicology and ecology that resulted. Once again we seem to have to make a choice between either exaggerating or ignoring boundaries.

What is particularly difficult to conceptualise in the cage model is how it is possible that the differentiation patterns change in any other way than towards further differentiation. If the historical differentiation between science and politics is a matter of the 17th century, then any debate over the issue today is either a romantic rearguard battle or the sign that new, more complex sub-differentiations have already come into existence. We are not talking about the spread of modernity over the world’s periphery, but about the heartland of its conception. For the cage model, we have cruised down the road of modernity inescapably, ever since the 17th century. The cage model especially fails to provide us with tools to grasp ‘lateral’ changes in the modern constitution: the renegotiations of divisions of labour; the de-differentiation and re-differentiation processes; the dynamic of boundaries that are demolished, reconstructed elsewhere, altered, made vague now and razor-sharp later, and then again vague; disciplines or professions that simply disappear or are taken over by others, or shifts in jurisdictions between them, which are hard to conceptualise.36 Yet these are the issues that concern the actors in regulatory

36 For a Luhmannian understanding ‘Integration in the sense of de-differentiation, however, would be evolutionary unlikely, since the social system would lose its capacity to handle complexity.’ (Leydesdorff, “Luhmann’s Sociological Theory”, pp. 297-300). For a few clear empirical examples of dedifferentiation, both in terms of disappearing functions and institutional structures, see Abbott, The System of Professions; Ruschemeyer, Power and the Division of Labour.
boundaries: citizens that claim a right to review the construction of expert evidence; research fields that vie for dominance as most suited source of expertise for specific issues; new policy discourses that restructure the problem definitions of research.

Hence, thirdly, a connection with present-day debates over the way science operates in the political debates of our societies is problematic for both models. In the most radical forms of the cage model, such as Luhmann’s, debates over the right place and nature of boundaries between science and politics are pointless, the delusion of those who cannot keep up with modernity, or of the nostalgic, retrograde ultra-conservative. Caught within the cage, resistance is futile. It is hard to see how this model could provide us with any other advice than to organise our societies along the lines of its rigid modernist taxonomy. At best, the cage model suggests that we can add an extra layer of complexity, but underneath that complexity the strict separation between science and politics still operates. The cage model therefore has little time for the complex interactions between science and politics that constructivist STS has been indicating: gender biases in science, the involvement of lay knowledge in science, or ‘the politics of scientific knowledge’ are oxymorons. To the extent that such problems occur in interaction between people, they are expected to disappear when interaction begins to follow the already progressing functional differentiation of communication.

Surprisingly, similar problems occur in the seamless network model, despite its militant attitude and politicised tone. The advocates of the seamless network model have systematically tried to show choice or multiplicity in what appeared to be the singular necessities of modernist technoscience. This has been posited as a political agenda of opposition, of undermining, of dissent: boundaries had to be slain, not recreated. This makes it very difficult to come up with a positive agenda from a network model: how can a division of labour between experts and decision makers be organised in a better way? In the words of Mol, looking forward after 20-odd years of ANT:

(...) it is as yet by no means clear what this might mean for further action: what it might imply for going about life in various sites and situations – state, politics, social movements, and technoscience formation.\(^{37}\)

With its radical anti-boundary ontology, ANT has radically distanced itself from the poor commoners who organise their daily world in terms of boundaries and still wonders what it has to tell them – apart from accusations against the evil modernists. (I will return to related problems of radical constructivism in the epilogue.)

Between the two models we are caught with boundaries that seem either too rigid or too fluid. A cage model’s approach to boundaries implies reification of boundaries; a seamless network model implies the opposite, a sort of nullification of boundaries. In both cases, this seems to come with conceptual schemes that are remote from the concerns of regulatory actors. My argument against both reductive approaches is thus also a pragmatic one: assumptions about boundaries made in the opposite positions, lead to an extremely distanciated or ‘Martian sociology’ that has little to do with the language that ordinary earthlings, ‘humans’, use. They inhibit communication with the world we study, both because we speak a different language and because we refuse to understand the categories actors use to cut up their world. Ultimately, the ‘adequacy’ or ‘fruitfulness’ of theories or conceptual distinctions therefore depends on how one identifies the important problems and what kind of solutions one is looking for.

39 Hoefnagels, Wat Heet Sociaal.
40 Shapin, ‘Discipline and Bounding’, 359. Using an understandable language in no way excludes the possibility of disagreeing with actors over the interpretation of the state of affairs.
41 Since we try to be reflexive constructivists: our own theories in STS are underdetermined by our empirical findings, just like we have found in the fields we have studied. The construction of STS knowledge can never be a matter of a cognitive process that is insulated in a scientific system, again just like we have found for any other research field. They cannot be explained away by a ‘non-scientific' context of whatever kind, but they cannot be understood in total isolation either. After fifteen years of constructivist research, simple representational or ‘internalistic' arguments for choosing between possible theoretical reductions are a wearing a bit thin (Radder, “Normative Reflexions on Constructivist Approaches”). It is my strong conviction that many of the theoretical battles of science studies over the last decades have been about which problems of science and society are important and what would be a desirable solution to them. The central argument or sometimes even obsession of different schools in STS varies widely: to defend the universalistic cognitive autonomy of science against unwanted interference (Merton), to provide scientific knowledge for the less resourceful (Science for the People, The Dutch Science Shop movement); to question the monopoly on cognitive authority of established science (SSK in the UK, with a strong ‘expertocracy’); to explain why the West has extended its tentacles over the entire world without assuming moral or cognitive superiority of – French, colonial - Rationalism (Latour, “De Antropologiserin van het Wereldbeeld”). Variation in such STS priorities is only logical, for example in different national contexts or time frames. (cf. Wynne, “Carving out Science”; Hasse, Krücken, and Weingart, “Laborkonstruktivismus”; Harbers and Koenis, “The Political Eggs of the Chicken Debate”) Directly connected with the identification of important problems is the issue of whether one
In order to fully appreciate the positions taken in the depiction of boundaries of science, it is clarifying to look at the development of the seamless network model as it evolved in response to the cage model in STS. I will show that the oppositional stance of the seamless network model has been fixated with the dangers of reification of boundaries as presented in the cage model, thus mirroring some of its fundamental problems, rather than tackling them.

1.5 The fear of reification...

A cage model of science was the cornerstone of the dominant theoretical repertoires in social studies of science well into the seventies.\(^4^2\) Functionalist sociology of science looked for patterns in the organisation and normative structure of science as a community of practitioners and paid less attention to the relations between science and other institutions.\(^4^3\) On a parallel course, rationalist philosophy of science investigated the foundations of the rationality of cognitive processes in science. Both became a target for new and radical approaches to science that emerged in the late seventies and early eighties.

The new sociology of scientific knowledge (SSK) pointed at the influences of social or cultural contexts on the development of science, thereby suggesting that the demarcation of science on both a social and cognitive level would be far less rigid than suggested in the cage model.\(^4^4\) Early SSK argued that the context for the production of scientific knowledge was not only the

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\(^4^2\) I am only interested here in a very partial reading of this history from the perspective of boundaries. It is not my aim to review the revolution that overturned traditional sociology, philosophy, and history of science into present-day Science and Technology Studies. This has been done elsewhere by scholars far more knowledgeable. (E.g. Mulkay, *Science and the Sociology of Knowledge*; Collins and Restivo, “Development, Diversity and Conflict in the Sociology of Science”; Collins and Restivo, “Development, Diversity and Conflict in the Sociology of Science”; Hagendijk, “Strukturation Theory...”; Leydesdorff, “The Knowledge Content of Science and the Sociology of Scientific Knowledge”; Shapin, “Discipline and Bounding”; Shapin, “Here and Everywhere”; Jasanoff, Pinch, and et al., eds., *Handbook of Science and Technology Studies*; Hagendijk, *Wetenschap, Constructivisme en Cultuur*.)

\(^4^3\) Claims about the ‘misdoings’ of functionalist sociology of science always need a reminder: evidently, Merton himself is an exception to this general statement. See the defence of Merton by Gieryn, “Relativist/Constructivist Programmes in the Sociology of Science”, and the critical replies to this piece.

\(^4^4\) Bloor, *Knowledge and Social Imagery*; also see discussion in Hagendijk, “Strukturation Theory...”; and Shapin, “Here and Everywhere”.

specifically scientific discourse supported by the neutral carrier of a scientific community, but that the production of scientific knowledge was influenced (if not determined) by the social structure of scientific fields, the culture of a society in general, or (class) interests. This undermined the idea of a strict boundary between science and non-science, since ‘non-scientific’ factors could be at least as influential for the content or truth-status of scientific knowledge claims. The next wave of SSK research made the consequences more explicit: rather than to hold on to analytic a-priori demarcation criteria between science and non-science, the sociology of scientific knowledge argued that there was no a priori reason to assume some sort of inherent cognitive superiority of science over other forms of knowing.

The original reasons in the background of these attacks on the cage model were diverse. For some, a model of science as an isolated and autonomous institution a priori excluded possibilities to steer science towards socially desired goals. The conceptual a priori choice for a sharp analytic boundary of science precluded political steering before the analysis had even started. For others, cage models of scientific rationality obscured ideological prejudices or cultural stereotypes that may have been built into scientific discourses, for example presenting sexism or racism as scientific fact. Also, a cage model could obscure the actual use and abuse of science by economical, military or political powers. For others still, cage models were seen to support technocratic tendencies, putting experts in positions of power beyond democratic control.

SSK and its typical contextualist approach started a logic that quickly radicalised. For if SSK was constantly blurring the boundaries of science, then was it not undermining its very explanatory resource: an independent explanatory ‘context’ in a ‘society’? Wasn’t it arguing that ‘society’ could not

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45 E.g. generational effects of career perspectives (Mulkay, Vooruitgang in de Wetenschap; or patterns in the organisation of scientific work (Whitley, The Intellectual and Social Organisation of the Sciences); or the patterns of fields in terms of cultural theory (Boon, De List der Wetenschap; Bloor, “Durkheim and Mauss Revisited”; Douglas, ed., Essays in the Sociology of Perception).
46 E.g. Forman, “Weimar Culture, Causality and Quantum Theory”.
48 Collins, Changing Order; Wynne, “Knowledges in Context”.
49 Hence the search of the Starnberg school for stages in the development of science when the boundaries would be ‘open’ or ‘permeable’, creating space for a new form of science policy, see Böhme, van den Daele, and Krohn, “Alternatives in Science”; Böhme, van den Daele, and Krohn, “Finalisation in Science”; Schäffer, “Finalisation in Perspective”; Johnston, “Finalisation”.
really be distinguished from ‘science’ in the first place?\textsuperscript{50} When in the early eighties laboratory studies drew in ethnomethodological resources to analyse science, the criticism of cage models went even deeper. These studies argued that on the level of everyday scientific practice, the demarcation of science versus non-science was highly irrelevant. As Knorr put it in a germane attempt to reformulate the research agenda, scientists do not operate in an epistemic community, guided by homogenous scientific socio-cognitive principles. Rather, they work in trans-epistemic arenas, in which economic, political as well as traditional scientific principles are brought together and where, eventually, these distinctions cease to be meaningful.\textsuperscript{51}

The importance of the cage model as an opponent for the new science studies was perhaps clearest in the internalist-externalist debates among historians of science. For internalist historians, science was to be understood in the context of science, rather than in the context of culture, colonialism, class, myths of nature, or needs of the capitalist state. To externalist historians, ignoring such connections was to follow outdated notions of science and obscuring the economic and political aspects of scientific knowledge. Although, as Shapin has pointed out, the terms of this debate were ambivalent and often confused, the central bone of contention was the extent to which science could be considered a social or cognitive realm isolated from other institutions in society. This debate was never really resolved, but rather abandoned for the new research motto that ‘internal’ and ‘external’ are categories unsuited for studying the development of science. By the end of the eighties, the use of these terms ‘betrayed the amateur, the neophyte, the outsider, or the out of touch.’\textsuperscript{52}

Following these lines of reasoning, two approaches radicalised their position against any form of cage model during the 1980s. One was developed by group of heirs of the SSK tradition and consisted of a series of hyper-sceptical analytic tools in discourse analysis or reflexivist approaches.\textsuperscript{53} A second was based on a semiotic approach to science and technology and developed in Actor-Network Theory one of the most radical forms of seamless

\textsuperscript{50} One of the politically radical programmes for STS launched during the last decades, is the so-called weak programme of Randall Collins c.s., which equally reacts sharply against any attempt to reify scientific demarcation, except that it is more explicit about its political agenda (Collins and Restivo, “Development, Diversity and Conflict in the Sociology of Science”).

\textsuperscript{51} Knorr-Cetina, \textit{The Manufacture of Knowledge}.

\textsuperscript{52} Shapin, “Discipline and Bounding”, p. 333.

\textsuperscript{53} E.g. Gilbert and Mulkay, \textit{Opening Pandora's Box}; Ashmore, \textit{The Reflexive Thesis}. 
network model. The latter continued to raid against SSK for its contextualism, seen as remnants of a ‘modernist’ cage model.54

Thus, cage models of science and their demarcationist imagery have operated as the favourite opponent for the generation of science studies that originated around the mid-seventies. Fighting this enemy was ritualised by adverse references to Merton55 or, later, the silent embargo on expressions and categories such as ‘external vs. internal’, ‘science vs. politics’, shorthand representations such as Venn-diagrams, and for some even the term ‘context’. Using the wrong token will still raise ritual warfare in meetings at STS conferences, often loaded with heavy moral connotations. The problematic nature of a sharp distinction between ‘science’ and other institutions and the impossibility to resolve the theoretical problems of a partial retreat from the cage model, resulted in a purification of language from any form of regional metaphor.56 Especially the radicalisation of ANT primarily seemed driven by the extension of methodological principles such as ‘generalised symmetry’, in stead of the references to political arguments about the opportunity of a cage model that had rung in the background of SSK debates.57

The zeal with which especially ANT scholars have done battle with cage models can sometimes be quite stunning. One may wonder why they are so concerned about the way sociologists think and talk about boundaries – especially since we are constantly reminded that we are not to be too concerned about actors’ talk of boundaries. We are to study the construction of boundaries at best, but not their rather unpredictable consequences. If actors’ representations of boundaries are inconsequential for what goes on in science, then surely the representations of boundaries by sociologists are not a matter of great concern either? If it is really true that we have never been modern (in the sociological meaning of the cage model), then what is the problem?

Obviously, ANT is indeed concerned about how we talk about the boundaries of science, not in spite of the fact that such boundaries have no consequences, but because ANT implicitly very much acknowledges the consequences of how boundaries of science are represented. Just like the more activist members of SSK, ANT is keenly aware of the ideological uses of the cage model and seeks to undermine it because it matters. Treating the


55 On the ritualisation of mertonian sociology of science as opponent long after this was a relevant oppistion, see Hasse, Krücken, and Weingart, “Laborkonstruktivismus”.

56 E.g. the attempt to get rid of public/private regionalist metaphors in Callon and Rabecharisoa, “Gino’s Lesson on Humanity”.

57 Jasanoff, “Beyond Epistemology”.
boundaries of science as somehow powerful, consequential, important, is perceived as a dangerous flirtation with the ideology of an autonomous science, isolated from Power, from desirable social goals, or from the hegemony of Western rationality. ANT does not want to reproduce such boundaries by treating them as more real than they are. It takes the position that it is better to err on the safe side: better to over-stress the absence of boundaries than to run the risk of over-stressing the presence of boundaries. Hence, we have to pretend that we have never been modern, because that is considered the best way to undermine the oppressive cage model of modernity. ANT’s denial of the relevance of boundaries of science cannot be a substantial position (since then its fierce arguments with its enemies would be entirely pointless). Rather, it is a specific choice of cultural politics harnessed by and packaged in methodological principle. The position only holds as long as there is a modernist enemy to fulminate against, be it among STS colleagues, philosophers, or especially sociologists. Ultimately, ANT’s strategy relies on the unlikely assumption that modernity will go away if we deny its existence.

The reification that lures in the heart of cage models of science is therefore not only an artificial enemy that I have projected in my opposition of the cage and seamless network models. I claim these models have been perceived as a very real enemy for post-mertonian science studies, both on an intellectual and political level. Unfortunately, the demonisation of demarcationism has pushed the most radical constructivists towards the other extreme of seamless network models. I found these equally fail to address my questions about regulatory expertise. Fortunately, there is a growing body of research, largely ignored here so far, that tries to overcome this opposition between the two models and tries to recover the best of both worlds. The agenda for the rest of this chapter is to further develop an conceptual repertoire to describe the construction and consequences of boundaries of science. It is an attempt to develop a non-reified boundary concept from alternative notions of boundaries that have also been developed in STS. This repertoire is to serve the analysis of regulatory regimes, rather than to construct a full-fledged alternative topology.

2 Assembling an alternative

2.1 ...and the belief in rhetoric

In 1983, Gieryn suggested that the demarcation of science should be studied empirically, as a practical accomplishment of scientists, rather than to ground the demarcation of science in an a priori sociological or philosophical premise of analysis. He called this practical endeavour of scientists ‘boundary work’, the:
(...), attribution of selected characteristics to the institution of science (i.e. to its practitioners, methods, stock of knowledge, values and work organisation) for purposes of constructing a social boundary that distinguishes some intellectual activity as 'non-science'.

To Gieryn, the lack of any notion of boundary would make the observable cognitive authority of science incomprehensible: how can science be such a powerful institution if it is not socially distinguished from other sources of knowledge? Not unlike ANT, Gieryn asks the question: What makes science strong? However, whereas ANT looks for an answer in the material networks of science, its labs, its collection of resources and mobilisation of allies, Gieryn looks for an answer in culture (a route ANT prefers to avoid). Gieryn does not define the culture of science by some essentialist criterion. Rather than to assume that a cultural uniqueness of science was accomplished by a bifurcation in the seventeenth century, his programme to study the bounding of science empirically suggested that a more or less demarcated science could be saved, as long as the historical (re)production of this demarcation could be shown. Gieryn suggested that the continued demarcation of 'science' from other social spheres could be studied by analysing 'public science':

Boundary work is analysed as a rhetorical style common in 'public science' (...) in which scientists describe science for the public and its political authorities, sometimes hoping to enlarge the material and symbolic resources of scientists or to defend professional autonomy.

Two years after the original paper, Gieryn, Bevins and Zehr chose the legal trials over the teaching of creationism and evolutionary theory at American secondary schools as a case to illustrate boundary work in the public sphere.

58 Gieryn, “Boundary-Work and the Demarcation of Science from Non-Science”, p. 782. There are earlier examples of a similar attention for boundaries in the study of professions, e.g with the term 'boundary maintenance' (see Kuklick, “Boundary Maintenance in American Sociology”), or the idea that professions attempt to draw monopolising boundaries around a set of tasks (Larson, “Professionalism”).
59 Compared to SSK, the problematic is defined complementary: a central goal of SSK was to show that there is no inherent cognitive superiority in science, Gieryn argued that the cognitive authority of science is an historical given, but one that needs to be explained (see Gieryn, Cultural Boundaries of Science).
60 Thus suggesting that the idea of 'differentiation of science' could be saved. I consider it significant that, a year before publishing his work on boundaries, Gieryn had defended the mertonian tradition against the constructivist onslaught. (Gieryn, “Relativist/Constructivist Programmes in the Sociology of Science”.)
They saw a growing professionalisation of American science in these trials, ‘where the boundaries of science are negotiated and where the allocation of cognitive authority and professional resources is at stake.’ Through successful cases of boundary work, the ideology of an autonomous science would become more entrenched, thereby warding off interference in agenda setting or research priorities by non-scientists. Although the bounding effect of a legal decision for the professionalisation of science is acknowledged, the focus of the analysis is on the properties attributed to science: the properties successfully attributed to science. Gieryn makes this explicit in his more recent work: at stake are the ‘cultural boundaries of science’, the ‘cartographies’ of science that scientists managed to produce in their boundary work. The success of boundary work is seen to depend crucially on the success of rhetorical strategies of scientists involved in boundary work.

The notion of boundary work – rather than merely ‘boundary’ – finds its roots in the symbolic-interactionist tradition. By using the term ‘work’, it is stressed that boundaries are not just merely ‘out there’: they only exist in as far as they are instantiated, reproduced. Gieryn too is weary of notions of boundaries of science that would tend to reification, of representing these cultural demarcations as fixed, harder than they really are. The focus is therefore clearly on acting scientists, rather than the routinised institutional patterns in the meanings of ‘science’. However, in 1994, approximately ten years after the original paper, Gieryn also signals that the negotiation of what is science and what is not can not be considered a voluntaristic matter:

[The] idea could easily be exaggerated into a silly conclusion that every episode of boundary-work occurs de novo, and that there are no patterns at all from one episode to the next. Scientific practices and antecedent representations of it form a repertoire of characteristics available for selective attribution on later occasions.

Repertoires are consequently interpreted by Gieryn in a discursive sense. Boundaries, in as far as they are structural at all, are thus a matter of language, of discourse in the narrow, Anglo-Saxon meaning of the term. Already in the work on the creationist trials, Gieryn c.s. had stressed the importance of the outcome of the trials in terms of the ideological reinforcement they provided

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63 Gieryn, Cultural Boundaries of Science.
64 Gieryn, “Boundaries of Science”, pp. 406, emphasis in original; cf. Giddens, The Constitution of Society, p. 171, referring to Roy Bhaskar and with an echo of Marx: ‘Human societies, or social systems, would plainly not exist without human agency. But it is not the case that actors create social systems: they reproduce or transform them, remaking what is already made in the continuity of praxis.’
for a defence of the autonomy of (academic) science against religious interference. It is the rhetorical strength of the repertoire that constitutes the relative obduracy of boundaries, although Gieryn cannot indicate why or how some discursive repertoires provide more obduracy than others.66

De Wilde took up Gieryn’s suggestion that boundary work could also be expected between competing disciplines in a study of the history of sociology. De Wilde makes a similar conceptual move: the demarcation of sociology from history or economics is a matter of boundary work, of settling recurring disputes over disciplinary territory. The continuity in the development of the discipline is sought in the vocabulary, in which sociology – or rather: various sociologies – try to carve out a domain of study. Once again, the continuity is found in language, the discursive repertoire that is accumulated over time and that can be mobilised to defend the disciplinary identity in times of threat, or to conquer new terrain in optimistic times of disciplinary expansion.67 Along similar lines, Jasanoff used the concept of boundary work to interpret the risk assessment/risk management divide in American regulatory agencies. She analyses this divide as a powerful rhetoric that could be mobilised against the Reaganist attack on environmental regulation, constructing a realm of authoritative science for the regulators to build on.68

Evidently, linguistic repertoires are powerful in structuring the world we live in. Dichotomies such as nature/culture,69 masculine/feminine, or is/ought are quite consequential for new episodes of boundary work.70 It is quite understandable that emancipatory movements that find these demarcations obstructing, try to avoid using them or try to find new distinctions that cut through them.71 However, the rhetorical power of linguistic repertoires seems a rather minimal resource to explain the obduracy of boundaries. It would seem enough to think the world in different dichotomies to make the boundaries go

66 Gieryn, “Boundaries of Science”, pp. 407, n.7. This belief in language, rhetoric, or (discursive) repertoires is not covered against an a-modernist critique: boundaries could still be merely an ideology that hinders our perspective on a society in which there are no boundaries really, that never has been modern really. Gieryn’s original approach was to analyse boundaries as a category of actors rather than as an analytic a priori. In the initial publication, he used the term ‘professional ideologies’ of scientists. (Gieryn, “Boundary-Work and the Demarcation of Science from Non-Science”)
67 De Wilde, Discipline en Legende; Halffman, “Non-Sociologie van de Non-Sociologie?”.
68 Jasanoff, The Fifth Branch; Jasanoff, “Policy Shortfalls”.
69 E.g. Mol and De Wilde, “Op de Grens”.
70 And, as Gieryn suggested, also in the reproduction of intellectual traditions: Hagendijk and Cramer, “Intellectual Traditions as Cognitive Constraints”.
71 E.g. Cyborgs, African Americans, and, yes, the prohibition of ‘internal/external to science’. Cf. supra.
away, to evaporate the structuring capacities of language.\textsuperscript{72} The resilience and dynamic of boundaries of science cannot be found in language alone, in spite of the methodological advantages of such a restriction (i.e.: study texts, documents).

A strength of Gieryn’s notion of boundary work\textsuperscript{73} is that it allows for a detailed study of the cultural boundaries of science, as the meaning of what ‘science’ is and is not shifts over time. We are not asked to assume that the world is a seamless network, because the approach shows how the world is carved up with cultural cartographies. Nor are we asked to buy into a cage model, since the fluidity of these boundaries is stressed and especially since these boundaries are not grounded in analytic criteria. Limitations of the notion of boundary work are its focus on action at the expense of attention for boundary institutionalisation. The prime motor behind this work is found in scientists’ quest for professionalisation, based on the possibilities they perceive in their environment. To the degree that boundaries do routinise, this routinisation is seen to reside almost entirely in the discursive schemes in which the cultural maps of science are couched. Therefore, the approach is weaker when it comes to boundaries that are not discussed explicitly as scientists try to more or less consciously professionalise, and when it comes to understanding the institutionalisation (i.e. obduracy) of boundaries. However, there are more theoretical resources for notions of boundaries.

\textsuperscript{72} Both Gieryn and Jasanoff have signalled the continuing problem of explaining the obduracy of boundaries, as if they are not convinced by their own recourse to language. The belief in the rhetorical anchoring of boundaries would send boundary research into the deadlocks of discourse analysis, which, if anything, has indicated interpretive flexibility of discourses, rather than their rigidity. The suggestion of Gieryn's that the structuring capacities of boundary discourse should be considered an empirical matter (Gieryn, “Boundaries of Science”, pp. 407, n. 6; similar position in Shackley and Wynne, “Representing Uncertainty in Global Climate Change Science Policy”), fails to cover the theoretical gap. (Note similar arguments even in Latour, \textit{Science in Action}, p. 175; and Leydesdorff, “The 'Post-Institutional' Perspective”). A slip into a seamless network, with boundaries as false consciousness, then seems not far away. Gieryn is not alone in his recourse to language as a way to avoid realism/reification. Pinch and Bijker also stress rhetoric as a way to come to closure (Pinch and Bijker, “The Social Construction of Facts and Artefacts”, p. 27 and 44) and it is a central assumption in discourse analysis (e.g. Hajer, \textit{The Politics of Environmental Discourse}). Inversely, taking up the question of where the resilience of boundaries comes from, puts us at risk of straying into a cage model. (Which boils down to an old problem in social theory: does one need to assume ruthless chaos as a state of nature and explain order, as in the parsonian tradition; or does society tend to suffocating order and must creative subversion be explained? Or can one avoid both assumptions? Cf. Giddens, \textit{Profiles and Critiques in Social Theory}, p. 43 et seq.)

\textsuperscript{73} Cf. discussion of Gieryn in Bal, \textit{Grenzenwerk}, p. 34 et seq.
2.2 Embodied Boundaries: TOP

We do not have to assume that the result of boundary work is only stored in language. The outcomes of boundary work are also stored in social and material ‘carriers’. Inversely, consecutive episodes of boundary work do not only start from already developed discursive repertoires. Boundary work is also set in a social and material environment. Steven Shapin and Simon Schaffer have described the way 17th century English scientists constructed a new experimental science, together with its conditions. In analysing the work of Boyle and his programme of experimental physics, they distinguish: material technologies such as those embedded in the construction and operation of the air pump; literary technologies for making science known to non-witnesses; and social technologies incorporated in the conventions for interactions among the gentlemen-scientists. By use of a combination of these technologies, Boyle and his companions of the Royal Society were able to construct a specific cultural space in which they could develop their experimental programme.

For example, the laboratory was a ‘disciplined space, where experimental, discursive, and social practices were collectively controlled by competent members’. Creating a boundary that kept alchemical ‘secretists’ and sectarian ‘enthusiasts’ out of this space was very much part of this project. These boundaries were not drawn once and for all, but were renegotiated throughout the 17th century and beyond, based on the resources made available by previous episodes of boundary work.

Similar to Gieryn, Shapin and Schaffer describe the cultural cartography of ‘science’ as they dissect the debates between Boyle’s experimentalism and Hobbes’ preference for deduction. The result consists of similar lists of (two incongruous sets of) properties of science as they slowly developed in these debates. However, Shapin and Schaffer also offer different pointers for where to look for boundary work. We do not only have to look at language or rhetorical manoeuvres, but are advised to look at the use of material and social technologies also. In addition, the cultural map of science is not the only result of the boundary work that goes on here. Another result consists of peculiar new spaces for science: the laboratory or the ‘public’ demonstration theatre, different from the alchemist’s reclusive workspace. Shapin and Schaffer point

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74 Parallel to the dichotomy male/female, feminist approaches to science and technology are now also trying to unravel how gender differences can be embodied in technology. Similarly, Bijsterveld has argued for the (comparative) study of arrangements that include practices, social relations, and apparatuses, along with language (against Latour, in Bijsterveld, “De Natuurlijke Ouderdom”, p. 79).
75 Shapin and Schaffer, Leviathan and the Air Pump, p. 25.
76 Shapin and Schaffer, Leviathan and the Air Pump, p. 39.
77 Shapin, A Social History of Truth, see especially the epilogue.
us at new institutions of science that have been produced by the modification of social conventions:

Any institutionalised method for producing knowledge has its foundations in social conventions: conventions concerning how the knowledge is to be produced, about what may be questioned and what may not, about what is normally expected and what counts as an anomaly, about what is to be regarded as evidence and proof.\(^7\)

Thus boundaries are drawn, including certain questions and problems into the cultural space of science and excluding others, constructing a ‘science’ and a ‘political philosophy’ along each other. The implied division of labour between ‘science’ and ‘politics’ was not radically new (as it built on older resources), nor ever definitive (as it was to be radically renegotiated for various parts of science for centuries after).\(^7\) In line with the SSK tradition, their question is how this state of affairs, these new boundaries, affected the kind of knowledge that was produced, but this time without a causal reduction of ‘the cognitive’ to ‘the social’ or ‘science’ to ‘society’. Simultaneously, the approach acknowledges a differentiation of science, without falling into the pitfalls of the essentialism of cage models. Shapin and Schaffer’s tactic suggests that boundary work can be performed with more than language alone. In addition, the results of boundary work are not considered to be discursive only.

Following Shapin and Schaffer’s list, we can distinguish three forms in which the results of boundary work can be embodied, three types of ‘boundary devices’.\(^8\) With a bit of shorthand, we can call them: texts, objects, and people (TOP).\(^8\) The precise analytic separations of what is a text and what an object and where the distinctions become blurred are not very interesting for our purposes here. We simply want to indicate that the devices used in and produced by boundary work are more than just discursive ones. The list

\(^7\) Shapin and Schaffer, *Leviathan and the Air Pump*, p. 225.

\(^7\) Shapin, *The Scientific Revolution*.

\(^8\) Shackley and Wynne, “Representing Uncertainty in Global Climate Change Science Policy”. They propose ‘boundary ordering devices’, but I think ‘boundary devices’ will do nicely too.

\(^8\) Alternative and more or less similar lists that could be used for distinguishing boundary devices can be found in Hacking, “The Self-Vindication of the Laboratory Sciences”, pp. 29-64 (more detailed, but leaves out people); Abbott, “The Future of Professions” (focuses more objects versus organisations); or Clarke and Fujimura, “What Tools? Which Job? Why Right?”, p. 5. However, the most straightforward and adequate comparison for this typology consists of the common ICT use of ‘hardware’, ‘software’, and ‘human ware’. The suggestion has meanwhile been used successfully in other empirical research, e.g. Oudshoorn, “From Population Control Politics to Chemicals”; Bal, “Boundary Dynamics”.

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functions foremost as a mnemonic, a shopping list for empirical research reminding us not to forget people and objects when we look for boundary devices.\textsuperscript{82}

Under ‘text’ we can consider boundaries marked with rhetoric, language, literary devices. Most of the research on cultural boundary work then reappears as studies of the production of one specific case of boundary devices: the production of specific repertoires of language to distinguish, for example science from non-science, ‘boundary texts’. These are the cultural ‘cartographies’ of science that have been studied so extensively by Gieryn. The ‘risk assessment versus risk management’ scheme we discussed earlier is a typical textual boundary device: it is a resource that was produced by the boundary work of the US National Research Council for specific purposes, and put to use in the context of American regulatory regimes around 1983, but one that was mobilised in entirely different regulatory settings later and elsewhere.\textsuperscript{83}

Similarly, we can now consider additional lines of research as studies of the other two forms of boundary devices. ‘Boundary objects’ are the material boundary devices used to mark boundaries. The concept was developed from the perspective of social worlds theory, starting from an article by Star and Griesemer. They described how relations between professional biologists and amateurs at Berkeley’s Museum of Vertebrate Zoology in the early 20\textsuperscript{th} century were bound by objects such as specimens, museums or maps of particular territories. Boundary objects facilitated specific ‘economies of information’ in a negotiated division of labour between museum biologists and trappers or collectors.\textsuperscript{84} In terms of Shapin and Schaffer, these boundary objects are material technologies of bounding, in this case of a specific scientific project.\textsuperscript{85}

\textsuperscript{82} With this distinction, we assume that boundary ordering devices can be more or less categorised in texts, objects, and people in the first place. This is not entirely unproblematic, as we can identify cases in which boundary devices are more ambivalent combinations of texts, objects, and people; boundary devices that take on a more ‘cyborg-like’ format. Scholars such as Latour or Haraway propose that we do not make such analytic distinctions because they would mutilate the inherent heterogeneity of the world (Latour, \textit{Nous N'avons Jamais Étés Modernes}; Haraway, \textit{Simians, Cyborgs and Women}). My claim takes the opposite route: I suggest that in order to show the heterogeneity of boundary devices, we need to make these distinctions in order to be able to describe how boundaries are delegated to different sorts of devices, i.e. in order to show their heterogeneity. Empiricism offers no easy way around this: ‘heterogeneity’ can only be indicated in comparison with some form of ‘homogeneity’. Heterogeneous networks are never merely heterogeneous – they do no even have that essentialist quality.

\textsuperscript{83} Jasanoff, “Harmonisation: The Politics of Reasoning Together”.

\textsuperscript{84} Star and Griesemer, “Institutional Ecology, ‘Translations’, and Boundary Objects”. Strictly speaking, Star and Griesemer included textual boundary devices
Star and Griesemer loosely compare boundary objects to 'marginal people': people that operate on the boundary of two social worlds. Their position typically forces them to reflect on their multiple memberships, producing representations of their position.\(^{86}\) Simultaneously, they articulate boundaries, sometimes even guarding them. We know people in such a position under different terms already: gatekeepers,\(^{87}\) boundary elites,\(^{88}\) or brokers.\(^{89}\) Just like the witnessing of experiments was restricted to *gentlemen* in Shapin and Schaffer's 17th century examples, the certified scientist is the person invited to conferences on regulatory science, its professional organisations, and the technical committees where the standardisation of toxicity tests is discussed. Editors of scientific journals are example of boundary people: they decide, among other things, what does and does not belong in the journal, thereby performing boundary work both on the issue of what is and is not proper science (or proper methodology in their particular field), as well as the

under the term boundary objects too, but that does not undermine the point that material technologies are used in the activity of bounding a practice. See also Clarke and Gerson, "Symbolic Interactionism in Science Studies"; Fujimura, "Crafting Science".

\(^{85}\) In fact, a much richer body of knowledge can be reinterpreted as offering examples of boundary objects. For example, parallels can be drawn with the material technologies used in regionalisation of social spaces by creating specifically designated buildings, detailed zonation within building structures, or even the lay-out of cities. Material embodiments of social boundaries take various forms here, ranging from doors and locks, to street patterns and bridges. (On regionalisation in buildings: Giddens, *The Constitution of Society*, pp. 110 et seq., Ch.3; Goffman, *The Presentation of Self*; Foucault, *Discipline and Punish*, and a most elegant empirical analysis of laboratory structure: Hillier and Penn, "Visible Colleges"; cf. Latour and Woolgar, *Laboratory Life*; cities: Winner, *The Whale and the Reactor*; Bijker and Aibar, "Constructing a City"; and even artists' studios: Hillier and Penn, "Visible Colleges"). It would be a rather peculiar mistake to exclude material technologies from sociology of science, after reproaching mainstream sociology a lack of attention for technology! (Woolgar, "The Turn to Technology")


\(^{87}\) Crane, "The Gatekeepers of Science". Similarly, judges in American courts are the gatekeepers of what constitutes acceptable scientific evidence and what, part of the larger demarcation issue between 'science' and 'law'. See Solomon and Hackett, "Setting Boundaries between Science and Law".

\(^{88}\) Cf. Hart and Victor, "Scientific Elites". The concept of 'boundary elites' has been suggested in Hoch, "The Crystallisation of a Strategic Alliance", and was used in Kleinman, "Layers of Interests, Layers of Influence".

\(^{89}\) E.g. the use of the term in the study of science and politics in Hajer, *The Politics of Environmental Discourse*. 
Boundaries of their field (what does and does not belong to the field of research the journal covers). Thus, the effect of bounding a practice can be achieved with the use of a combination of devices. This effect has been described by Shapin as:

Bounding a practice is a way of defining what it is, of protecting it from unwanted interference and excluding unwanted participants, of telling practitioners how it is proper to behave within it and how that behaviour differs from ordinary conduct, and of distributing value across its borders.

Evidently, boundary texts, objects, and people usually operate together. For example, the bounding of the domain of interest of a scientific journal is not just a matter of the conceptions of the editors, but is also embodied in the name of the journal or the description of the subject matter of the journal. Here is a typical example from the eco/toxicological journal *Ecotoxicology and Environmental Safety* in 1987:

Ecotoxicology and Environmental Safety publishes manuscripts dealing with studies of the biologic and toxic effects caused by natural or synthetic chemical pollutants to ecosystems, whether animal, plant, or microbial. The Editors are particularly interested in reports about the entry and fate of chemicals in the biosphere and reports of qualitative and quantitative studies of the toxic effects of chemical pollutants and their impact on man. (...) 

This piece of text means very little in itself, but in the hands of an editor, it becomes a resource, a boundary device that can be mobilised, perhaps altered or reinterpreted, but nevertheless a part of the repertoire available for drawing the boundaries of eco/toxicology.

Other simple examples can be found in laboratories of regulatory research. In the winter of 1992, a very friendly and helpful biologist of the Environmental Research Laboratory of the US Environmental Protection Agency in Duluth, Minnesota, gave me a tour of the premises, on the way to one of the many interviews that had been arranged for me. We walked past a door to a part of the laboratory that was locked and had a sign telling me that this was a restricted area, only for authorised personnel. The biologist apologised for not being able to take me into this part of the laboratory, as this

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was an area where commercial interests were involved in the ongoing research. A combination of a temporary ID card, a sign on a door, a lock, and an apologetic biologist regionalised the laboratory for me, the outsider. This was the first time that I ran into the boundary ordering devices that shield parts of regulatory regimes from outsiders, actors like me who could decide to use material to challenge regulatory decisions or reveal commercially sensitive information. This particular door with its lock and uninviting sign was not solely responsible for demarcating this particular part of the regulatory regime for me. Exploring further, I would discover a more complex set of connected boundary devices in other texts, legal texts that drew boundaries between what is and is not to be considered commercially sensitive information. Many more of such occasions were to follow, as I was to discover the peculiar ways in which access to regulatory information was restricted. There would be laboratories of regulatory science with fences, cameras, gates, magnetic passes, locks with number codes and once even a secret floor in a building to hide toxicity tests from animal liberation activists. There would be documents I could read but not copy, information that I was told but could not read, etc. These devices, distributing what is and is not public knowledge, play an important part in the boundary between the parts of regulatory proceedings that are considered political and considered science, as I will show in later chapters.

In the previous chapter, I have used the term ‘boundary configuration’ loosely, to indicate that a boundary is a complex affair. We now have one way to analyse a boundary configuration: we can consider it as a combination of (various types of) boundary ordering devices that are mobilised together to produce the effect of demarcating science from non-science, or research fields from each other. I have suggested a useful way to enumerate these boundaries with a simple checklist: texts, objects, and people. The fact that boundary devices operate together, in configurations, does not mean that they always cooperate neatly. For example, in many regulatory regimes the boundary devices distinguishing science from non-science are organised in incongruent and contradicting ways: the strict demarcation of science and politics will be the text proclaimed in public, while the people that are meant to guard this boundary in regulatory practice operate much more pragmatically. Such ambivalences notwithstanding, the core of the argument so far is that we have to extend the analysis beyond boundaries expressed in language if we are to understand the division of expert labour in regulatory regimes.

2.3 Divide and coordinate

Up to this point, the main focus has been on boundaries as demarcations: devices for distinguishing different spheres of practice. They embody differences and indicate what properly belongs to a practice and what does not.

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However, boundaries also specify conditions of exchange: under which conditions can demarcations be crossed? What are the conditions for cooperation across demarcations? The stress is now on the latter part of Shapin’s definition of bounding: the aspect of ‘distributing value across its borders’. In fact, it is the integrative aspect of boundaries that has received a lot of attention in research that has expanded boundaries beyond language.

Star and Griesemer’s interest in Berkeley’s Museum of Vertebrate Zoology was precisely for the way boundary objects could organise cooperation between professionals and amateurs. For them, the key aspect of boundary objects was their ability to negotiate between different social worlds, taking on different meanings for actors in these different social worlds, providing a device for cooperation without the need of integration into a homogenous community of practitioners. By using boundary devices such as ideal types (e.g. concepts of species), diagrams, and even the museum itself, trappers, farmers, university administrators, amateur collectors and biologists were able to cooperate, while simultaneously the boundary between their social worlds was reproduced.

Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual use. These objects may be abstract or concrete. They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognisable, a means of translation. The creation and management of boundary objects is a key process in developing and maintaining coherence across intersecting social worlds.93

In some cases, the conditions of exchange embodied in a boundary object are flexible, in other cases they are rigid, just as with linguistic repertoires. Joan Fujimura has expanded on the notion of boundary objects to show how boundary devices can become more stable than the cases described by Star and Griesemer. When various boundary objects are combined with standardised methods, they can form a ‘standardised package’, as ‘codefinitions and corestriction narrow the range of possible actions and practices, but also do not entirely define them.’94 Using this concept, she has described how a wide range of social worlds cooperated around notions of ‘cancer genes’ combined with

recombinant DNA technologies to form a ‘bandwagon’ of oncogene cancer research.  

In their description of climate models, Shackley and Wynne have showed how these models negotiate uncertainties between different groups of experts and simultaneously developed knowledge packaged for the policy process. They have showed how these models are actually a co-production of climate policy and climate research, making it very hard to separate the ‘science’ and ‘the politics’ with the essentialist criteria of a cage model. Nevertheless, a division of expert labour is constructed around them, even to the extent that competing forms of climate knowledge are excluded from the process. 

Similarly, boundary people (gatekeepers, boundary elites) do not just guard boundaries, but also negotiate the exchanges between social worlds, such as between research fields or between these fields and policy makers. When scientists advise policy as experts, they are boundary people: scientists who package science as ‘expertise’, i.e. science that is translated into knowledge pertinent to policy makers. While they translate science, they also perform boundary work marking science from policy.

The ability of concepts, metaphors and other boundary devices in language to coordinate heterogeneous activities has also been extensively researched. Shackley and Wynne have indicated how, in the volatile world of global climate change, the boundaries between science and policy are unstable and not univocally articulated. They meticulously analyse diverging interpretations of ‘uncertainty’ as these are used to order the exchanges between scientists and policy makers, in line with their respective agendas.

This is an example of a boundary repertoire that fosters cooperation across a demarcation. A key property of such boundary texts is that they are not fully defined. There are more examples of concepts or developed repertoires with multiple meanings that enable exchange and cooperation: ‘sustainability’, the immunological ‘self’, or the notion of ‘uncertainty factor’ in toxicology, or the use of radar representations of environmental quality (the Dutch ‘amoeba’). Similarly, notions such as ‘discourse coalition’ suggest how

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95 Fujimura, “The Molecular Bandwagon in Cancer Research”.  
96 Shackley and Wynne, “Global Climate Change”.  
98 Shackley and Wynne, “Representing Uncertainty in Global Climate Change Science Policy”.  
100 Bal, “Boundary Dynamics”.  
101 Van der Windt, En dan: Wat is Natuur nog in dit Land?.  
102 Wagner and Wittrock, “States, Institutions, and Discourses”; Wagner and Wittrock, Discourses on Society.
scientists and policy makers cooperate through shared basic metaphor of nature,\textsuperscript{103} or because they construct their activities with the same story lines.\textsuperscript{104}

Our earlier example of ‘risk assessment/risk management’ shows this double character of boundary devices nicely: the conceptual pair divides science and politics, drawing up a list of how these activities are different. However, simultaneously, the division suggests a way scientific knowledge should be integrated into the policy process: the policy maker provides the problem definition, the scientist provides the factual knowledge, and then the policy maker can produce a decision balancing political interests in light of the scientific evidence. Thus boundary devices divide and coordinate, split and bring together.

Tests used for regulatory purposes, computational models, or standardised methods provide interesting examples of boundary devices along the science/policy boundary. Van den Bogaard has described in detail how macro-economic models were constructed in the Netherlands. In these models, definitions of key concepts were a co-production of concerns from economics and from social policy, such as for example the definition unemployment. Even though these models became a mongrel of considerations that could be considered ‘scientific’ and ‘political’ with the strict criteria of a cage model, the end result was that the macro-economic model of the Dutch Central Planning Bureau did come to represent the state of the art of economic knowledge. Together with drawing a boundary between what was to be considered politics and what economic science, it also made economical knowledge available for the policy process. In order to do this, the economy and economic planning had to be ‘configured’: translated in the mathematical terms of the model.\textsuperscript{105}

In sum, boundary devices demarcate and coordinate as two sides of a coin. They distribute meaning, values and participants between practices, while simultaneously structuring interactions between such practices.\textsuperscript{106}

\section*{2.4 Organisations and institutions}

Expert organisations, such as the Dutch Central Planning Bureau, combine various boundary devices: their expert economists act as a boundary elite between policy makers and academic economics,\textsuperscript{107} their discourse of prediction and mathematisation has a bounding effect, similar to the models

\begin{itemize}
\item \textsuperscript{103} Kwa, \textit{Mimicking Nature}; Kwa, “The Rise and Fall of Weather Modification”.
\item \textsuperscript{104} Hajer, \textit{The Politics of Environmental Discourse}; Harre, Brockmeier, and Muehlhaeusler, eds., \textit{Greenspeak}.
\item \textsuperscript{105} Van den Bogaard, \textit{Configuring the Economy}.
\item \textsuperscript{106} This implies that these two practices have to be in contact, e.g. be aware of each other’s existence, or have implicit contact. Otherwise, it might be better to simply call their relation a ‘separation’.
\item \textsuperscript{107} Wilts, \textit{Economie als Maatschappijwetenschap}.
\end{itemize}
they develop. Expert organisations can be seen as complex concatenations of boundary devices, dividing and coordinating science and policy. Expert organisations are therefore strategic locations for studying boundary configurations: several boundary devices interacting with each other. Similarly, the organisation of a journal’s editorial board mentioned earlier contains such a combination of boundary devices. Within organisations, boundary devices are coordinated and integrated.

As part of this coordination, boundary configurations can become embedded in organisational structures. For example, the risk assessment/risk management scheme discussed earlier was copied throughout the US Environmental Protection Agency in the 1980s. As the scheme was taken on board and used to restructure the decision-making process, it became a guiding principle for organising regulatory decision making (in spite of ambivalences in the original report recommending the scheme). Jasanoff has shown how the use of this rhetorical boundary device (i.e. ‘text’) did not suffice to contain regulatory conflict, but was joined by organisational changes in the way the USEPA set up its expert advisory bodies. The risk assessment/risk management scheme was embedded in the structure of the organisation and its procedures. However, simultaneously, the bounding of science and policy changed, as the USEPA learned to keep its experts at arms length. Similarly, propositions for representing desired relations between science and policy are not just presented in terms of list of properties and rules of mutual engagement, but come with organisational diagrams, suggesting how regulatory procedure ought to be organised. Abbott describes how the division of expert knowledge (i.e. its boundaries) can be embedded in organisational structures:

Within the organisational structure of a hospital are encoded the design of a certain type of health care, the partitioning of the task carrying it out, and an order of their performance. The hospital’s role assignments, its rules of procedure, and its interrelations actually contain an expertise of health care: an interventionist expertise as opposed to a preventionist one, and episodic as opposed to a permanent one, a technical as opposed to a spiritual one.

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108 Van den Bogaard, *Configuring the Economy.*
110 As she concludes: ‘It is a final irony of environmental decision making that, in the effort to keep politics distinct from science, the processes of scientific fact making so freely accommodated themselves to the demands of politics.’ Jasanoff, “Science, Politics, and the Negotiation of Expertise at EPA”, p. 217.
111 E.g. Gezondheidsraad, *Risico, Meer dan een Getal*.
As boundary devices are embedded in organisational structures and routines, they may become harder to identify as they shift out of explicit language. Apart from integration of boundary devices, there is therefore another kind of work that is performed in organisations, especially in expert organisations. When such expert organisations try to package the knowledge of experts into models or tests, they delegate the effect of bounding from the people to the objects: now the model or the test become the main boundary device, possibly even functioning without the presence of the expert. A model then comes to stand on its own, becomes ‘an immutable mobile’ in Latourian terms.\(^{113}\) With this delegation, the effect of bounding also can be delegated. As Van den Bogaard showed, conceptions of 1940s Dutch economists about the way economic policy (planning) should relate to economics as a body of knowledge were packaged into the model they developed. In later chapters, I will show how similar delegations occurred in regulatory regimes for chemicals, as specific science/policy and eco/toxicology boundary configurations were packaged in toxicity tests and models. The boundaries of scientific fields can be similarly delegated, for example as conceptions or representations of where one fields begins and another stops are embedded in the selective membership of a laboratory or a review board.

Inversely, boundaries can be thematised again, ‘shifted back’ to language, for example as organisations or professional groups become reflexive (as they typically do in times of crisis). They may produce maps, regional and non-regional, of their operations, draw up ‘ins’ and ‘outs’, and reorganise themselves accordingly. In this sense, maps quite literally construct the territory.\(^{114}\) Such maps may even travel to other organisations, serving as exemplars for restructuring.

These activities of organisations to bring together or delegate boundary devices points at two particularly interesting types of organisations for studying boundaries in regulatory regimes. The first are, obviously, the organisations that provide expert knowledge for regulatory policy. At institutes such as the Dutch National Institute of Public Health and the Environment (RIVM), the USEPA’s environmental research laboratories, or England’s Water Research Centre, a large number of boundary devices were produced in the form of assessment procedures, toxicity tests or expert reports, shifting and renegotiating boundaries. A second type of organisations for studying the boundaries of regulatory science consists of the organisations of/for eco/toxicologists: their journals, their laboratories, their professional associations, and their funding agencies. Not just because in their laboratories eco/toxicologists also produce knowledge for regulatory regimes, but also

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\(^{113}\) Latour, *Science in Action*.

because in these organisations boundaries are embodied dividing and coordinating ecology and environmental toxicology as research fields, thereby bounding bodies of knowledge for regulatory regimes.

This leaves one important question open: can we talk of ‘boundaries’ at all? Shouldn’t we only talk of ‘boundary devices’? The term boundary suggests some degree of institutionalisation: boundaries that have somehow routinised, can be found in various locations, and that seem to recur beyond the episodic occasions of boundary work. (i.e. institutions are not to be confused with organisations.) The constructivist studies of boundary work have focused on the production of boundaries. I have already indicated how Gieryn identifies language as the principle repository for the outcome of boundary work. To the extent that he pays attention to institutionalised boundaries, these are seen as stored in repertoires for talking about science. In his empirical research, the focus remains with processes of production of boundaries, rather than their reproduction. However, in order to understand the division of expert labour in regulatory regimes and especially how that division of labour seems to acquire some degree of obduracy, it will be necessary to pay more attention to processes of institutionalisation, tracing texts as well as objects and people. In other words: boundary ‘work’ needs an institutional counterpart, ‘boundaries’.115

I will describe how boundary devices become institutionalised through a combination of processes. Boundary devices are copied form one context to the next. In the process, their originally more or less explicit bounding effects travel along. For example, toxicity tests produced for regulatory purposes in the US, packaging particular regulatory concerns and especially boundaries of American regulatory sciences, are taken over by international standardisation committees and transferred to European regulatory regimes, carrying their original load as boundary devices, even as they are adapted again to local needs. A first mechanism of institutionalisation is therefore distribution (and translation). An additional process is routinisation: boundary devices that are used without specific attention for their potentially problematic bounding effects. Boundary devices may shift out of language, or the connotation of boundary texts may be lost (e.g. as the routinisation of the risk assessment/risk management scheme lost the specific regulatory context in which it was proposed). This means that the routine use of boundary devices implies a reproduction of boundaries. Last, I have already indicated how boundary devices tend to be organised together, forming boundary configurations. As such processes operate together, they weigh heavier on attempts to shift or redefine boundaries, as environmental movements that have tried to change the state of affairs of the division of expert labour in regulatory regimes have experienced. I believe that, in that sense, the use of an institutional term

115 Cf. similar argument in Bal, Grenzenwerk, p. 37.
boundary is warranted and even necessary. As I hope to show, an institutional constructivism is possible and, in fact, overdue.

The risk of using the institutional term boundary is that we end up with a cage model of differentiation again. We may be tempted to reduce a boundary to an essentialist criterion again. For example, we can try to locate ‘the boundary’ between science and politics in an expert institution on the basis of a narrow definition of what is and is not science. Describing the complexities of a boundary configuration as a boundary means that we, as analysts, also perform a delegation: from the configuration (where a boundary may have shifted out of language into tests and relations between people) to our own language. There are only two imperfect answers to this problem. The first is that this is what (our own) science is: a translation of what we register into what we write. The second is that we have to maintain a sensitivity in our research practice for the multifarious forms of boundaries, resist the temptation to reduce more than is necessary to show interesting regularities in the division of expert labour, without presenting our own reconstructions as causes. Aiming in between the Big Narrative of modernity and the ‘vignettes’ of its anecdotal refutations may require a specific sensitivity. The alternative is to remain willingly blind to such regularities out of fear of reification.

3 Taking stock: a vocabulary for boundaries

Against the background of cage and seamless network models of boundaries of science, I have developed an alternative notion of boundaries from some of the available research on boundaries of science. I needed such a notion in order to be able to study and describe boundaries of science in regulatory regimes in ways that do justice to both the regularities in the division of expert labour, as well as its constructed nature. In other words: I have provided a vocabulary that allows for an understanding of institutionalised regularities in boundaries of science, while not reifying such boundaries more than they should be. Although I do not claim to have resolved the more profound tensions between modernist and post-/a-modernist theories of science (far from that), I do claim to have at least provided myself with some conceptual tools for describing boundaries that make the best possible use of the strengths of the cage and seamless network models, while avoiding some of their pitfalls.

This vocabulary can be summed up now. Boundary work defines a practice in contrast with other practices, protects it from unwanted participants and interference, while attempting to prescribe proper ways of behaviour for participants and non-participants (demarcation); simultaneously, boundary work defines proper ways for interaction between these practices and makes such interaction possible and conceivable (coordination). Thus boundary work has the double nature of dividing and coordinating. Boundary work uses and

116 Cf. Radder, *In and About the World*. 
produces boundary devices: textual, material, or social resources for demarcating and coordinating practices, i.e. boundaries can be embodied in texts, objects, or people. As outcome of boundary work, boundary devices set the stage for consecutive episodes of boundary work. Boundary devices can be stable and maintain their identity in the practices they bind (e.g. ‘standardised packages’), or they can be flexible and negotiable. Some boundaries can manage to coordinate practices because they define these practices sharply; others derive their strength from their vagueness (such as ‘loose concepts’).

Organisations are locations where boundary devices are integrated and delegated, i.e.: the effect of bounding can be shifted between different types of boundary devices, for example from experts to expert models. Boundaries can even be embedded in the structure of an organisation. Organisations of particular interest for the study of regulatory science are expert organisations (such as advisory science boards), and organisations of scientists (especially for field boundaries, such as journal or laboratories). Boundary devices usually operate together (texts in combination with people in combination with objects) as do organisations where boundary work is performed. We can describe these combinations as boundary configurations. In some cases, boundary configurations may operate to bound a practice in the same way, in other cases boundary configurations can be ambivalent, for example as boundary work in public discourse and boundary objects as used in regulatory practice do not match.

The term ‘boundary’ is used for institutionalised boundaries, i.e. configured boundary devices that are distributed and routinised in use. It is shorthand for describing their patterns and for pointing at the extra work that will be needed to change these patterns.

I want to stress that this is only a vocabulary, a descriptive tool, developed for the specific purpose of describing the division of labour in regulatory regimes. As a vocabulary, it explains next to nothing: it is only a skeleton for the actual analysis. In order to put some flesh on these bones, we have to turn to the description of historically specific regulatory regimes and fields of knowledge. However, I do claim that with this tool, I can address some key issues in regulatory regimes. We can look more systematically at the organisation and structure of the regulatory sciences. In the alliances between policy makers and scientists, there is not just one science, nor an entirely contingent heterogeneity of sciences. An analysis of boundary configurations can show how regulatory science is divided amongst fields with their typical approaches to environmental effects of chemicals, their typical way of framing and translating environmental problems, and their typical way to look for solutions. The assumption made in the analysis of a boundary configuration is that boundaries are crucial in ‘framing’ scientific discourse. The relevance of such an analysis for regulatory policy is that these ways to identify, define, and address environmental problems are fed into the way policy regimes regulate.
In addition, this vocabulary will allow us to understand more precisely the potential of science to bring about policy closure. The way in which scientific fields affect policy regimes is clearly not one of 'dissemination' of science into policy, or the sole result of scientific entrepreneurship. The alliance is a matter of two-way interaction and mutual adjustment. From empirical research on this topic, we know that the organisation of the boundary between science and policy is one of the crucial mechanisms in this alignment process. My notion of boundaries draws the attention to the interplay of social, material, and literary technologies in this process. The boundary concept developed here allows us to describe the form that the alliance takes. It does not say anything about the conditions under which an alliance is likely to come about, nor does it predict sources of tension. Those are matters that require a more substantive theory, for which, towards the end of the book, I will argue an institutional constructivism offers good perspectives. (Chapter 7 will relate the patterns in the integration of expertise with policy to styles of decision making, drawing on neo-institutional theory.)

Last, I have suggested in the previous chapter that we should look less at public controversies and to study more the run-of-the-mill of regulatory regimes. This methodological point runs parallel to my argument to study embodied boundary devices and their institutionalised reproduction, rather than boundary work alone. This should allow us to get an idea of what happens to boundaries when they are not explicitly at stake in the public disputes of regulatory policy making. It is now time to turn to the actual stories, the complex jungle of regulatory regimes, and especially their particular divisions of labour.