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"How Many Individuals Consider Themselves to Be Cell Biologists but Are Informed by the Journal That Their Work Is Not Cell Biology"**

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Summary: What can we gain from co-analyzing experimental cultures, regionalization, and disciplinary phenomena of late twentieth century life sciences under our historiographic looking glass? This essay investigates the potential of such a strategy for the case of cell biology after 1960. By merging perspectives from historical epistemology inspired by the work of Hans-Jörg Rheinberger with a focus on boundary work in the realm of scientific publishing, community building, and disciplinary norms, a set of understudied scientific practices is exposed. These practices, historically subsumed under the label *descriptive*, have been as central in cell biology as hypothesis-driven research aiming at mechanistic explanations of cellular function. Against the background of an increasing molecular-mechanistic imperative in cell biology since the late 1960s, knowledge from *descriptive practices* was often judged as having low value but was nonetheless frequently cited and considered

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essential. Investigating the underlying epistemic practices and their interactions with disciplinary gatekeeping phenomena (as policed by journals and learned societies) provides historiographic access to the plurality of experimental cultures of cell biology, scattered into many interdisciplinary research fields—with some of them only partially engaged with mechanistic questions.

Keywords: History of the life sciences, history of cell biology, historical epistemology, experimental cultures, description, mechanistic explanation, gatekeeping

1. Introduction

Modern cell biology has been described as an initially transdisciplinary venture between cytologists, biochemists, and biophysicists that developed through phases of formation (1930s–1940s) and maturation (1950s–1960s) into an established scientific discipline (1970s), interested in explaining cellular functions in mechanistic terms.[1] This development was driven by an increasing molecularization on the one hand, and an elaborated "heuristic of form" on the other hand. This heuristic of form refers to morphological knowledge as a guidance for meaningful experimental perturbation of cell functions, interpretation of results, and iterative hypothesis refinement.[2] But if you consider the many epistemic (and institutional) realms of researchers who self-identified as cell biologists after 1960, this clear-cut picture blurs. Contemporary statements of researchers about their professional experience in the realms of cell biological scientific communities point to a tension between self-identification and external judgment by scientific institutions such as funding agencies or journals—exemplified by the quote from 1976 in the title of this essay, referring to the *Journal of Cell Biology*. Taking this tension as a trajectory for historiographic scrutiny, a variety of widely applied scientific practices is exposed that does not fit into the idea of a modern cell biology typically engaged with hypothesis-driven interventional experimentation to provide mechanistic explanations on the molecular level. A study of one episode of US-American cell biology in the 1970s, the adoption of immunofluorescence microscopy as an epistemic tool for cell biologists and its role in establishing cytoskeleton research as a sub-field of cell biology, identified phases of substantial re-descriptions of this subcellular entity.[3] The strong dissenting opinions on *description* expressed in sources and interviews prompted a further investigation of this topic. As opposed to a mechanistic imperative perceived by some contemporary witnesses or expressed in various sources criticizing the non-mechanistic, one notion stands out as a signifier for research not fitting

[1] Bechtel 2006, on 14.

[2] Matlin 2022.

[3] Worliczek 2020.

the mechanistic ideal: *descriptive* approaches. Initially understood as a neutral epithet to morphological cell research in the 1950s, this notion of reviewers and journal editors has since then differentiated into a receptacle for studies insufficiently fulfilling demands for experimental-interventional research aiming at mechanistic explanations into a dismissive label for manuscripts to be rejected by journals. To untangle this phenomenon as a manifestation of scientific cultures and their epistemic norms, disciplinary and post-disciplinary developments need historiographic analysis alongside synchronic and diachronic regional phenomena, referring to both location-specific developments and the plurality of research fields.

2. Experimental Cultures vs. Disciplinary Boundaries—a Contradiction?

Investigating experimental cultures as conceptualized by Hans-Jörg Rheinberger[4] enables the trajectories of scientific practices that had been subsumed under the label descriptive to be unraveled. At the same time, the non-laboratory practices, including publishing, funding, and career development, motivations for mobilizing this label, and its consequences for the whole spectrum of scientific work call for putting historiographic attention to a framework usually attributed to the logic of scientific disciplines. This might be even more important when inter- or post-disciplinary dynamics of research fields are considered. To provide the binding glue for scientific communities, such settings might require even more epistemic norms and boundary work[5] to maintain themselves. In the case of cell biology, geographical differences must be considered as well. While its practitioners established a strong disciplinary and departmental identity in the US between the 1970s and 1990s, the situation in West Germany, with its strong traditional dedication of university chairs to zoology, botany, or physiology, was considered a major obstacle for the interdisciplinary subject cell biology.[6] Consequently, individuals had to subject themselves to the disciplinary logic and its cultures at their institution while simultaneously living up to the cultural logic of their research fields.

Rheinberger recently emphasized characteristics of the sciences to be considered in his long-established approach on "cultures of experimenting," three among them central to the topics under discussion here: first, scientific work processes as embedded in the conditions providing the realm for their articulation and "creative reconfigurations" that bring about new knowledge. Second, the "non-reducible plurality of epistemic targets and ways of approaching them" fruitfully "erode science from within," leading to the creation of productive "intercultural zones." And third, experimental cultures are not "passively subjected to historical change" but rather facilitate and

[4] Rheinberger 1994, on 408, 416.

[5] Gieryn 1983.

[6] E. g., Zarnitz 1968, on 83.

provoke transformation.[7] He furthermore reinforced a focus on epistemologically defined experimental cultures that constitute the generation of knowledge in its informal dimensions—but disqualifies dynamics of scientific disciplines as historiographically informative in this context. Disciplines, defined through institutionalization and transgenerational transmission of knowledge, do not give justice to the circulation of techniques, epistemic objects, and the skills of individuals migrating within networks of experimental cultures.[8] This "regionalization" (in Gaston Bachelard's words)—or "fragmentation" of research cultures—is a prerequisite for the epistemic flexibility[9] necessary to firsthand establish a phenomenon and to transform it into an epistemic object. I will argue that, additional to analyses of its experimental cultures, an inclusion of disciplinary boundary work is central to a historical-epistemological approach that gives justice to the historical trajectories of cell biology, but also to the characteristics in need of consideration according to Rheinberger.

3. Descriptive Research as the Poor Cousin of Mechanistic Studies

Observation and description as scientific practices in their own right for investigating phenomena of life are especially well-studied[10] for periods predating the experimental turn around 1900.[11] However, they did not receive strong attention in the history and philosophy of late-twentieth century experimental life sciences, as opposed to (still sporadic) studies of other practices associated with natural history, like collecting or comparing.[12] The historiography of modern cell biology mainly covers the establishment of a discipline with its constitutive instruments, dominant epistemic characteristics in the realm of mechanistic explanations, the role of metaphors, foundational dynamics in the earlier twentieth century, and lately also visual epistemic modes in the latter half of the twentieth century.[13] The coverage of descriptive practices from the 1960s onwards, however, remains marginal. Practices traditionally understood as descriptive were incorporated into the idea of modern cell biology from morphologically oriented cytology. Cellular morphology went through a substantial modernization with the invention and increasing application of electron microscopy in biology since the 1940s. The experimental-interventional contribution to cell biology, on the other hand, was rather attributed to biochemists and cell physiologists.[14] The maturation of

[7] Rheinberger 2021, on 188–189.
[8] Ibid., on 185–186.
[9] Ibid., on 187–188.
[10] E.g., Daston and Lunbeck 2011.
[11] Allen 2005.
[12] E.g., Strasser and de Chadarevian 2011.
[13] Rasmussen 1997; Bechtel 2006; Reynolds 2018. See Matlin et al. 2018 for the broad spectrum of current HPS-perspectives on cell biology.
[14] Rasmussen 1997, on 113–148.

cell biology as a "functional domain of biology" was characterized by a thorough integration of studying form and function together in the 1960s, culminating in the "mission" of mature cell biology: "determining the mechanisms that enabled organelles to perform their functions."[15] While it is widely acknowledged that "[a]ll mechanistic explanations begin with (a) the identification of a phenomenon […], (b) proceed by decomposition into the [relevant] entities and activities, and (c) give the organization of entities and activities by which they produce the phenomenon" (recomposition), Phyllis McKay Illari and Jon Williamson emphasized that this process of "mechanism discovery is often messy and iterative" and that the phenomenon in question needs thorough description (and sometimes redescription) "to make it susceptible of mechanistic explanation."[16] For modern cell biology, Karl Matlin highlighted the central importance of (i) a mechanistic hypothesis generated from "ancillary knowledge," (ii) a "heuristic of form" guiding decomposition and recomposition, as well as (iii) experimental perturbation of parts to unravel their function.[17] The process of discovering a mechanism can last several decades, involving researchers from several academic generations and institutions;[18] it consists of various lines of discovery, and phases of technology development entail a repetitive re-establishment of the phenomenon in question by descriptive practices.

Yet, a strong historiographic emphasis on hypothesis-driven and experimental-interventional research in combination with the idea of a progress from descriptive morphological cell research to a mechanism-oriented "functional domain of biology" may lead to the contribution of practices apparently not fulfilling this epistemic ideal, scattered across or even central to the many asynchronous developments in the plurality of research fields subsumed under the umbrella of cell biology, to be overlooked. Consequently, the regionality of cell biology might be lost as an analytic perspective. First, phases of establishing or re-establishing a phenomenon and characterizing its potentially constituting entities tend to be treated as pre-history and consequently excluded from the in-depth historiographic discussion. Second, descriptive knowledge categorized as "ancillary" is nonetheless central to the idea of mechanistic knowledge according to Illari and Williamson. Third, explorative research,[19] i. e., hypothesis-generating inquiry and demarcating phenomena of interest for functional research remains understudied, especially for the second half of the twentieth century.

If there has been a dominating mission of cell biology as a whole, one might ask from a critical perspective if this has been true for all cell biologists whose publications have received substantial attention at a certain point in

[15] Bechtel 2006, on 2–4, 13.

[16] Illari and Williamson 2012, on 123.

[17] Matlin 2018, on 276.

[18] "Discovery" and "re-discovery" might only be addressable post-hoc with all the anachronistic pitfalls. Often such processes were not guided by explicitly constructed contemporary research programs.

[19] Including exploratory experimentation, see Steinle 2016, on 301–338; Colaço 2018.

time—especially for researchers whose work provided the descriptive basis for mechanistic hypotheses and subsequent interventional experimentation focused on cellular function. But this work equally fed into other practices like comparing, exemplifying, and sorting.[20] The historiography of cell biology has recently been further elaborated along the trajectories of a process gaining traction in the 1970s that led cell biologists to redefine their field as "molecular cell biology"—replacing molecular biology as the disciplinary construct that formulates and answers the most relevant biological questions on a cellular and molecular scale, and even ousting molecular biology to a mere portfolio of techniques. This interpretation is substantiated by the notion that cell biology, as opposed to molecular biology, never lost contact to the context of the cell as the space that enables all cellular functions.[21] These findings by Karl Matlin underline the relevance of historiographic studies of describing and defining this cellular context—including cellular morphology and so-far unidentified practices.

To assess their historical role, we should mirror the mobilization of knowledge and technical-turned epistemic things derived in such descriptive phases with the dominant epistemic imperatives of journals and learned societies. Such knowledge was frequently cited but still judged (contemporarily or retrospectively) as having low priority and value, as well as giving only low prestige or little academic merit to its producers.[22] Such cases indicate the respective knowledge and its experimental systems as essential for a research field and as everyday business for some cell biologists or labs. By investigating them we gain access to phenomena of gatekeeping targeting certain epistemic styles and its potential collateral consequences (funding, career development, loss of expertise by people leaving the field) for those not fitting the mainstream while still providing the essential knowledge base. Investigating such phenomena exposes the plurality of motivations, talents, aims, and resources of individual actors, labs, or research groups that allowed for contributing meaningful non-mechanistic knowledge and methods. Historians of late-twentieth century experimental life sciences are nonetheless confronted with a lacuna: clear criteria for identifying practices understood as descriptive in this period are missing. Actor's terms and discursive constructions offer an entry point for identification.

[20] Strasser and de Chadarevian 2011; Pickstone 2011.

[21] Matlin 2022.

[22] For a critical reflection by a practitioner of electron microscopy on this topic, see Bell 1985, on xi.

4. The Descriptive in Actors' Categories

Aiming at a typology of descriptive practices in the latter half of the twentieth century, an expert interview-based study[23] was recently conducted by the author of this essay. It revealed patterns of a discursive construction of the label *descriptive* within cell biology. The interviewees reported a perceived introduction of a hierarchy of epistemic styles by creating the label *descriptive* as a signifier for research less valuable than other modes of scientific inquiry and therefore in many cases inappropriate for the most prestigious journals, starting already in the late 1960s. Scientific work has been labelled *descriptive* when it did not test hypotheses, investigate cellular functions and dynamic processes, explain mechanisms on a molecular level, use experimentation that perturbs cellular functions, or provide quantitative data. Some practices emerged that were directly called descriptive in, for example, referee's reports: explorative studies, pure morphological approaches, describing the (supra-)molecular architecture of cell components, reporting method development, observational studies based on visual evidence, or producing atlases, databases, and handbooks. The label *descriptive* has been frequently used in practices of gatekeeping in scientific publishing and funding—to police (often implicit) epistemic imperatives by dismissal of so-called insufficiently mechanistic approaches.

The interviewees furthermore explained strategies for evading dismissal: (i) selecting journals not excluding descriptive work or actively inviting it; (ii) justifying their descriptive work on a pressing question by exemplifying the usefulness of a presented new method to approach relevant questions that are currently not addressable due to technical limitations; (iii) having an achievement at hand that promises to be of outstanding importance for any prospective functional questions; (iv) knowing the editors and their (normative) desires, and (v) formulating post-hoc hypotheses. As one interviewee put it: "[…] you can word it in a way where description is hypothesis driven. And if you are not wording it right then you can get accused of [being merely descriptive]." Moreover, written sources[24] from the 1950s to 1970s showed a stark contrast between the US, the UK, and Germany regarding the status of morphological research on cells, which is historically the most explicit instance of description.

And while there is no doubt from an analytical viewpoint that representations of (molecular) mechanisms—in diagrams or through text—are an instance of description, these findings show that analyzing actor's terms and discursive constructions unveils a realm of the descriptive in the history of cell

[23] Twenty-one semi-structured narrative expert interviews conducted without a focus on 'descriptive research' (topic: microscopy ca. 1970–today), and nine narrative expert interviews with an openly declared focus on dismissing descriptive research. Interviewees were cell biologists, journal editors, and imaging specialists active from the late 1960s until today in the US, UK, and Germany. Interviewees gave informed written consent on being interviewed and the further use of audio records and transcripts.

[24] Journal scopes, editorials, published reports of funding agencies, minutes of editorial meetings, personal correspondence of editors.

biology involving a complex spectrum of scientific practices[25] not solely addressable by an analytic lens on representation of mechanisms, pictorial evidence, or descriptive modelling.[26]

These preliminary findings identify practices that have been essential to the development of certain research fields and fall into the category of *descriptive* at different timepoints in post-1960 cell biology. Their value was not unanimously acknowledged by contemporary peers and gatekeepers of publishing and funding.[27] Yet, these practitioners published their results, supported by strategies to evade dismissal as *merely descriptive*—and were frequently cited. Such evasion strategies, however, required familiarity with gatekeeping modalities and consequently access to backstage knowledge if inclusion and exclusion criteria were not made explicit (for example in journal scopes or editorials) to the non-initiated—including newcomers to the field and people without first or secondhand experience as editors and referees. If one was not a member of a certain in-group with the necessary access to otherwise opaque backstage knowledge, their work might appear peripheral to the high-prestige epistemic mainstream at first sight but is still historically essential. Considering the implicit quality of such gatekeeping dynamics, their impact on the epistemic orientation of cell biological research trajectories might appear historiographically inaccessible. Interviews and the analysis of sources often associated with disciplinary dynamics such as institutionalization, gatekeeping, transgenerational transmission of implicit knowledge, and initiation processes offer such access. This spotlights interview questions about gatekeeping and a set of written sources to investigate the realm of articulation for historical epistemology: published journal scopes, instructions for reviewers, records of editorial debates and decisions, papers of learned societies, and referees' reports. Expanding historical-epistemological inquiry interested in dynamics of experimental cultures along these lines gives justice to the regionalization of inter- and post-disciplinary research fields in cell biology while including the simultaneous necessity of disciplinary boundary work and its epistemic impact as a highly relevant context of knowledge production.

In the case of post-1960 cell biology, only such an entangled focus identified a spectrum of practices central for epistemic developments under the umbrella of cell biology—discursively labelled as descriptive approaches by cell biologists. They appear nested in the plurality of research fields in an asynchronous, re-occurring, and iterative manner, strongly entangled with cultures of the technical when new methods are considered. The experimental cultures constituting epistemic styles behind these practices await historiographic attention, as well as the detailed consequences of gatekeeping modalities.

In a current perspective, such historiographic work might provide insight into the historical foundations of ongoing debates among cell biologists about the downsides of an epistemic authority expressed through a primacy of

[25] See Findl and Suárez 2021 for a philosophical analysis of descriptive understanding.

[26] See Ankeny 2000 on descriptive modelling.

[27] See Haufe 2013 for an in-depth discussion of hypothesis-driven imperatives at major funding agencies.

molecularized mechanistic research, where highly successful researchers advocate for valuing descriptive and explorative work as a basis for innovation.[28]

5. The Regionality of Cell Biology vs. the Necessity of Normative Boundaries

Some US-American cell biologists of the 1970s saw themselves confronted with an opaque epistemic authority that told them their work is not cell biology. Enough free-text comments in this fashion criticizing the editorial policies of the *Journal of Cell Biology*, the periodical sponsored by the *American Society for Cell Biology*, were received during a survey conducted by the society in 1975 to flag this topic as a substantial area of concern.[29] This identifies such concerns as a prevalent historic experience. While cell biology has been fundamentally interdisciplinary, cell biologists apparently developed certain epistemic standards used for inclusion or exclusion within their scientific community. I argue that we would gain much in our historical understanding of the experimental life sciences after 1960 by integrating a perspective on experimental cultures—or *regionality*—with one on dynamics usually attributed to the formation and maintenance of scientific disciplines. This seems especially appropriate for cell biology with its many specialized research fields and consequently its scientific sub-cultures, each of them a melting pot for individuals often trained in different, but explicitly disciplinary settings. Cell biology has served as a semi-disciplinary umbrella, constituted by the least common denominator of its research fields. This umbrella has been shaped by interest groups giving justice to the necessity for boundaries within a competitive system of science governance, funding, and economies of attention, and the need for standards as a basis of qualified peer-review.

While Rheinberger's approach allows for in-depth historical-epistemological reconstruction and analysis of experimental practices, it does not allow for a fully-fledged investigation of phenomena central to the realms of articulation as far as gatekeeping processes targeting the epistemic orientation are concerned. By drawing from Markus Arnold's concept of disciplinary identity embedded in scientific cultures, which is strongly shaped by initiation practices, one can enrich the historiographic lens on experimental cultures with one on concrete realms of articulation: traditions and customs that frame ways of knowing, the command of symbolic and linguistic forms of knowledge and communication, as well as codes of conduct[30] (and the knowledge how to bend them). In this discipline-oriented model, initiation usually takes place in pre- and post-graduate training settings. I argue that in inter- and post-disciplinary subjects such as cell biology fluent command of this articulation of identity matters for becoming a member of a given scientific community, but initiation is much

[28] See, e. g., Hyman 2014; Hess 2019.

[29] Stephen E. Baldwin, "Comments on the Comments from the ASCB Survey," September 1976, UMBC ASCB MSS95 01, Box 75, Folder 25.

[30] Arnold 2004, on 18.

opaquer and more implicit than in clear-cut disciplines. Editorial policies as well as reviewing processes of manuscripts submitted with scientific journals offer insight into these dynamics. They are often characterized by a mismatch between explicitly communicated norms (published journal scopes, information for contributors) and the criteria for inclusion and exclusion applied backstage (negotiated among editors and communicated to reviewers). The experiences of individuals with this system may influence their modes of communicating knowledge (for example by using certain rhetoric figures as a minimum) but also their practices of experimentation—and can be understood as initiation processes. Within this perspective, the historically decreasing value attributed to practices often associated with old-fashioned natural history such as observation and description, might lead to an underestimation of their impact by historians of science. And while historians are urged to avoid judgements of sciences as proper/improper or important/less important, historical actors indeed applied such judgements to certain scientific practices as a mode of gatekeeping—in need of historical research.

Taken together, these findings highlight the potential of investigating experimental cultures à la Rheinberger *together* with aspects of scientific cultures usually attributed to the logic of disciplines and considering potentially diverging developments in different countries or even more fine-grained local settings.[31]

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[31] For frameworks to investigate policy, place and organizational contexts see Merz and Sormani 2016.

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