

# 11 Purity and interest

On relational work and epistemic value in the biomedical sciences

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## **Boundaries, interests, and modes of purification, a pragmatic take**

Actors in biomedicine constantly produce different versions of valuable science and knowledge. Certain tropes of value, such as ‘science for innovation’, ‘translational science for clinical utility’, or ‘basic science for biological understanding’ are endlessly being made, remade, coordinated, and hierarchized. We are all too familiar with these different stories from political discourse, social analyses, and the news media (cf. Shapin 2008). The biosciences are simultaneously assembled: ‘in articulation with neoliberal, entrepreneurial modes of participation’ (Hayden 2003: 29) and other tropes of value such as Merton’s CUDOS norms (1973a).<sup>1</sup>

The problems with many contemporary approaches that purport to analyse these tropes are that they tend to contribute to reproducing and performing them. For example by calling for specific versions of entrepreneurship and innovation, as in the large literature on the Triple Helix model (Etzkowitz and Leydesdorff 2000), or alternatively decrying the corruption of the biosciences, as in the literature on Biocapital (Sunder Rajan 2006).<sup>2</sup> The drawback of this situation is that these analytical approaches often presuppose and reify a specific ontology of science and industry, clouding the work that goes into

<sup>1</sup> In Merton (1973a) the norms are universalism, communism, disinterestedness, organized scepticism.

<sup>2</sup> These questions should be understood in light of recent academic debates where the biosciences are argued to be shaped by economic interests. Patents and industry–science collaborations are argued to be changing the research landscape in profound manners; some even argue that there is a shift in the political economy of the biosciences (cf. Mirowski 2004, 2011). Notions such as biocapital (Rose 2007; Sunder Rajan 2006, 2012; Yoxen 1981), bio-value (Mitchell and Waldby 2010; Waldby 2002), and bio-piracy (Shiva 1997) have become increasingly utilized for describing this intermingling of the biological, the scientific, the medical, and the economic. It has been argued that there is a shift in the scientific apparatus of knowledge production and a redrawing of boundaries in (and between) science and business (cf. Widmalm 2007: 120). However, as other researchers have shown, these boundaries are not clear-cut, but are complexly intertwined (Shapin 2008).

making these entities in practice. That is: in approaches calling for ‘science for innovation’, science and industry are seen as separate pre-existing entities that are found in a System of Innovation (Lundvall 1992) or in relation to a Triple Helix of state, university, and business (Etzkowitz and Leydesdorff 2000). As a consequence, these approaches perform science, industry, and the value of them in particular manners, stressing how science will produce knowledge that contributes to business innovation and thus national economic growth.

Alas, the same argument is true for many critical approaches analysing the science/industry relation. For example, the literature on biocapital presumes that capital is a dominant force in the contemporary biosciences, and that capital has an (often corrupting) influence on scientific knowledge production. Biocapitalist critique thus decries leaks between two separate entities, reifying an ontological separation of science and industry where one (business) corrupts the other (science).

In both these approaches the values of science and knowledge are performed in completely different manners, albeit using the same ontological cut: ontologically separate, pre-existing entities that interact, even if their relations are seen as beneficial in one case and detrimental in the other. Like economic theory (Callon 1998), the tropes that these approaches purport to analyse are also articulated by them.

A possible inroads to rectify this analytical impasse could be to approach these tropes on science and industry by attending to the rhetorical production of the boundaries between them (Gieryn 1983, 1995). The question is then shifted from the reifying and normative question of ‘how pre-existing, and ontologically separate entities *should* interact’ to the analytical question ‘how these entities come to be seen as separate in the first place’. A powerful example of this is Gieryn’s introduction of boundary work, which calls for analysing how the boundaries of science are made in practice. The underlying assumption of this approach is that games of social interests, power, and authority explain how and why boundaries are erected. The boundaries around science are seen as performed, permeable, and contingent, but the explanation for them is sought in a presupposed power game, trying to explain ‘uneven distributions of authority, power, control, and material resources’ (Gieryn 1995: 441).<sup>3</sup> The pre-existing ontologies of the normative approaches such as triple helix or biocapital are here dismantled in favour of a powerfully contingent and performative account of the boundaries of science.

However, as is well known, the reliance on interests as an explanation has been widely criticized. For example, Susan Leigh Star (1991) has pointed out that this type of explanation assumes a Machiavellian understanding of scientists, where science is but an arena for great scientists to further their

<sup>3</sup> Another similar example is Callon’s work to explain science as a strategic game of enrolment and mobilization, where actors attempt to align other actors’ interests in order to serve their academic goals (Callon 1986).

own career interests. Another critique, developed by Steve Woolgar (1981), argues that interests become black-boxed in approaches using interests as explanatory strategy, leaving unsolved important parts of the work to understand science in action. Woolgar argues that a fruitful technique would be to utilize an ethnomethodologically inspired approach, and attend to the *making of interests* in practice. He argues that interests should become a topic for inquiry, rather than a resource for explanation. This strategy has been put into practice by Cori Hayden (2003) in her analysis of bioprospecting in Mexico, where she follows the making of interests: in research agreements, in intellectual property rights, in indigenous rights movements, etc. Rather than seeing interests as that which explains, Hayden investigates the making of multiplicities of interests in bioprospecting. Interests become something that needs to be investigated and understood.

A performative approach to both science/industry relations would shift the analytical searchlight from boundary work as a result of interests, to highlighting the making of both boundaries *and* interests. This would mean that we would avoid reifying interests as explaining action, and instead taking seriously the various and shifting ‘interests’ attributed to science in practice. This would sidestep the fundamental analytical problem of conflating analysis and critique of biomedical tropes: where does the empirical trope of corruption of science begin and where does a biocapitalist critique end? Thus, rather than contributing to the ‘normative surfeit’ (Zuiderent-Jerak, in press) often associated with analysing science/industry relations, this approach would be able to produce descriptions of the multiplicities of shifting boundaries and interests in science.

Rather than presupposing an ontological boundary between science and industry, and rather than presupposing social interests as the explanation, this chapter proposes to, in a pragmatic vein, pay attention to the actors’ parallel construction of *interests* and *science/industry relations*. Similarly to Viviana Zelizer’s (2012) recent work in economic sociology, this approach to analysing the science/industry relation would allow for a description of how actors engage in *relational work* to distinguish meaningful, valuable, and appropriate relations in the biosciences without recourse to pre-stabilized tropes on what science or industry ‘are’ and what Machiavellian scientists ‘want’. An important consequence of this move would be the acknowledgement of the ontological multiplicity and complexity of science and industry: the multiplicity of tropes, the multiplicity of interests in knowledge, and the multiplicity of organizational forms. Thus, just like Annemarie Mol (2002) has shown disease to be ontologically multiple, science and industry, with concomitant boundaries, relations, and interests, are here argued to be performed in practice, and therefore to be ontologically multiple and shifting. Thereby actors’ making of science/industry relations can coincide, clash, be hierarchized, and calibrated in different situations.

What is at stake in the making of interests and science/industry relations is nothing less than the making of the conditions of possibility of the biomedical sciences. The multiplicity of tropes, objects, relations, and boundaries that are produced around the science/industry nexus shape what it is possible, valuable, and desirable to do in the biosciences. The argument is that the making of these perform what is seen as productive at the lab bench, what is seen as valuable knowledge, how ownership and organization should be arranged, and which methods are seen as fruitful. Furthermore, the yardsticks—the ways of assessing value—for determining all of these things are at stake. What is valuable, and how do we determine this? Fundamentally, the question at stake is the negotiation, delineation, and coordination of several versions of ‘Good Science’.<sup>4</sup>

This chapter introduces and highlights a specific type of relational work in the science/industry nexus: that of *purification*. This concept emphasizes how actors in their relational work constitute science and industry as separate ontological entities in specific ways. The argument is that just as nature and society are purified (Latour 1999a), science and industry are produced as ontological entities through purification: assembling certain configurations of interests, boundaries, and values. In doing this the chapter identifies *two modes of purification* that are used to separate science and industry: first, a *temporal mode* of purification; and second, an *organizational* mode of purification. This suggests a multiplicity of strategies to fashion different versions of science and industry as well as a multiplicity of possible relations. To elucidate these themes, this chapter enquires into two evaluations of a research project and how they construct different interests in the project, which includes different boundaries between science and industry, different views on what is valuable knowledge, as well as different yardsticks for the laboratory bench.

## **Beginnings: A commercial database to find potential protein targets**

The case used to explore these themes is the peer review and evaluation of a large bioscientific research project, the Human Protein Atlas (HPA).<sup>5</sup> The

<sup>4</sup> This argument draws on the theoretical work of Laurent Thévenot and Luc Boltanski, who have worked extensively on justifications and valuations (Boltanski and Thévenot 2006; Thévenot 2007). However, the orders of worth that they produce are much too static to be able to capture the multiplicity of relations that are produced in the science/industry nexus.

<sup>5</sup> This inquiry started as part of a larger research project on the links between research and research policy in Sweden from 1960 onwards. It began as an open-ended inquiry into research of the political process and led to the establishment of the HPA project. The chapter is based on interviews and

HPA is a protein mapping project that today aims to chart all the genetically coded proteins in the human body to execute a ‘large-scale characterization of potential protein targets’ that can be ‘used to understand disease and develop new and more efficient drugs’.<sup>6</sup> That is, the project aims at finding potential protein-based targets for identifying, diagnosing, or treating disease, with special focus on cancerous diseases.

The project is to result in a map consisting of annotated images of protein locations in different tissue types (see Figure 11.1). These images are argued to be useful starting points for the development of medical diagnosis and intervention. Echoing the familiar innovation trope in the biosciences, the idea for the HPA project was articulated at the height of the genomic boom in the end of the 1990s and was tied to a commonly occurring dream of creating a proteomic ‘goldmine’ through pharmaceutical development (cf. Ezzell 2002; Service 2001a, 2001b, 2001c).

The beginning of this story takes place in a Swedish company called Affibody, which was founded in 1998 as a gamble on the medical and commercial success of certain synthetic molecules, Affibodies (interviews, Fredrik Pontén and Stefan Ståhl).<sup>7</sup> The company had two main research trajectories: to develop artificial antibody molecules that could be used to identify proteins *in vivo* (interview, Fredrik Pontén); and to produce in the style of Craig Venter’s Celera Genomics, a database and map of the proteins in the human body and offer it for commercial subscription (interview, Mathias Uhlén). The dream was to take the next step after the mapping of the genome: to go from mapping the genes to mapping the proteins (interviews, Mathias Uhlén and an anonymous researcher).

The feasibility of mapping proteins in human tissue was first explored in a preliminary study undertaken by Affibody. The study started in March 2001, and used antibodies to map 168 of the 225 proteins of chromosome 21. The preliminary study was deemed successful and was reported in a publication

analysis of documents around the HPA. The conflicts around the valuation of research, and methods that emerged as themes in the very first interview, I followed up in subsequent interviews. I conducted twenty interviews with (1) people who were affiliated with the Human Protein Atlas; (2) people who had insight into the funding process leading up to the project; (3) researchers who had utilized the atlas in different ways; as well as (4) researchers with insight into proteomics or antibody methods. The external informants had affiliations ranging from research foundations to international research organizations. The insiders (and to some extent the outsiders) were chosen based on being mentioned/recommended in previous interviews. The ‘outsiders’ were identified using interviews, contacts, and publications. All informants were offered anonymity: most declined, while some of the critical informants were adamant about being treated anonymously.

<sup>6</sup> Affibody Annual Report 2001 for the first quote, and KAW Annual Report 2006 (Knut och Alice Wallenbergs Stiftelse 2007) for the second.

<sup>7</sup> Affibody was named for an artificial and patented protein, an Affibody molecule, which is produced from a specific domain of ‘Protein A’. Affibodies can be described as artificially constructed antibodies or so called antibody mimetics which can bind to any protein.

(Agaton et al. 2003). For Affibody, and also later the fully realized project, the interest in what was called the proteome was articulated as a commercial potential that would be realized by selling subscriptions to access a database of proteins:

The platform is currently used to generate Affibodies™ and antibodies for large-scale characterization of *potential protein targets*. (Affibody 2001: 15)<sup>8</sup>

If we could develop 22000 Affibodies, one for each human protein, it would have been a goldmine. (Interview, Stefan Ståhl)

In the quote above ‘potential protein targets’ alludes to finding targets for medical intervention; that is, proteins that can be targeted in medical diagnosis and treatment. The articulated dream was to find the seeds for the next blockbuster drug. One might say that the interest was in creating a treasure map that showed the location of proteomic gold.

However, in 2001, in the wake of the dotcom crash—according to project mythology—Affibody’s board of directors decided to discontinue the study to focus on other ventures. The decision made it necessary for the project team to find other financial sources. This led the main scientist–entrepreneur, Mathias Uhlén, to pursue a project proposal with John Bell at Oxford University, whom Uhlén knew from a previous project. The proposal included moving the project from Affibody to Oxford with financing from the Wellcome Trust, and would entail a move from the commercial setting in which the project was born, to an academic setting at Oxford University.

## Temporal purification: Or the linear model in action

In the spring of 2002 discussions between the Wellcome Trust and Uhlén got under way, and negotiations about what was to become the HPA project began in earnest. A preliminary budget of about £100 million was outlined. A time plan for a 10-year Oxford-based project was drawn up (interview, Mathias Uhlén). During the spring of 2002, negotiations were held between the Wellcome Trust, the Swedish research foundation *Knut och Alice Wallenbergs Stiftelse* (KAW)—who wanted to keep Uhlén in Sweden—and representatives from the international pharmaceutical industry (notably Astra Zeneca).

As the negotiations progressed, the Wellcome Trust set up a peer-review process that involved a large number of scientists (12–20; sources vary) in a review of the proposal. According to Michael Morgan, who was in charge of

<sup>8</sup> My translation. All quotes from Swedish actors are henceforth translated by the author.

the negotiations at the Wellcome Trust, most of the reviewers were critical of the project. The reviewers, and the Wellcome Trust, argued that the value of the protein map, and the reason for doing the project, was to provide a resource to the academic community, rather than seeing the project as a route to a proteomic goldmine. The treasure map, intended to find proteomic gold, changed characteristics to become an atlas that was to be used as a scientific resource. As Michael Morgan recalls the review process:

It became clear that there were serious concern[s] being expressed by at least some of the scientific community about the feasibility of the proposals and the value to the scientific community. (Interview, Michael Morgan)

As an echo of Merton's CUDOS norms (1973a), the value of the project was seen as dependent on its usefulness to the academic community. Rather than commercializing the resulting data and tools, which was the original plan in Affibody, interest in the project had shifted to produce data and tools that could be used widely and freely. The importance of the wide-ranging usefulness of the project—not just for the economic benefits of the research group or its investors—was stressed. The importance of free distribution of tools and reagents and the free release of data was also emphasized:

Yeah, because this being a resource we wanted to make sure that there was no proprietary tools being involved, or reagents being involved, that we would not be able to distribute freely to the scientific community. (Interview, Michael Morgan)

The project was seen as producing a *free resource* for the scientific community. This stance on ownership was also reflected in a worry about leakages between the proposed project and the commercial activities of the Uhlén research group. As a UK charity, the Wellcome Trust was concerned with the link between the proposed research project and private enterprise:

There were issues of, around, the involvement that Mathias [Uhlén] had with . . . [Affibody] . . . And I think there were some concern[s] being expressed by our legal colleagues as to what exactly was the relationship. Because as [a] charity . . . in the UK, charities are not allowed to use their money to help support private enterprise. And so the legal team would have been very concerned to ensure that there was no possible leakage between, how should I put it, Mathias' academic activities and his innovative industrial activities. (Interview, Michael Morgan)

For the Wellcome Trust it was of utmost importance that they were supporting science rather than private enterprise. Legally, a clear demarcation was drawn between acceptable and unacceptable support. As the Wellcome Trust saw it, the purely academic nature of the project—i.e. ownership of data and tools, as well as the relations to Affibody—were vital for being able to finance

the project. The project needed to be legally and organizationally cleansed from its ties to the commercial sphere for it to become acceptable.

However, the dichotomy between the academic project and the commercial sphere drawn up by the Wellcome Trust legal team was more complex in the actual negotiations, as the pharmaceutical industry participated in the negotiations with the express purpose to be able to rapidly capitalize on the knowledge that was generated by the project (meeting-minutes, OE). The interest generated in producing pure knowledge for the scientific community was combined with an interest in making it possible for the pharmaceutical industry to utilize knowledge after the facts had been discovered.

The project was consequently carried out as a purified scientific activity where science and business were to be separated in the quest for creating an atlas of the proteome. However, as the pharmaceutical industry was invited to partake in the project, it was not a complete separation, but rather a *temporal purification*. The imagined project was enacted using the common 'linear model of science' (cf. Godin 2006) where the academy produces knowledge, and industry becomes involved in making knowledge commercially useful. Basic science was seen as an initial stage and industrial development, it was argued would evolve later. The linear model of science was rhetorically, legally, and organizationally enacted as the model of science.

However, eventually, and despite efforts to adapt the project to the linear model—to purify it—and despite efforts to defend the project against critics, the Wellcome Trust decided to decline to fund the HPA project. The number of interpretations for why this actually happened increased with the number of interpreters.

## **The goldmine of proteins returns—a hybrid science**

The Wellcome Trust, however, was not the only contender for financing Uhlén's Oxford project. As indicated above, the large private Swedish research foundation, KAW, had at the outset become involved in discussions on financing the project. The KAW foundation was established by, and still is tightly connected with, the Swedish industrialist family Wallenberg. The loosely formulated goal of the foundation is to fund research that would be 'of benefit for Sweden'. The Wallenbergs can be described as *the* industrialist family in Sweden and has (over more than a century) had a key role in Swedish business, politics, and science. The board of KAW consists of researchers as well as a number of members of the Wallenberg family and business empire.

Erna Möller, the immunologist who was the executive board member of KAW at the time, explains how they saw a partially different set of interests as the reason for mapping the human proteome. As in the Affibody preliminary

study, the *raison d'être* of the project was to screen for medically interesting proteins that could be used for identifying diagnostic biomarkers for cancer:

The important thing is if they [the Protein Atlas] identify a protein that is typical for something... And perhaps later it becomes obvious that it is something incredibly interesting that only exists in one cell, or during a certain part of the cells' developmental stage, or perhaps in a specific form of tumour. And perhaps not in all tumour forms, but some that only exist in the most malign? These could become really important things. (Interview, Erna Möller)

Here, as in the Affibody preliminary study, interesting protein structures were seen as being biomarkers for cancer treatment and diagnosis. As in the Wellcome Trust case, these arguments were also accompanied by a specific articulation of ownership and intellectual property rights. However, for KAW, the linear view of science that the Wellcome Trust articulated seemed to miss the point. For KAW it was seen as a positive development that companies were founded on research output. A valuable project pinpointed interesting and valuable protein property, and founded biomedical and biotechnological companies. The atlas shifted back to a treasure map. Möller explains the stance:

Wellcome followed the principle that everything should be free, and we accepted that. But we thought it was a shame, as the researchers had the possibility to develop and found their own companies.

During the course of the investigation, when you find a new exciting antibody, then you say that this is something that shouldn't be sent out immediately, but it should be looked over. Should it be stowed away? Patented? (Interview, Erna Möller)

KAW argued that the development of patents and companies on scientific results was the model for pursuing the HPA project. Thus, Möller and KAW argued for a different version of the project from the linear model proposed by the Wellcome Trust. For KAW, just as for Affibody, the value of science was tied to the treatment or diagnosis of cancer and the establishment of commercial actors. The goal was market-oriented interventions in the biomedical realm. The gauge for a good project was joint medical and economic development. In performing this version of science, the actors made bioprospecting for proteomic gold the yardstick for science.

## **Yardsticks in the lab: Interests, epistemic value, and methodological assessments**

Let us now attend to how the performance of interests played out in the valuation of knowledge. How 'value for the scientific community' or finding 'medically interesting proteins' entered into the assessment of knowledge and

yardsticks for the proposed methodologies. As is customary in any scientific evaluation, peer-review processes were put in place by the two research foundations involved. The evaluation of the proposed methodology revolved around the usefulness of different types of antibody molecules, the criteria for usefulness, and how appropriate they were for achieving different articulated interests. What I highlighted here are not the inherent characteristics of different types of methodologies, but rather the making of interests, measures, and values in the review process and how they pertain to the valuation of lab work. The focus is on how the relational work of pursuing interests in the science/industry nexus intertwines with the values of the lab bench.

Let us first consider the Wellcome Trust and their articulated interest in producing a protein map ‘for the scientific community’ and how this played out in evaluating the lab work of the proposed project. As is commonly the case, the reviewers’ critique was not solely focused on organizational forms or on the ownership of data or tools. The map was deemed interesting as a resource for unknown ventures. Thus, the review and evaluation of the project were predicated on an understanding that *the entire map* was potentially of scientific interest. Deciding on which proteins (and parts of the map) were valuable was left unarticulated in the review process—the interest was left for future researchers to decide. It was the atlas view of the project that carried the day. Further, the idea that the project should become a widely used resource was tied to specific yardsticks for evaluating the proposed methodologies. These arguments point us towards the assessment of different types of antibodies—which were the tools of the project. Michael Morgan of the Wellcome Trust again:

First of all there was a question as to whether or not the approach was the most appropriate one. I remember there was discussion about monoclonal antibodies [produced in cell-cultures] versus antibody being raised in rabbit... And the question about monoclonals of course is that it becomes a *permanent source of material*, whereas antibody raised in rabbit is sort of a *one-off exercise*. (Interview, Michael Morgan)

The tension highlighted here was between the wish for a ‘permanent source of material’ versus doing a ‘one-off exercise’. The argument from the Wellcome Trust reviewers was that interest in the project hinged on the production of identical batches of antibodies that could be used within the scientific community as a permanent resource for further research. This articulation of the project tied into a long understanding of so-called monoclonal antibodies, which can be produced in laboratory cell-cultures and thus produce an eternal supply of identical antibodies to the common view that monoclonal antibodies make it possible to ascertain, through repeated experiments, that they do what they should. They are then said to be specific or to bind specifically. As one of

the informants, Hans Wigzell—who is also the former Vice-Chancellor of the Karolinska Institute and one of the founders of Affibody—expressed it in the 1984 Nobel Prize presentation speech for monoclonal antibodies:

Köhler's, and Milstein's development of the hybridoma technique for production of monoclonal antibodies have in less than a decade revolutionized the use of antibodies in health care and research. Rare antibodies with a tailor-made-like fit for a given structure can now be made in large quantities. The hybridoma cells can be stored in tissue banks and the very same monoclonal antibody can be used all over the world with a guarantee for eternal supply. (Wigzell 2012)

The value attributed to monoclonal antibodies in the biosciences was tied to the possibility of producing an 'eternal supply' of identical antibodies which allowed an experimental replicability, and a possibility for standardization and packaging. Monoclonals were supposed to 'revolutionize' the 'tinkering' with antibodies into a tool 'that could yield standardized, reproducible results' (Cambrosio and Keating 1992: 369).<sup>9</sup> Thus, the value of the antibodies in the Wellcome Trust's review process was tied to an eternal supply of monoclonal antibodies. Furthermore, as many of the proteins in the body are unknown, that fact that the project was to produce monoclonals for all genomically coded proteins in the body was articulated as of immense value to laboratory research: both as a location map of proteins in tissue, and as an eternal source of tools (so-called reagents) for further lab work. The map, from a value perspective, was seen as a homogeneous entity, an atlas of the proteins. The map, the unknown proteins on it, and the antibodies were given homogeneous epistemic value.

As I have shown above, KAW's reviewers, on the other hand, tied the project to a completely different set of interests: that of rapidly screening for interesting proteins. The project was to do a first pass through the proteome in order to identify proteomic real estate for patenting, and only go into depth for certain interesting proteins. The last step would then be to execute the costly process of developing monoclonals. Recalling Möller's words:

I was completely floored by it being possible... This [project] is insane! If this [methodology] works it is a hundred times faster and better than... making monoclonals en masse. And if you have an antibody and know what its target is: it's as easy as pie to take the protein and make a monoclonal. But then you only do it for the maybe one per cent of all proteins that are interesting (Interview, Erna Möller).

<sup>9</sup> The use of polyclonal antibodies was sometimes described as bordering on an uncertain and unscientific 'black art practiced by immunologists'. According to Cambrosio and Keating this division between monoclonal and polyclonal antibodies echoed a division of immunologists into 'those who believed in immuno-chemistry and those who believed in "immunomagic"' (Cambrosio and Keating 1995: 74).

Thus, in KAW's assessment of the project, the proteins were given a heterogeneous epistemic value. Interesting parts of the proteome were ascribed higher epistemic (as well as medical and economic) value. The enactment of this particular interest in the project made other yardsticks for assessing antibodies salient. John Bell who was supposed to host the HPA project in Oxford, and who championed the project at the Wellcome Trust recalls:

[Mathias Uhlén] had enough data to suggest that you could get a *pretty good* monospecific reagent out of more than half of the genes you look at, *which was enough*, and that's from the *first pass*.

But, the trouble is that if you try to think in high-throughput terms—that's the way you think—you'll immediately not get there because [monoclonal antibodies are] too *cumbersome*, it's too *slow*, it's a hell of a lot of *screening* you've got to do well. This [polyclonal] methodology is much, much more powerful. (Interview, John Bell)

The idea of doing a high throughput, 'pretty good' first pass of the proteome in order to discover medically interesting proteins was tied to a completely different epistemic valuation involving completely different yardsticks for the assessment of the antibodies. In the review process, the project's production methods became articulated as speedy, easy, and efficient. KAW and others argued that it would be inefficient and costly to produce monoclonals for all of the proteome, and that it would be better to produce them for the economically and medically interesting parts of the proteome. *The project was on a treasure hunt, not a topographical survey*. Briefly, KAW and the project team contended that the methods made it possible for the project to quickly and efficiently produce polyclonal antibodies that in the next step could be used to identify proteins in specific tissues—to find the coveted location of interesting proteins in the human proteome. A good map was a treasure map, and a good antibody was no longer articulated as being replicable and eternal—rather it needed to be simple, fast, and cheap.

Thus, the differing interests attributed to the project were tightly connected to the articulation of both epistemic value and methodological yardsticks. In sum, the arguments were: 'a map to be used as a resource for further scientific discovery needs to be replicable and specific' vs 'a map for bioprospecting needs to be efficient in identifying valuable real estate'. The yardsticks deployed to assess monoclonal antibodies, eternal replicability for the scientific community, was contrasted with antibodies that were sufficiently accurate, cheap, and fast for bioprospecting for proteomic gold.

## Performativity and purification: Redrawing the boundaries of science and industry

In the fully funded and running project, KAW's and the researchers' articulation of speed, ease, and efficiency was highly performative. For example, the production of antibodies was outsourced to a factory in China to get the speed up and the production costs down:

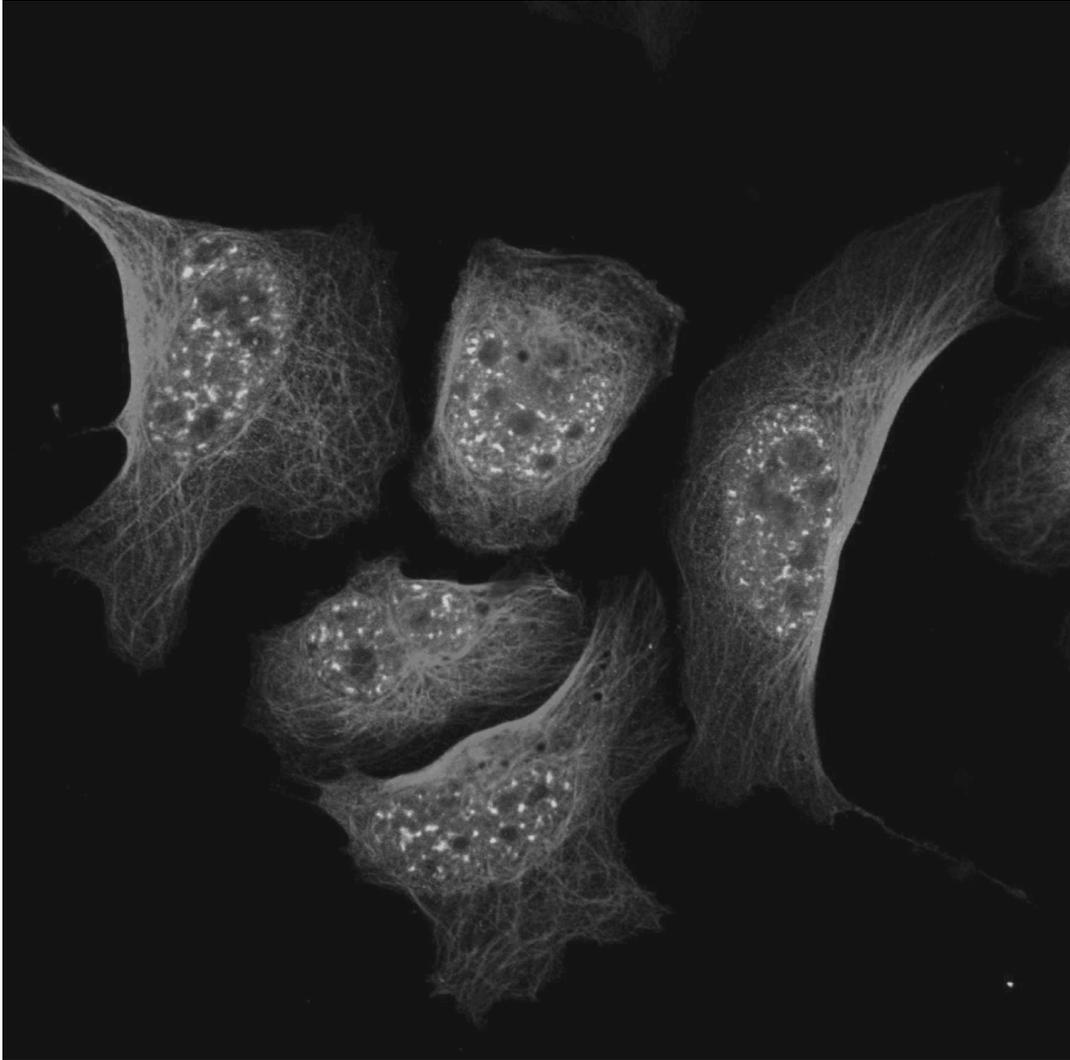
I had suggested to Mathias [Uhlén] that he needed to look at very high throughput methodologies for producing antibodies and he talked about this centre . . . in China as a way of really producing polyclonals *very rapidly*. So, since then he started by making antibodies in Scandinavia and I said 'No, you're never going to get there.' And he's now developed I think very good collaborations with the Chinese. That's really *got the price down* on making the antibodies. (Interview, John Bell)

Furthermore, the annotation of the images resulting from the atlas (see Figure 11.1) was outsourced to Indian pathologists. The argument for this was that foreign organizations were cheaper and more motivated to perform the type of monotonous work necessary for producing high-throughput analysis.

Moreover, the interest in commercializing the intellectual fruits of the HPA mapping project was performative in the complex ownership relations developed for executing the project (see Figure 11.2). The patents and intellectual property that the university-based HPA project generated were transferred to a holding company, *Atlasab Intressenter*. The IP holding company in turn owned a stake (32 per cent) of Atlas Antibodies, which was founded to commercialize the IP generated by the project. Atlas Antibodies was also partly owned (32 per cent) by a research foundation (controlled by the HPA-researchers) which was established to finance research at the participating universities as well as two university holding companies (6 per cent). Two venture capital companies held the last stake (30 per cent) of Atlas Antibodies; the Wallenberg family controlled one of the venture capital companies, Investor Growth Capital.

The plan was that the results should be freely available. Now this didn't happen as they [the group] patented some interesting finds. And that was good. Because this is a possibility for Atlas Antibodies to sell. (Interview, Erna Möller)

Nevertheless it is important to underline that KAW did not view science as being the same thing as business. The separation between science and business was rhetorically and practically upheld. For KAW the complex organization of investment and ownership led to an undesired ambiguity, where the relations between science and industry needed to be clarified. Here, the connection between the KAW and the Wallenbergs' industrialist legacy was important in that the separation between the foundation and the venture capital arm of the Wallenberg family empire, was stressed. Just as for the Wellcome Trust, the



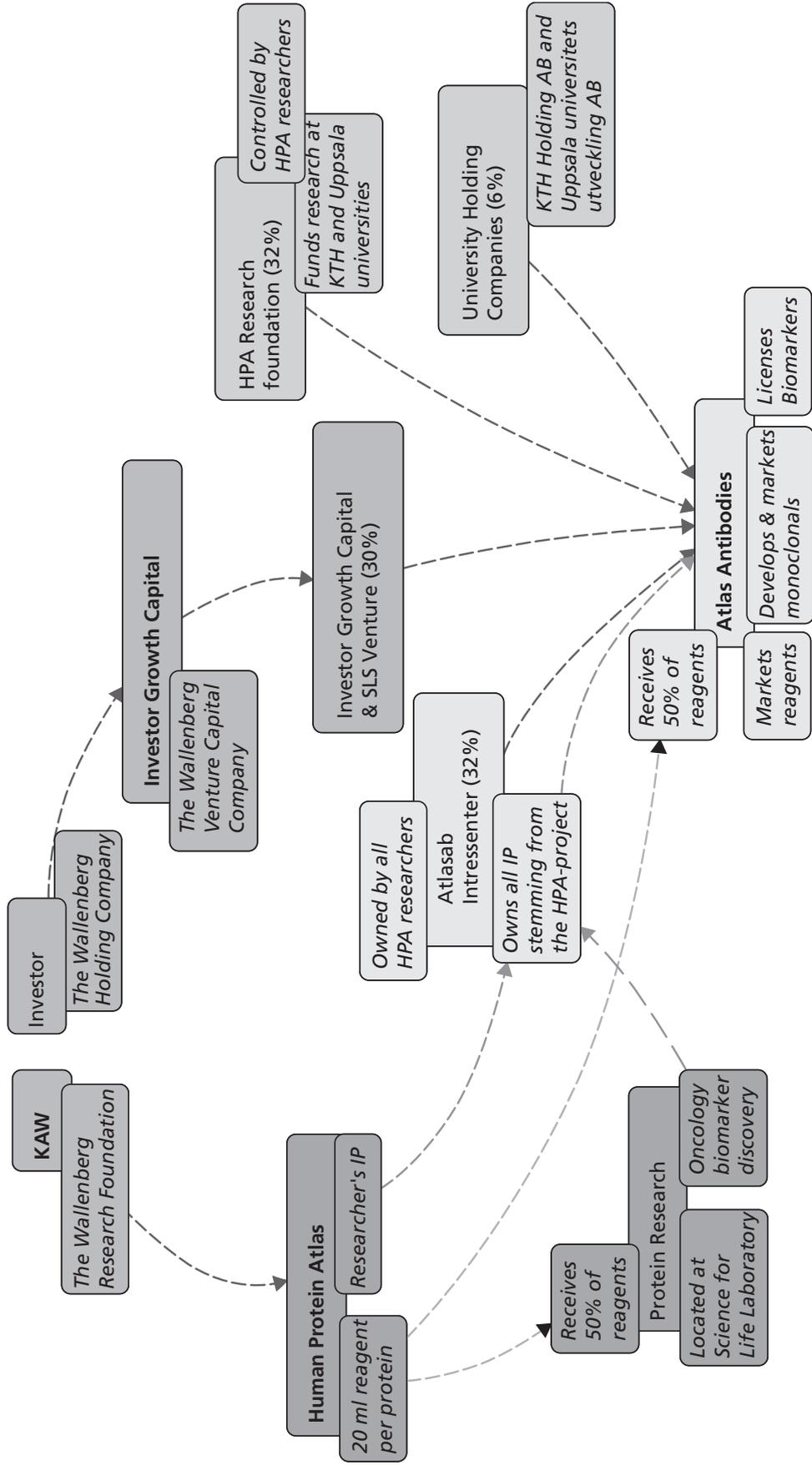
**Figure 11.1.** Immunofluorescence in human cells

Image from the Human Protein Atlas. RBM25; HPA003025: U-2 OS. See <<http://www.proteinatlas.org>>, accessed 27 May 2014.

project was still articulated as scientific—not primarily a matter of industrial development:

We just think it's nice if we get some nice business out of it, but never, no interference. And no money back. On the contrary. That's why there was some discussion when Investor Growth Capital [the commercial sister of KAW], wanted to invest. And then we said that they could not be lone investors. And they [Investor Growth Capital] had a hard time understanding that. Because they don't think of themselves as the foundation... They are completely separate. They never ask us for advice. But I thought it was very good that there were other investors.

I don't know if Health Cap and Investor Growth Capital will get back their money on this. I don't know...I know very little about Atlas [Antibodies] because for us it's a clear-cut case: We give money. You develop. We have absolutely no... we don't want to interfere. (Interview, Erna Möller)



**Figure 11.2.** A schematic representation of the complex organizational structure that was developed

Thus, Möller and KAW articulated both a hybridized science/industry relation, and a simultaneous purification of science and industry. Investments in science should be kept separate from commercial development. The purity of the two spheres was upheld. However, rather than enacting a temporal purification, as the Wellcome Trust, Möller and KAW stressed an *organizational purification*, where the Wallenberg's science foundation, KAW, and their venture capitalist arm, Investor Growth Capital, did not mix their business. Rather than pursuing the linear model of scientific development, KAW enacted a division of labour founded on different roles in developing and commercializing science. The boundary between science and industry was enacted with different boundaries for what was deemed acceptable relations, what was seen as interesting knowledge, and what was seen as productive methodologies.

## **Relational work, interests, and epistemic value: Some concluding remarks**

This chapter makes a lateral move in analysing the tropes of the science/industry nexus. How do we understand that the biosciences are constantly articulated in a multitude of conflicting manners? At one point they are articulated as being for innovation and national economic innovation; at another time and place they are articulated as a fundamental science by the same actors; and elsewhere, further down the road in a new situation, they are pronounced a production factory for providing new means to clinical ends.

This chapter has argued that by attending to the performance of these tropes, the performance of these interests, in the biosciences, we might provide an opportunity for avoiding the all too familiar narratives of self-interested scientists; a space for fashioning empirically sensitive stories that attend to the multiplicities of interests and values that scientists produce and relate to in practice. The tropes of medical development, economic innovation, and scientific progress are all present in the biosciences, and scientists perform and relate to all of them at different junctures.

By exploring the prolonged valuation of a large-scale protein mapping project, the HPA, the chapter has shown how the performance of different interests in science produce profoundly different valuations of science: with effects on what is seen as epistemically valuable; which yardsticks should be used for evaluating methodologies; and how laboratory work should be organized.

The mapping of the proteins—and by extension this may apply to any bioscientific project—does not have unambiguous value or interest. Is it a treasure map that marks proteomic real estate that has potential for medical

intervention and economic development? Is it an atlas, a topographical survey, where all proteins are potentially valuable? What is an interesting and valuable map? And how should it be produced?

This chapter has proposed to attend to the making of interests and values in terms of *relational work* and different *modes of purification*. Thus, it attempts to understand how actors produce science through their interest-work and value-work. Following this line of inquiry, the chapter proposes two modes of purification in order to understand the strategies that actors use to uphold the difference between science and industry: one *temporal mode of purification*, and one *organizational mode of purification*. Inspired by Annemarie Mol (2002) the chapter suggests that it might be fruitful in the future to attend to further modes of relational work such as coordination, clashes, hierarchies, and calibrations.

By looking closely at the valuations of science we can begin to understand the epistemic shifts, breaks, and reorientations of the academic hierarchies of university research. How should science be done? For whom? With what purpose? The HPA is not unique in this manner, but rather one of many that question the disinterestedness of science and belies the boundaries between scientific work and commercialization.

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