 RESEARCH ARTICLE

Scientific Pluralism in Practice: Responses to Anomaly in the Sciences

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Scientific pluralism has become a household position within the philosophy of science literature. There are numerous accounts of plurality within various research fields. Most scientific pluralists, however, focus on the plurality of theories, explanations, or mechanisms, while other potential targets of plurality that the philosophy of scientific practice has particularly emphasized have so far not received extensive treatment. How should we approach such practice-based candidates of plurality? And what are potential pluralist positions concerning the objects of scientific practice? In this article, I set out to answer these questions. I combine approaching a widely influential topic within the philosophy of science, scientific pluralism, with social science methodology. Using interview data combined with sociological analysis, I provide a nuanced picture of the dynamics of one particular research field that displays plurality. Focusing on how sociological configurations resonate with intellectual commitments within a research field, I disentangle practice-based from theoretical plurality. I consider how these empirical results should feedback on the scientific pluralism literature.

Keywords

scientific pluralism • philosophy of scientific practice • model organisms • scientific repertoire • theoretical pluralism • anomalies in science

1 Introduction

Scientific Pluralism (SP) has become one of the most debated topics within the philosophy of science literature (Kellert, Longino, and Waters 2006; Chang 2012; Potochnik 2017). At the same time, it becomes increasingly hard to locate those philosophers who do not support SP. If one, for example, looks at the philosophy of biology, it is tempting to ask: Where have the monists gone? Has SP become uncontroversial, at least in some areas within the philosophy of science?

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Scientific Pluralism is often defined as the position that certain natural phenomena need more than one explanation, theory, or method, to be fully understood. The concepts scientific pluralists are interested in extend, of course, beyond these. The philosophy of science in practice demands the inclusion of experimental techniques, research traditions, and repertoires when studying SP (Pickering 1992; Galison 1997; Soler and Catinaud 2014; Ankeny and Leonelli 2016). SP comes in many different flavors, such as metaphysical vs. epistemological SP (Cartwright 1999; Dupré 1993; Longino 2006); normative, evaluative, or descriptive SP (Chang 2012; Dupré 1993); intra- or interdisciplinary SP (Galison and Stump 1996); and integrative vs. non-integrative SP (Mitchell 2002; Chang 2012), to name a few. While SP is often not explicitly defined, I formulated these three criteria elsewhere as a common ground for card-carrying scientific pluralists in the recent literature (Veigl 2021):

1. non-monism: It is not known or denied that the nature of the world is such that it can, at least in principle, be completely described or explained by one comprehensive account.

2. plurality: There are, might be, should be (the verb be can have different modalities here) several theories, explanations, or methods for approaching the same natural phenomenon.

3. acceptance: Plurality is favorable.

Case studies of SP in the sciences are pervasive. One problem of SP is, however, relatively unexplored. How does plurality work in practice? Pleas for scientific pluralism are often formulated in terms of "theories" and "explanations." While issues about scientific practice have been emphasized, they have not yet been fully incorporated into the SP literature. What would SP regarding scientific practice look like? Does plurality of methods, experimental techniques, standards, or model organisms only invite a "weak," resolvable pluralism, whereas issues pertaining to theories or explanations give rise to "strong" pluralisms? Finally, how do theoretical aspects and issues of scientific practice relate when it comes to plurality or pluralism within a scientific research field?

With the plea for a stronger focus on issues pertaining to scientific practice also comes the question of which methods are adequate for studying such issues. Applied techniques are diverse and span from inductive metaphysics (Hüttemann and Kaiser 2018) to approaches inspired by Science and Technology Studies (Leonelli 2012). While quantitative empirical methods have been established as one primary tool in experimental philosophy (Knobe 2007), employing qualitative empirical methods in the philosophy of science seems considerably less established (with a few exceptions, e.g., Stotz, Griffiths, and Knight 2004). If scientific practice is to be emphasized, qualitative empirical methods will, however, be necessary for illuminating at least some questions about this subject area. This invites a second, related question—how to work from empirical results to arrive at a philosophical position. How can we make the transition from the explicitly descriptive to the normative?

In this article, I will provide an attempt to study the issue of how to relate conceptual issues of SP to issues arising within scientific practice, employing an empirical strategy based on sociological methods and analysis. I will subsequently ask and try to answer how an empirical and qualitative account of a scientific research field, in which different issues regards plurality arise, feeds back on the more propositional aspects of scientific pluralism. And I consider how to disentangle the social and the intellectual when investigating scientific pluralism in practice.

I will proceed in the following steps. First, I introduce my case study and argue its aptness for studying the fate of plurality and SP in practice. In the central part of this paper, I discuss the results of the investigation and provide empirical data that showcases plurality within a research field. I use interviews conducted with key players in small RNA inheritance research (SRIR),

gathered between 2016 and 2019. Interview partners were selected through core-set analysis (Collins 1981). I conducted 25 interviews, representing about 70-80% of small RNA inheritance key players at the time interviewed. The interviews were semi-structured. The method of analysis was "a priori coding" (Creswell 2013). During the analysis process, sub-themes emerged inductively, and these were incorporated into the analysis (Ryan and Bernard 2003). For this article, I worked on selected sub-themes that subsumed interviewees' accounts of SRIR. Quotes that appear throughout the text are written in italics. I use a grid/group framework (Douglas 1970) and a Scientific Intellectual Movements approach (Frickel and Gross 2005) to account for the small RNA inheritance field. Next, I will discuss issues particular to plurality in practice and will emphasize the plurality of model organisms (MOs). In the concluding section, I will consider how this case study's cues can or should feedback into ongoing discourses regarding SP.

2 Small RNA Inheritance—a Case of Plurality?

In the field of small RNA inheritance, several actors propose that their findings support a particular theoretical framework that is not considered "established."[1] Simultaneously, these actors do not claim that the established framework is wrong entirely; it just does not sit well with the concrete phenomena they study. I interpret this status quo as a sign of plurality within the field. Because small RNA inheritance has received criticism from actors within more established fields, it is possible to identify a dispute regarding this plurality.[2]

To study this situation, I take a particular interpretative viewpoint to start my analysis: Some findings in small RNA inheritance are considered anomalies regarding a particular theoretical framework. Actors propose heterodox theoretical frameworks to account for these anomalies. Phenomena recognized as anomalies reveal plurality in the field because they are accommodated with alternative theoretical accounts. However, in many aspects of researchers' professional lives, "theory" might take the back seat, and other issues pertaining to the intricacies of scientific practice might determine negotiations within the field. I aim to investigate how anomaly and plurality of theoretical accounts relate to pluralities of scientific practice. Consequently, I aim to explore how to synthesize these spheres into a position about SP informed by the intricacies of scientific practice. In the remainder of this section, I will say a few words about small RNA inheritance, give a rough idea about what is biologically at stake, and explain the different positions within and about the field.

Small RNAs are short, regulatory molecules. Their mechanism of action is complementary binding. They interfere with messenger RNAs (mRNAs) through co- and post-transcriptional gene silencing. Most small RNAs interfere with the translation of specific mRNAs in the cytoplasm. Some bind to mRNAs while they are being transcribed in the nucleus. Small RNA activities usually have a "silencing" effect; this means, if bound to a small RNA, a particu-

[1] In what follows, I will use the terms "establishment" to describe actors who oppose that small RNA inheritance supports reconsidering certain aspects of an established corpus of knowledge. This term is necessarily imprecise as it tracks several different interest groups, of which this article will only focus on one. Nevertheless, this imprecision resonates well with how interviewees accounted for their opponents.

[2] This particular situation could, of course, be subjected to several different analytic frameworks, among them Miriam Solomon's consensus/dissensus model (Solomon 2001). Solomon champions a view on debate in science that regards consensus as a rare and special case of dissensus and focuses on how particular decision vectors influence outcomes of scientific debate. Importantly, Solomon considers accounts of SP, such as Longino's, Feyerabend's, or Dupré's, to also favor dissent over long periods. In the end, the consensus/dissensus model primarily traces mobilized factors which maintain or discontinue dissent. In this article, the focus is, however, shifted toward the results of maintained dissent, and how it affects the dissenting community.

lar mRNA cannot be translated, and thus no corresponding protein can be produced (Fire et al. 1998; Hamilton and Baulcombe 1999; Hammond et al. 2000; Bernstein et al. 2001; Lipardi et al. 2001; Sijen et al. 2001).

Small RNAs occur in most species and perform a variety of physiological functions. In mammals, particular types of small RNAs are, for example, associated with specific diseases and developmental defects (Stroynowska-Czerwinska et al. 2014). In invertebrates, small RNAs have a central role in the immune response (Aliyari et al. 2008). In both animal and plant kingdoms, small RNAs protect the germ line, functioning as "watchdogs" to ensure selfish genetic elements remain under control (Vagin et al. 2006). Several authors have reported the stability of small RNA-based effects over generations in a range of model organisms (reviewed in Rechavi and Lev 2017; Miska and Ferguson-Smith 2016). For example, in the roundworm *C. elegans*, small RNA-based resistance to certain viruses and responses to starvation persist for several generations (Rechavi et al. 2011; Rechavi et al. 2014).

Small RNA inheritance researchers (SRIRs) often describe their findings as instances of the inheritance of acquired traits. In many of these publications, the reported phenomena receive the label "Lamarckian" (Schmidt and Kornfeld 2016; Nowacki and Landweber 2009; Wang et al. 2017; Rechavi et al. 2011). Some authors openly address the incompatibility of their findings with the standard premises of Neo-Darwinism due to the persistence of "directed" and not "random" adaptive changes throughout generations (e.g., Rechavi et al. 2011). While small RNA research is situated almost exclusively within the molecular biological disciplines, it has attracted other stakeholders' interests. Most prominently, advocates of the Extended Evolutionary Synthesis (EES) like to point to small RNA inheritance findings to support their claims that DNA-based natural selection of random mutations is not the only trajectory of inheritance that might play a role in evolution (Pigliucci and Müller 2010; Jablonka 2017). In conclusion, different stakeholders frame the inheritance of small RNAs as an anomaly to the Neo-Darwinian account. Particular theoretical frameworks are proposed to accommodate the anomaly, such as a Lamarckian evolution and the EES interpretation.

I consider the small RNA inheritance field a promising area to investigate plurality in practice. There are at least three possible theoretical perspectives on small RNA inheritance:

1. The Lamarckian—that small RNA inheritance is an instantiation of the inheritance of acquired traits.[3] It is important to note, however, that SRIRs are not evolutionary biologists; they can broadly be classified as molecular biologists, most identifying as either geneticists or biochemists. Thus, SRIRs produce molecular data that they present in a certain way, so it is taken up by evolutionary biologists; however, they are not evolutionary biologists themselves.

2. The EES perspective—which is not necessarily Lamarckian but welcomes small RNA inheritance as instantiations of inheritance beyond the strictly DNA-based paradigm.[4]

3. Lastly, the Neo-Darwinian—in a sense, the "reactive" perspective, that conceptually accommodates small RNA inheritance as nothing outside the ordinary.

[3]Admittedly, this is a very crude definition of what it means to be Lamarckian, but most articles connecting small RNA inheritance with Lamarckism don't go further than that (for a critical discussion of the term's use see Loison (2018) and Speijer (2019)). It is, however, nevertheless possible to associate small RNA inheritance equilibria with a Lamarckian use/disuse paradigm (Veigl 2017; Veigl 2019). As these deliberations rely too much on mechanistic details of small RNA inheritance, in the remainder of this article the crude version of Lamarckism will be used, reflecting interviewees' use of the term.

[4]Vice versa, it is also possible to support a Lamarckian interpretation of small RNA inheritance, but disagree with certain propositions of the EES, such as ecological inheritance (Odling-Smee and Laland 2011).

At the moment, there is both a debate regarding small RNA inheritance and evolution and a debate within molecular biology on the validity of small RNA inheritance experiments. Opponents of small RNA inheritance thus have heterogenous disciplinary identities, some quite distant (such as evolutionary biologists) and some quite close (such as molecular biologists) to SRIRs' discipline. In what follows, I will analyze the status of the "anomaly" against the backdrop of small RNA inheritance being a subject of negotiation for different interest groups with shifting boundaries and memberships.

3 Testing Scientific Pluralism in Practice

There are, in a sense, two ways of investigating plurality in a research field. One is to ask researchers what they think about plurality directly. The other is to distill the different types and consequences of plurality by asking them to provide more general accounts of their research field. "Scientific pluralism" is not a term endemic in molecular biology. Discussion of SP with interviewees necessarily projects the interviewer's preconceptions onto the questioning. I, therefore, decided to go with the second option. In this section, the notion of SP will fade into the background, and the analytic focus will mainly lie on social dynamics. I will differentiate between different types of interactions and how plurality is situated in different contexts. I disentangle different pressures to set apart scientific practice-based plurality within the field and theoretical plurality as a pressure "from above."

To understand the dynamics of the SRIR field, I will employ two analytical concepts, one "large scale" and one "fine-grained." I employ grid/group analysis (Douglas 1970) to approach the SRIR field as a social gathering with a particular community structure and compare their community structure with those of described opponents. Zooming into the small RNA inheritance field to understand group dynamics, I employ a scientific intellectual movement (SIMs) framework (Frickel and Gross 2005; Parker and Hackett 2012). The notion of the SIM was developed vis-à-vis social movements theory, in that they are "collective efforts to pursue research programs or projects for thought in the face of resistance from others in the scientific or intellectual community" (Frickel and Gross 2005, 206). Grid/group analysis provides a general account of cultural controls. Practitioners of grid/group analysis assume that "the infinite array of social interactions can be sorted and classified into a few grand classes" (Douglas [1982] 2013, 1). In this sense, SIMs inhabit a particular grid/group configuration, and thus, as we shall see, a SIMs framework is an apt tool to "zoom" into the particular grid/group configuration SRIRs inhabit.

Grid/group analysis has two dimensions (see fig. 1). Group commitment describes the ties between the members of the group. Grid control defines the hierarchical structure and the degree of regulation. There are, thus, four different possible configurations of social groups ([1982] 2013, 3-4). Each has different modes of reward and punishment that shape the behaviors of its inhabitants. Individuals behave in sanctioned ways and use their framework to judge or justify actions. Grid/group configuration predicts how social gatherings accommodate anomalies. None of the grid/group modes is viable on its own, as other modes are needed to perform demarcation and solidify group identity. Also, membership is not eternally fixed, and movement up and down the grid or group axes is possible. Grid/group analysis has been employed to account for social configurations within science and industry (e.g., Bloor 1978; Bloor and Bloor 1982; and recently, Cerroni and Simonella 2014). One of the best-known examples is David Bloor's text "Polyhedra and the Abominations of Leviticus," which assigns grid/group

locations to Imre Lakatos's students in *Proofs and Refutations*[5] (1976), relating their grid/group status with their respective responses to mathematical anomalies.

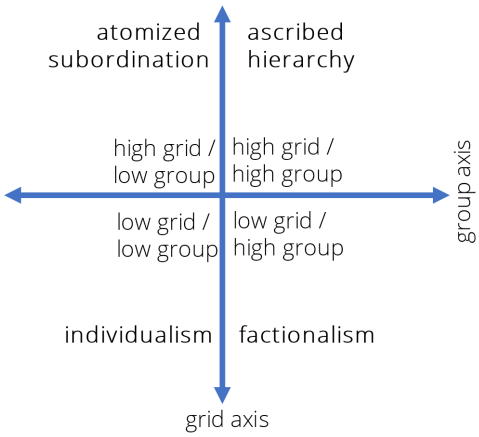

Figure 1: Scheme of the four grid/group configurations (cf. Douglas 1970).

Grid/group analysis has been criticized for being so broad that it fits almost any social group. I, therefore, use the grid/group framework to produce a preliminary, broad-brushstrokes account of groups of actors involved in the small RNA inheritance debate, given, as discussed in the previous section, that several disciplinarily separated interest groups partake in the small RNA inheritance debate. Grid/group location is assigned interpretatively from interviewees' assertions and expressions of belief (e.g., Bloor 1978; Bloor and Bloor 1982). Thus, the analysis will be performed based on how one social group constructs boundaries.

In general, grid/group analysis is a means to localize social groups and not necessarily individuals. Nevertheless, one might view the grid/group framework as a coordinate system, yielding four possible configurations $(x/y, x/-y, -x/y, -x/-y)$. Within these quadrants, individuals assigned members of a particular configuration might display differences in numerical values, that is, some might be closer, and others farther from abscissa and/or ordinate (Bloor and Bloor 1982). Also, movements are possible, and there are pressures to move up/down grid or group. Inhabitants of one grid/group quadrant do not necessarily behave and speak homogeneously.

For a more fine-grained analysis of the SRIR field, I will rely on the concept of SIMs. The notion of the SIM was developed to account for the fact that "rebels" in science often do not operate on their own but form strong bonds with the like-minded. "SIMS must develop a socio-emotional culture that motivates the creation of innovative ideas and shelters them in the face of resistance, while also managing the negative emotions associated with the skepticism their ideas generate" (Parker and Hackett 2012, 22). Contention over movement identity is frequent. SIMs are political concerning the "interests in the distribution, maintenance, or transfer of

[5]Bloor makes his case by assuming that alpha, beta, and friends represent real-life mathematicians of the 18th and 19th centuries.

power" (Weber [1918] 1946, 78). Grievance knits together SIMs by generating solidarity and stabilizing emotional orientation and resistance (Parker and Hacket 2012, 25).

SIMs form enclaves, characterized by the low grid/high group configuration "in building the emotional capital necessary for creating and defending ideas, group bonds become more intense than bonds with the scientific community" (Parker and Hackett 2012, 25). In that process, boundaries become a means of identity work. "Disciplines are political institutions that demarcate areas of academic territory, allocate the privileges and responsibilities of expertise, and structure claims on resources" (Kohler 1982, 1). Thus a boundary in this context is always social and epistemological; it is co-produced by both forces (Jasanoff 2004). Boundaries and social identity are interconnected (Lamont and Molnar 2002; Riesch 2010). "Building a social identity involves defining a group and differentiating it from other groups, which effectively builds a symbolic boundary between the groups." (Riesch 2010, 457). Boundary work, the activity researchers engage in when they aim at demarcating themselves and/or the like-minded from other practitioners (Gieryn 1983), is the result of but also reinforces social identities.

These analytical tools will be employed to complete three tasks in what follows. First, they will aid in providing an account of the small RNA inheritance field. Second, I will analyze the sociological configurations of groups interacting with the small RNA inheritance field. Third, following up on these results, the configurations of plurality within the small RNA inheritance field will be assessed and explained by reference to the social configurations described. In a sense, an attempt to synthesize the social and the propositional will be provided.

3.1 Towards a Sociological Account of the Small RNA Inheritance Field

In this section, I will use interviewees' accounts of their research field to assign a grid/group quadrant for SRIR. I will particularly focus on by which means interviewees assigned memberships and which researchers (in- or outside the small RNA inheritance research field) they perceived as a threat. During interviews, interviewees struck me as highly conscious of group membership and described only a fraction of other interviewees as group members. They engaged in boundary work, justifying their choices by referring to particular categories, for example, working with the "right" model organism (mouse, fly, worm, plant, yeast, etc.), the "right" small RNA species (siRNAs, piRNAs, miRNAs, …), or a specific experimental setup ("natural" or "artificial" experimental settings). Some reasons for exclusion were less precise, such as "being too nutty." In a sense, interviewees were concerned with the purity of their field. They did not assign membership lightly. Such behaviors are typical in low grid/high group structures (factionalism); the external group boundary is clear. All other statuses are "ambiguous and open to negotiation" (Douglas [1982] 2013, 4). Accordingly, SRIRs have no established hierarchy or leaders; members cannot take away resources from other group members. Also, hierarchies do not play a role when sanctioning particular behaviors. This quadrant is often described as a sectarian or an egalitarian enclave. "Organizations in this quadrant regard themselves as unique, as mavericks that are categorically different from other organizations" (Caulkins 2009, 65). Interviewees uttered sentiments about special skills and unique endowments regarding the types of intricate research they perform. They did not believe that everyone was qualified to undertake SRIR (see fig. 2).

Outsiders perceive low grid/high group inhabitants as deviant, while members feel membership "is a matter of pride and indictment of the dominant organizations" (Caulkins 2009, 66). Inhabitants of such groups are mutually dependent but cannot trust one another. There might be contradictory goals and conflicting duties, such as loyalty and competition (Bloor and Bloor [1982] 2013, 93). These characteristics make the low grid/high group configuration somewhat

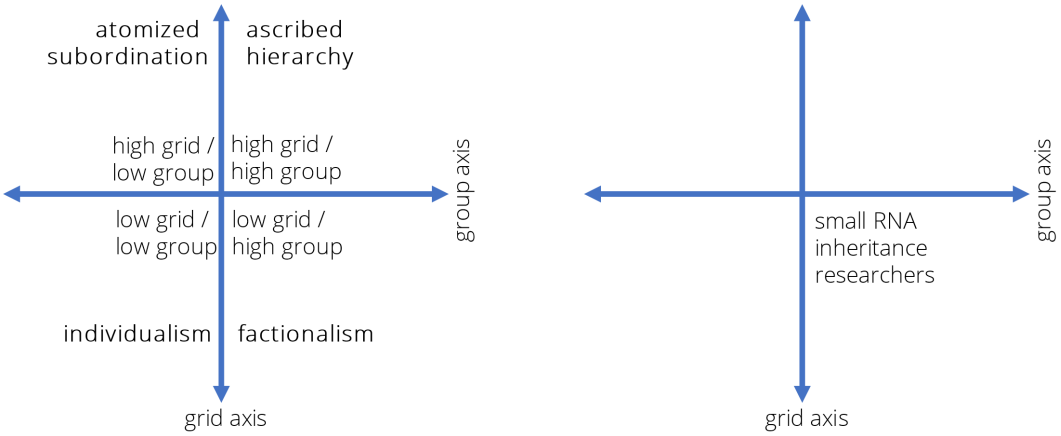

Figure 2: SRIRs located in the low grid/high group configuration.

unstable: while highly interactive, there are few and, if present, highly fluid, institutional or hierarchical controls to settle disputes.

Interviewees identified the reproducibility of experiments as a significant problem. Most of the time, interviewees identified the same papers as problematic. They expressed strong opinions concerning replication, such as being *worried*, *concerned*, *nervous*, or *troubled*. What struck me, however, was that those research papers identified as having replication issues were nevertheless frequently cited in the SRIR literature.[6] They identified conferences or private correspondences as the most convenient way to learn about problems with reproducibility. Others reported their own experiences of being unable to replicate published findings. Thus, SRIRs rely on highly access-restricted knowledge regarding replication problems but do not address these problems in public.[7] While interviewees characterized reproducibility as a general problem and not necessarily related to bad practice, many discerned a type of researcher they considered bad for the field. Because these researchers produced poor-quality papers, the whole field suffered from criticism. They worried about researchers who wrote *complete bullshit* and thus made the whole field *look bad* to the outside, especially to practitioners within more established professions. Interviewees described the literature as *contaminated* with instances of poor-quality research. They perceived small RNA inheritance as a slippery field that produced *a lot of garbage*, despite *very beautiful* examples.

Interviewees were also quite alarmed about creationists: *You gotta be really careful what you*

[6]That "refuted" claims persist and are recited in the relevant literature is a fairly general phenomenon (Ioannidis 2005). For this specific case, the original papers were never "officially" refuted; that is, their refutation was not shared with researchers beyond the small RNA inheritance community.

[7]For example, most interviewees classified one early paper—one which gave the small RNA inheritance field a considerable push forward—as not replicable. Interviewees, however, felt that even though the experimental data wasn't reproducible, the paper *got something right*. One researcher even hinted that at the time this paper was published, no one believed the findings because of methodological problems. No critique was raised at that time, however, because the community thought that the conclusions were correct nevertheless.

put out there because it's what creationists and intelligent design people grab. They feared that they opened *a little crack in the door* for creationists. Although interviewees felt that this asymmetrical alliance was not their fault, they were worried about people who performed small RNA inheritance experiments and were *getting a little too nutty* by talking about evolution in *strange ways*. Along with the fear of researchers getting "too nutty," interviewees pointed out that they were unhappy with using the term "Lamarckian." Such statements were in tension with the prevalence of the term "Lamarckian" in their writing. Most commonly, they criticized the use of this term as a marketing trick. Some worried that this practice made the small RNA field unpopular because it gave the impression that researchers were *downplaying genetics*. Others, however, insisted that using the term "Lamarckian" as a rebellion is warranted. They argued that they reached a point where they found such rebellion justified.

The small RNA research field thus realizes several features of a SIM: Interviewees profit from a network that helps alleviate pressures and facilitates the flow of information within the community, but not to the outside. Some expressed apparent pride in group membership, conspicuously guarding its boundary, identifying with its rebellious character. At the same time, identity- and boundary work feedback on each other, creating social identity and rules for belonging. Plurality is deeply linked to these social configurations. Two separate levels are discernable: the theoretical and the practice-based.

The practice-based level of plurality is epitomized by how SRIRs assigned group membership: working on the "right" model organism, small RNA species, experimental setup …. Take the plurality of model organisms as an example. Model organisms are types of scientific repertoires (Ankeny and Leonelli 2011, 2016), constituted by the history of their establishment, components mobilized, experimental standards, and socio-political factors. SRIR is conducted in several model organisms that differ in molecular processes, evidential cultures, and validation procedures. This heterogenizes the research field and makes a unified account impossible, causing strong responses, as epitomized by interviewees' demarcation strategies.

The theoretical level is individuated by interviewees' concern regarding particular propositional/theoretical assertions regarding small RNA inheritance. They fault others for using the framework of Lamarckism excessively, fear being picked up on by creationists, and aim at excluding those who are "too nutty," who violate the purity of the SRIR field. Simultaneously, using the term "Lamarckism" in research papers is a common theme among SRIRs. There is thus tension regarding the status of theory.

In conclusion, several aspects of interviewees' accounts of their field locate them in the low grid/high group quadrant. This particular social configuration, lacking hierarchies and common ways of sanctioning behaviors, invites different types of plurality to cause contend over movement identities: both practice-based and theoretical aspects of plurality create disputes over membership.

3.2 Small RNA Inheritance Between Establishment and Lunatics

The collective experience of grievance is one major characteristic of SIMs. No matter which quadrant of the grid/group system, low grid/high group inhabitants perceive as the "majority," it is perceived as an enemy or threat. Several interviewees reported sanctioning by what we might describe the "establishment," that is, researchers submitting to an established theoretical framework in various elements of their scientific life, such as receiving grants and getting journal articles published.

Interviewees were quite ambivalent when asked to characterize critics. Before being explicitly asked who their opponents were, several indicated that they were constantly confronted

with opposition and *yelled at* from different sides. When asked to name their opponents, most were not able (or possibly did not want) to provide names. Interviewees identified their critics by other means. They often characterized their opponents by affiliation with a specific research field. In this article, I will examine three groups: evolutionary biologists, researchers working on "classical" epigenetics, and researchers who did not succeed with small RNA inheritance experiments.[8] Therefore, the "establishment" is comprised of a heterogeneous group of actors.

Almost all interviewees described evolutionary biologists as opponents of small RNA inheritance. Some explained that they did not *like the hype*. Others hypothesized that evolutionary biologists were angry that molecular biologists started to make claims about evolution, although they were *novices* in evolutionary biology. In this context, interviewees mentioned that evolutionary biologists were angry at people who *come from mechanism* and tried to think about evolution. Several interviewees emphasized that they considered it their job to uncover mechanisms and that they were *mechanist kind of guys*. They argued that they did not receive training in *paradigmatic thinking* or *high-level kind of thinking*. While interviewees acknowledged their status as novices regarding evolutionary theory, several criticized evolutionary biologists as lacking the knowledge necessary to voice criticism regarding the experiments per se.

While evolutionary biologists have quite disparate disciplinary identities compared to SRIRs, another set of critics, molecular biologists—especially researchers working on "classical" epigenetics—are located in the same disciplinary context as SRIRs. Interviewees described the formation of two camps within the study of epigenetics: *chromatin* vs. *small RNA people*. They criticized the *chromatin people* for thinking that only modifications to DNA or histones could play a role in inheritance. Some interviewees argued that researchers working on chromatin factors opposed the small RNA field because they were frustrated as chromatin research did not produce fruitful results. The distribution of *chromatin people* and *small RNA people* investigating a specific model organism was also mentioned as a problem. For example, most *drosophila folks* were *chromatin people*.

Another group of researchers opposing small RNA inheritance was those whose small RNA inheritance experiments were unsuccessful. Together with researchers working on "classical" epigenetic phenomena, these critics have the insight to criticize SRIR methodologically. Interviewees characterized these researchers as having invested much money in small RNA inheritance experiments, but having not succeeded nor been able to replicate previously reported findings. So, they had been scrutinizing small RNA inheritance findings for methodological flaws. Thus, SRIRs experience their field as attacked from different sides, with different types of criticisms.

Given SRIRs' accounts, it is possible to delineate another grid/group configuration. Critics SRIRs describe as part of the establishment qualify for the high grid/high group quadrant. In this configuration, "loyalty is rewarded and hierarchy is respected, an individual knows her place in a world that is securely bounded and stratified" (Douglas [1982] 2013, 4). High grid/high group structures are bureaucratic, hierarchic, and collectivist. Interviewees' relationship with the "establishment" is multi-dimensional, reflecting the different disciplinary identities SRIRs assigned to "establishment." SRIRs aim at challenging established knowledge and therefore try to set themselves apart from it. But they are also dependent on institutions founded and governed by the establishment. High-impact journals, grants, and impactful conferences are part of establishment structures. This results in two movements between the high grid/high group and the high grid/low group quadrants. One is up-grid: SRIRs try to move up-grid to access the establishment's reward systems, to access funding and publish papers. The other is down-grid: SRIRs experience pushback from the establishment. They know that the gatekeepers

[8]Note that this characterization is anything but purely descriptive, and mainly tied to denying a particular set of skills, and a particular emotional state (frustration).

consider them "heretics," and they are conscious of efforts to keep SRIR out of reward systems. Researchers who do not follow these rules are viewed as deviants and blamed for instabilities within the group. Pretending to be part of or move closer towards the establishment is, however, strategic for SRIRs (see fig. 3).

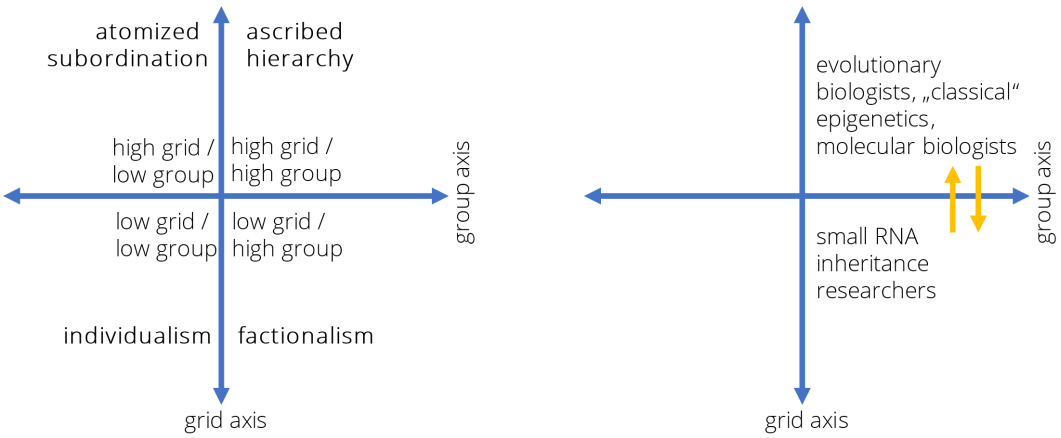

Figure 3: Interactions of SRIRs and "establishment."

Historian of science Mario Biagioli has analyzed such configurations. He characterizes episodes of plurality as struggles for (institutional) power. In most cases, the establishment aims to defend its authority, whereas "invaders" try to find a place within the reward system (1990, 191). As an additional factor, maintaining socio-professional or disciplinary boundaries is crucial. Biagioli argues that newcomers often use "strategic bilingualism" to force their way into funding systems. They use the language of the establishment without adopting the associated identity (1990, 205). "Strategic" bilingualism means to make use of a "fossil," a language of one's scientific past (e.g., one's training), without changing one's socio-professional identity (ibid.).[9] This description fits well with the characterization of SRIRs. SRIRs are proficient in the language of "orthodoxy" because it is the language of their training; it is the language they were "disciplined" in (Arnold 2004, 19). They need to use the language of orthodoxy—the "useful fossil" (1990, 205)—to access funding and prove their proficiency on issues that concern orthodoxy. In other contexts, such as conference talks and lab group meetings, they show their fluency in their program's language, which breaks the rules of orthodoxy by proposing different formulas, methods, theories, and molecular entities.[10]

[9]The notion of "strategic bilingualism" rings similar to a concept proposed in the philosophy of science literature, "strategic pluralism" (Sent 2003; Van Bouwel 2005; Jackson 2018). Strategic pluralism is mainly situated in the philosophy of (heterodox) economics, but was adopted by Ronald Giere to denote "primarily just a strategic move in the game of trying to dominate a field or profession. Those in the minority proclaim the virtues of pluralism in an effort to legitimate their opposition to a dominant point of view. But one can be pretty sure that, if the insurgent group were itself ever to become dominant, talk of pluralism would subside and they would become every bit as monistic as those whom they had replaced" (Giere 2006, 40).

[10]Note the "granularity" of grid/group accounts. While the level of analysis is one that makes differences between

After assigning a grid/group quadrant for orthodoxy, I shall now discuss the last group of researchers interviewees interact with. Interviewees most frequently engaged in boundary work to detach themselves from individuals who are *a little nutty*. These researchers fit the low grid/low group quadrant, "individualism." This configuration facilitates "negotiating contracts or choosing allies, and in consequence, it also allows for individual mobility up and down whatever the current scale of prestige and influence" ([1982] 2013, 4). Low grid/low group configurations are individualistic, competitive, entrepreneurial. Individuals are only responsible for themselves. Low grid/low group individuals have somewhat similar socio-professional identities as low grid/high group researchers. Installing boundaries is thus necessary to ensure that "lunatics" cannot invade the SRIR field and thereby harm it. There are two movements between SRIRs and "lunatics." Individuals who become "too nutty" move down-group. The SRIR community pushes these researchers out, for they are considered harmful to the field. Researchers described as "too nutty" aim to move up-group to profit from group ties' privileges (see fig. 4).

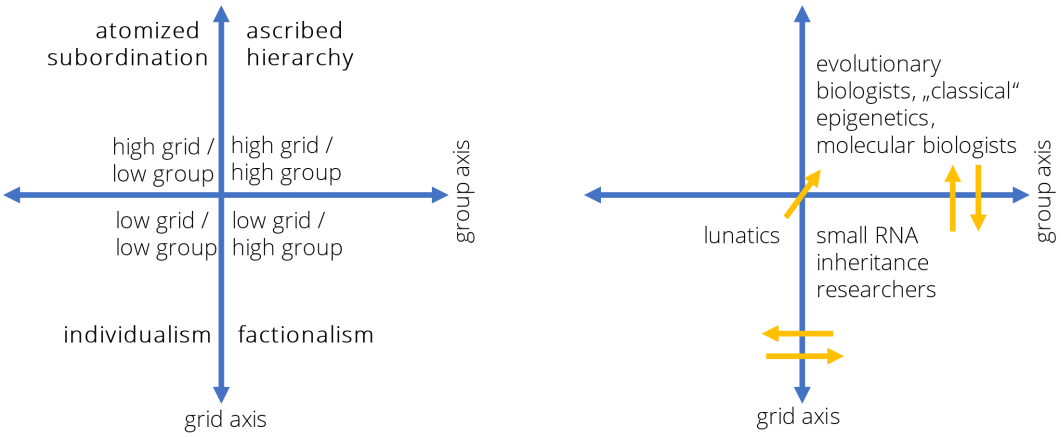

Figure 4: SRIRs' interaction with establishment and lunatics.

Although several researchers inhabit the low grid/low group quadrant, there are no strong group ties. While all of them were described as "lunatics," and roughly the same researchers (or "types" of researchers, identified through, e.g., work on a particular MO or aspect of small RNA inheritance) were identified by interviewees, their beliefs, methods, model organisms, and theoretical commitments will be heterogeneous, and it is difficult for them to form alliances. Inhabitants of the "individualism" quadrant might find other members within the same quadrant "too nutty." They fear for their reputation and believe that such alliances might prevent them from moving up-group. "Lunatics" do not only interact with "factionalism" but also with "ascribed hierarchy." While some would like to profit from the group ties of SRIR, some are successful in securing mainstream funding or publications without the support of the SRIR community. Orthodoxy is essential for all grid/group configurations regarding funding, journals, and grants

high grid/high group and low grid/high group salient, they might share similarities on a "higher" level, such as, e.g., that both believe in the importance of data and experimental evidence.

(see fig. 4). Within the low grid/low group quadrant, inhabitants have fewer restrictions regarding publication outlets and funding options. Inhabitants of the low group/low grid quadrant are more likely to publish in journals or attain funding from streams that might be considered by their peers "non-mainstream," at least.

Having assigned three different grid/group quadrants to SRIRs and groups they interact with, it is crucial to query whether it is justified to partition different groups of researchers—such as those researching small RNA inheritance, those who also work on small RNA inheritance but are considered lunatic, and those who oppose small RNA inheritance, but are also molecular biologists—into distinct communities. Wouldn't it be possible to consider all discussed actors as part of one community, given that they aim at securing similar institutional goods? In this view, we would regard, e.g., SRIR as an enclave, a SIM, within the hierarchical establishment. Interviewees' accounts were not free of tensions, thus supporting either possibility. In a sense, zooming out to find the studied quadrant only as a sub-quadrant in a larger analytical framework might always be possible. Scaling might thus be a result of the particular investigative lens. As this article focuses on plurality within the SRIR field, it elaborates on its features by operating in a level of depth that looks at SRIRs as inhabitants of an entire grid/group quadrant.

Investigating the SRIR field reveals many tensions that culminate in organizing and fortifying social and intellectual boundaries. Interviewees who fault others for being "too nutty" still cite articles by "nutty" researchers.[11] Similarly, interviewees cite papers that are known to have replication issues. Presenting a picture of unity, consent, and homogeneity to the outside seems pivotal to be taken seriously. Thus, how a field appears to the outside and the actual disputes and commitments within might be disparate. In addition to theoretical commitments, interviewees' accounts also highlight several other factors of plurality that are mobilized to fortify the field's boundaries: model organisms, experimental techniques, and setups. In addition, different subcommunities use these practice-based aspects of plurality differently to draw boundaries, and membership is assigned heterogeneously. Plurality created within the research field mostly does not concern theories. However, these practice-based aspects of plurality vanish when SRIRs present their outputs outside their field. Theoretical plurality cashed out by referring to Lamarckism is used as a representation to the outside. This theme of rebellion creates emotional capital in-group and serves as a strategy to present small RNA as a unified field to the outside, rallying around a theoretical program.

Why is it crucial for SRIRs to present themselves as a somewhat "united front"? Representation to the outside is likely to serve specific institutional goals rather than mirror what is going on inside. SRIRs encounter substantial threats and criticisms. Presenting SRIR as unified to the outside shields it from further criticism. It is sufficient to handle problems like replication issues by spreading access-restricted knowledge within the field. Avoiding exposure of these problems is instrumental. SRIRs do not expose signs of heterogeneity and disagreement to be taken more seriously as a research program. In the following section, I will try to disentangle further theory and practice-based plurality against the backdrop of the particular social configuration of SRIR.

3.3 Theory has a Janus-faced role in small RNA inheritance

This section aims to synthesize the sociological configuration of the SRIR field with knowledge claims by asking how the social structure of the SRIR field determines how it handles plurality.

[11] Such citations are often used as either "exemplars," or to (re)construct the history of the field. For example, studies reporting similar findings to those known not to be replicatable, or to be authored by "nutty" researchers often cite these to back their findings up through "previous findings." Another example is across-model-organism citations. While many *C. elegans* researchers regarded research in, e.g., mammalian MOs not part of their field, they use citation of, e.g., mouse results in order to establish relevance and continuity of their claims.

Grid/group analysis helps understand how individuals respond to things that do not fit into the boundaries of accepted ways of thinking, hinging on Douglas's conjecture that different social groups organize the intellectual coherence of their world differently. Smaller-scale social relations are detectable in more comprehensive systems of classification (Durkheim and Mauss 1903). But how are these connections established? Two processes fortify the link between both realms. On the one hand, ideas about the nature of the world are often used to legitimate particular social structures, such as institutions. On the other hand, social arrangements are used to understand physical or metaphysical things (Bloor 1978).[12]

To tie grid/group location of SRIRs with their ways of responding to anomalies and accommodating dissent, I take David Bloor's "Polyhedra and the Abomination of Leviticus" (1978) as an inspiration for applying an ethnographical/sociological framework with a body of philosophical literature (Lakatosian *Proofs and Refutations*). Furthermore, it illustrates how to employ grid/group analysis when analyzing speech acts of scientists. In addition, in what follows, I will also provide a theoretical extension to Bloor's framework, thus using a case study on a problem currently salient in the philosophy of science to develop a way of synthesizing the philosophical and the sociological that I deem pretty fruitful, but is only very peripherally known in current philosophy of science.

Bloor uses grid/group analysis to classify responses to deviants. A deviant might be a moral norm or a counterexample to a mathematical proof (Bloor 1978, 245). "Being a counterexample is a role which is conferred upon something, and this depends on how it is used" (1978, 249). Within the sciences, different responses to anomalies are caused by different social structures: "The response to anomaly, and hence the drawing of intellectual boundaries will be negotiated into alignment with the pattern of social boundaries" (1978, 258). In other words, social boundaries, as accounted for by grid/group analysis, and the drawing of intellectual boundaries, characterized by a spectrum of responses to anomaly, resonate. Bloor thus assigns the Lakatosian procedures of "monster-barring," "exception-barring," "monster-adjustment," "dialectical methods of proofs and refutations," "coexistence of theory and counterexample" to the individual grid/group locations (see fig. 5). Importantly, Bloor does not use these categories to import or follow the Lakatosian model of scientific progress; rather, he imports some of Lakatos's notions as a conceptual toolbox to find within-science analogues to the larger-scale, societal processes Douglas described. A "monster" in the Lakatosian sense is a counterexample to a theorem that is recognized by the relevant set of practitioners as a counterexample. Not every potential counterexample is thus a "monster"; "monster" is a social status awarded by the relevant group of practitioners.

In what follows, I assess whether the grid/group locations I assigned for SRIRs, "establishment," and "lunatics" predict their responses to anomaly. I primarily focus on SRIRs (low grid/high group) and orthodoxy (high grid/high group). Before starting with the analysis, I would like to highlight one aspect that Bloor did not cover. I explained above that Bloor urges regarding "counterexamplehood" as a social achievement. The process of "recognizing" a counterexample is similar to "recognizing" someone as, e.g., a knight. I believe that this analysis is somewhat incomplete. If being recognized as a "counterexample" is a social achievement, this recognition might not happen evenly across all grid/group quadrants. Some groups might recognize a specific phenomenon or proposition as a counterexample, while other groups do not. Let me thus propose that counterexamplehood is not only a social achievement but also situated. Empirical data that could be regarded as a counterexample is usually produced in one grid/group quadrant and then spread throughout the other quadrants. In other words, counterexamples do

[12]The mutual dependence of social and intellectual boundaries can also be found in more recent works, such as Sheila Jasanoff's notion of coproduction (2004).

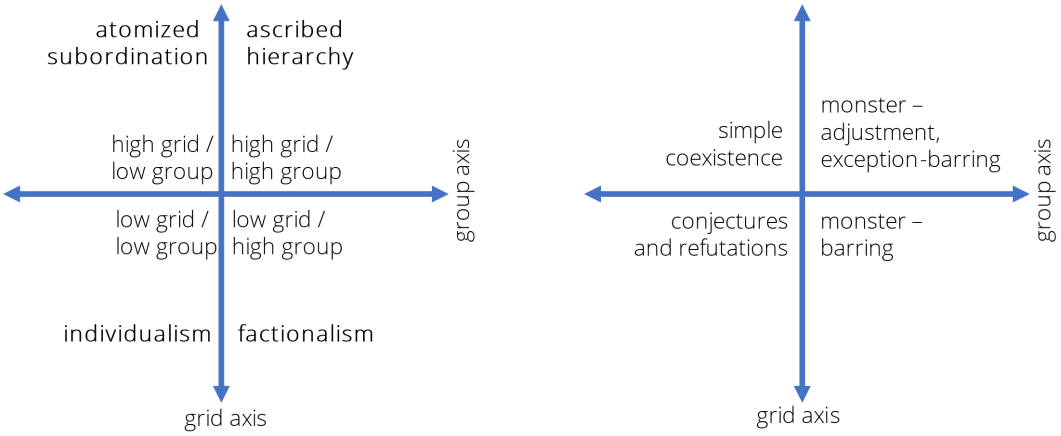

Figure 5: Grid/group location predicts responses to anomalies.

not "fly in" from *outside* the grid/group system. Counterexamples are produced and recognized *within* the grid/group system. Their respective status is *situated*. We shall see how SRIR findings are produced in one specific grid/group setting and subsequently recognized differently in another quadrant.

Let me start with the establishment, the high grid/high group structures, characterized by complex relations and complex procedures to make conceptual accommodations. Reactions toward anomalies are "exception-barring" and "monster-adjustment" (Bloor 1978). Exception-barring means accommodating an anomaly by introducing more subdivisions. As a result, the counterexample only limits the scope of a theorem but is not a refutation. Monster-adjustment is the procedure that helps to see a counterexample so it fits an established theorem. Because of these responses, knowledge gets "additive" and "segmented." This type of compartmentalization is, however, costly and requires hierarchical structures. High grid/high group structures have the resources for compartmentalizing knowledge.

Exception-barring and monster-adjustment are, in fact, two strategies that defenders of the establishment often employ. As an illustration, let me provide two examples. Critics of small RNA inheritance engage in "exception-barring" when they argue that small RNA inheritance is only well-documented in "weird organisms." In saying that, they suggest that the principles of genetics or even theorems such as evolution by natural selection need not cover "weird organisms."[13] The "relative significance debate" is the best-known instantiation of this exception-

[13]As pointed out in section 2, SRIR is not situated in a context that directly bears on evolutionary theory. Yet, small RNA inheritance, along with other forms of epigenetic inheritance, often gets mobilized by proponents of an EES or Lamarckian outlook on evolution as an example of descent with modification through the inheritance of acquired traits and not through natural selection of random mutations. Currently, there are two ways of justifying this sentiment. First, in the case complementary to viruses, which are directly transcribed from viral RNA, there is no DNA phase that could ensure natural selection of random mutations. Second, studies have reported the correlation of histone marks deposited by small RNAs with DNA methylation (in species where DNA methylation occurs) and DNA mutation. Thus, they posit also a directed, and small RNA-dependent way of mutation.

barring (Grossniklaus et al. 2013; Griesemer 2011). They engage in "monster-adjustment" when they argue that phenomena described as small RNA inheritance were always covered by Neo-Darwinist propositions (Singh 2003; Haig 2007; Laland et al. 2014). A "molecular" version of this argument is to insist on small RNAs being genetically encoded and thus also subjected to random mutations. In conclusion, for the case of "orthodoxy," their grid/group location matches their responses to anomalies. Grid/group analysis is predictive.

When analyzing SRIRs, the low grid/high group inhabitants, the problem regarding asymmetries becomes visible. SRIRs "produce" the anomaly. Orthodoxy responds to the anomaly. The activity characteristic of factionalism is "monster-barring"—a somewhat extreme response to anomalies. Lacking hierarchical structures, low grid/high group configurations do not possess the repertoires to compartmentalize knowledge. If something does not fit a specific classification, the particular anomaly is thrown out of the knowledge system. Because of its "pathological" character, the anomaly need not be considered anymore. This sentiment resonates with the preoccupation of low grid/high group structures with "purity" and elevated consciousness for "pollution."

Intuitively, "monster-barring" seems undesirable for SRIRs. They do not "monster-bar" small RNA inheritance. Neither in interviews nor published articles do SRIRs monster-bar traditional forms of inheritance, such as DNA inheritance—that is, declare it as insignificant, an artifact, or only causally secondary to small RNA inheritance.[14] Both forms of inheritance do not merely coexist. SRIRs work with both at the same time. For example, when they perform crossing experiments to trace small RNA-based phenotypes across generations, they also make predictions regarding phenotypes conveyed through DNA inheritance. Methodologically they are pluralistic, or "dualistic," at least.

Do the predictions of grid/group theory fail here? Not necessarily. It is possible to detect "monster-barring" activities of SRIRs, but they occur on a different level. These monster-barring activities, I will demonstrate, ensure that the status of the anomaly will never be attained within the field. One monster-barring activity is the sentiment of researchers that their task primarily lies in uncovering the mechanistic underpinning of small RNA inheritance. By claiming their sole focus is to lie on mechanisms, they need not think about theories or provide their results with a Lamarckian framing. Interviewees faulted others for using a theoretical framing for their results. Thus, one strategy employed by SRIRs is to maintain purity by barring theory. Within SRIR, no debate about theory takes place. The term "anomaly" is meaningless in a space free of theory.

As discussed in previous sections, SRIRs expressed strong concerns regarding researchers being "too nutty." Researchers who are too nutty offer interpretations of SRIR that are "monsters." These interpretations are also recognized as monsters by the inhabitants of factionalism. Another instance is more practice-based monster-barring of researchers that work on the wrong model organisms. (Many interviewees insisted that, e.g., retrospective SRIR on humans should not be accepted within the field.) They try to keep those researchers outside their quadrant through excessive boundary work. We can thus see that monster-barring is not absent but takes place on another level: whereas small RNA inheritance is an observable phenomenon and, as a part of experimental techniques, is not barred, monster-barring operates on the level of interpre-

[14]This position rings, of course, absolutely radical to our ears, but is, in principle, possible given that in ciliate MOs, heritable small RNA direct the organization of the genome in each generation. In this scenario, one could regard small RNAs as the causally prior heritable structure that subsequently aligns a secondary structure, DNA, that is causally relevant for certain processes in each generation. Again, this is not a position ever expressed, but counterfactually, maybe possible, if ciliates and not, say, flies had been central to establishing the discipline of genetics.

tation. It ensures that small RNA inheritance does not receive the status of an anomaly within its field. Monster-barring thus operates the distinction between practice- and theory-based plurality.

In conclusion, the grid/group location successfully predicts responses of SRIRs once asymmetries between grid/group quadrants are appreciated. If counterexamplehood is situated, we should expect that different aspects of the counterexample need to be barred/adjusted for the individual quadrants. While SRIRs bar theory to alleviate pressures within their field, they use a particular theoretical framing to represent their field to the outside. While appeals to theoretical plurality are used to represent and unify the field to the outside, constructing a particular identity and agenda, it creates tensions within the field.

In the above paragraphs, I have argued that monster-barring activities distinguish theory- and practice-based aspects of plurality. While SRIRs experience pressures from "above," primarily concerned with theory, pressures from within the field are different. Disputes about the right way to experiment (the right model organism, the right small RNA species, …), and replication issues challenge the small RNA inheritance field from within. Dividing these pressures into "ideological—from above" and "methodological—from within" traces discrepancies in representation. To the outside, addressing the pressure from above, the status of one unified small RNA inheritance field is maintained through a theoretical program—the challenge of Neo-Darwinism. This "rebellion" functions as a social glue, creating group spirit and collectivizing, suffering grievances. On the inside, the field is not unified, and group members heterogeneously assign membership. Theory, in this context, plays no role. Discussions about theory, Lamarckian theory, are even "barred" to maintain the field's stability.

Certain aspects of plurality visible through examining implications of scientific practice get erased, and other aspects reappear in representations to the outside quite programmatically, having the face of something akin to theoretical pluralism. Experimental practice is not designed to support a particular epistemological claim. However, it gets instrumentalized for supporting a particular claim post-hoc (cf. Lynch et al. 1983; Lynch 1985). In conclusion, "theory" has two contradicting roles. Theory is mobilized as a representation to the outside and a unifier for SRIRs. Theory is, however, avoided on the inside; it is handled as a "monster." In the case of small RNA inheritance, "theory" is Janus-faced; theory unifies and destabilizes simultaneously.

4 Forms of Plurality in Scientific Practice

While previous sections aimed to uncover the relation between theoretical and practice-based plurality, it is now necessary to conceptually explore and formulate an account of plurality in scientific practice. I will do so by discussing one particular example, the plurality of MOs. I will show how MOs, as a form of scientific repertoire, bring about sources of plurality, defined by the specific histories of the MO research communities and the specific repertoires which characterize these communities.

Let me examine the problem of "artificial" vs. "natural" experimental setup as one sub-debate of the plurality of MOs. One group of researchers maintains that the right way of doing experiments on small RNA inheritance is to control experimental conditions as much as possible. Controlled conditions require inbred strains with as little genetic variation as possible and the use of transgenes to control for the readout. Such strategies profit from the well-aligned features of repertoires—standardization, established technologies, and comparability of results. The other group of researchers opposes this way of doing experiments that they consider "unnatural." Studying small RNA inheritance under such conditions entails that "real" effects will be missed. Small RNA inheritance should be studied in conditions as "natural" as possible. For

example, proponents of this view discourage using inbred strains and suggest using genetically heterogenous strains instead. Furthermore, they advise against the use of transgenes as readouts. Instead, they emphasize the importance of "endogenous" readouts—that is, small RNAs expressed under physiological conditions.

This dispute is not new. Specifically, it is not new in the history of genetics in general or in studies on the inheritance of acquired traits in particular. For example, controversies surrounding Paul Kammerer's experiments on the inheritance of acquired traits in the 1910s and 1920s foreshadowed these types of debates. Critics faulted Kammerer for not using uniform and inbred lines but lines with significant variations. For example, the famous geneticist and flyroom founder Thomas Hunt Morgan criticized Kammerer for using toads of mixed origin and genetic composition. Kammerer did not, however, share the geneticist's perspective on how to breed experimental organisms. The institution where he worked, the Biologische Versuchsanstalt, emphasized work in naturalistic settings (Müller 2017). Naturalistic settings, in turn, require genetic variability. Neither did Kammerer share Morgan's socio-professional identity. Morgan represented a new breed of geneticists, and Kammerer was a defender of experimental zoology. Their notions of appropriate methodology were disparate (Gliboff 2006, 2010).

The continuity of the dispute about "controlled" vs. "natural" conditions is striking. Changes in repertoires, model organisms, socio-professional identities, institutional and political factors require special attention nevertheless. This issue takes different shapes in different historical contexts, depending on participants, their socio-professional identities, and the repertoires they can mobilize. This controversy is a significant source of plurality, the plurality of repertoires. Different model organisms are better apt for "controlled" vs. "natural" conditions, respectively. Centralization and standardization also play a significant role. Using inbred, clearly defined strains is facilitated by researchers working on specific MOs, such as *C. elegans*. The *C. elegans* repertoire enables accessing strains from institutionalized breeding centers, apt for highly controlled experiments. This is much more difficult for mouse strains, and pleas for "natural" conditions are much more common amongst representatives of the mouse community.

Concerns about theoretical plurality are absent from interviewees' accounts of the dispute about natural vs. controlled conditions. This runs parallel to the example of Kammerer and Morgan. Of course, these researchers had different theoretical commitments. The start of the twentieth century was one of the best times for disagreeing about theory: chromosome theory of inheritance, Mendelism, neo-Lamarckism, old-school Darwinism, neo-Darwinism, experimentalism, …. For example, Kammerer believed that acquired adaptive traits "mendelize" on the chromosome (Gliboff 2006). His mentor Hans Prizibram rejected the chromosome theory of inheritance and believed in the "Weismann barrier." Despite major theoretical disagreements, Prizibram supported Kammerer. Morgan shared many of Kammerer's theoretical commitments but rejected Kammerer's work because of its experimental design. To conclude, although "theory" was not the primary concern in the scenario sketched above, Kammerer's story is often framed as a dispute about theory—that is, Lamarckian theory. We seem to experience a similar situation with small RNA inheritance.

In conclusion, how well a field can accommodate plurality might depend on established model organisms, accepted techniques, modes of data sharing, the founding history of the field, relations of practitioners within the field, and the structure of centralization (Ankeny and Leonelli 2016). The individual components of the repertoire, in turn, need to be aligned successfully. To exemplify this assertion, let me compare the relative stability of claims about small RNA inheritance in mouse vs. plant model organisms. Experimenters in both fields utter very similar claims: evidence supports that small RNA inheritance is an alternative form of inheritance. This claim is not upsetting at all for researchers working with, for example, *Arabidopsis*

thaliana. In the literature, such claims asserted within plant research do not receive noticeable criticism from the outside. This is entirely different in the mouse field. Repertoires explain this situation: alignment in the plant field is much more successful than in the mouse field.

The mouse MO repertoire has a unique standing. Its representational scope covers humans, and its representational target is human-associated physiological problems. As a result, claims about mouse MOs enter dangerous territory because they are of substantial sociopolitical impact. Evidence on small RNA inheritance produced in mouse MOs cannot be accommodated by classifying it as "weird mouse things." In contrast, findings in plants on small RNA inheritance are very often labeled "weird plant things." Such findings might, however, concern a much more extensive range of organisms. Even if the effects observed in mice were limited to mammals, they would nevertheless be considered tremendously important. Anthropocentrism is omnipresent in biological research.

Repertoires also account for social and political tensions like those discussed in the previous paragraph. This differentiates them from the Changian concept of "systems of practice" (Chang 2012). These social and political tensions add up to the problems of alignment within the mouse MO. Ankeny and Leonelli argue that because mouse research is primarily application-driven, successful alignment of repertoire components remains impossible. In conclusion, comparing the same type of claim uttered for different MOs shows an essential feature of scientific pluralism in practice. The fate of a research field that exposes plurality might depend highly on its specific repertoires and the successful alignment of its parts.

5 Conclusion: Towards Practice-Based Forms of Scientific Pluralism

Investigating plurality and pluralism in a research field and formulating pluralist positions requires diligent attention to the role of repertoires. While there might be significant changes concerning repertoires in a research field, theories might remain relatively stable. SRIRs use "theory" to represent their field to the outside, while repertoires are sources of plurality within the field. While different groups of researchers might present similar claims, their respective repertoires determine the success and stability of the claim.

If philosophers of science, or scientific pluralists, focus on theories and theory change when they are studying research fields, they might miss key episodes of plurality. At least in my case study, repertoires have proven to be a much more common source of plurality than any abstract concept, such as a plurality of theories, mechanisms, or explanations. Scientific pluralists need to be prepared to find disputes about theory on a somewhat rhetorical level. Theories, although part of the discourse, are primarily a means for RNAi inheritance researchers to frame the dispute. Plurality of theories is used as an interpretation of small RNA inheritance experiments after experimentation, at the stage of presenting research.

What kinds of pluralists should we be about small RNA inheritance? Should the fact that theory does not play a significant role within the field discourage theoretical pluralism? Does the observation that the plurality of repertoires is much more prevalent push us towards emphasizing this form of pluralism? Is the plurality of repertoires "too little" or "too modest"—a "pluralism-lite?" I will answer these questions bearing in mind that SRIR contributes to two levels of discourse: the molecular-mechanistic and the evolutionary. I showed that "theory" is a pressure that trickles down from the evolutionary-biological. SRIR bears on evolutionary theory and challenges presentations of evolutionary theory. SRIR sits better with alternative ways of presenting evolutionary theory. On the molecular-mechanistic level, however, it is hard to see how small RNA inheritance violates a specific theory.

The "practice turn" in philosophy of science has enabled philosophers to realize that signif-

icant changes in research fields might occur without the necessity of theory change. Without any visible change (or challenge) to theory, a whole research field might change. The plurality of repertoires, and thus MOs, is a necessary ingredient for pluralism about small RNA inheritance. This point is not trivial, or a "pluralism-lite." In what follows, I will elaborate on this issue. I will formulate a specific type of pluralism concerning MOs that abstains from metaphysical commitments and is purely epistemological.

MOs are not sources of plurality because of their metaphysical properties. Metaphysical commitments, such as those of Mitchell (2003) or Dupré (1993), are not required for pluralism in biology. I will not, for example, refer to contingency or complexity of biological phenomena when arguing for scientific pluralism regarding MOs. I will offer a different account of the MOs' properties, an account that avoids metaphysics. Relevant properties of the MO that contribute to plurality are not related to any metaphysical or essential properties. Instead, the history of establishing the MO, components that were mobilized to form the MO repertoire, experimental standards, and socio-political factors define the properties of the MO. These aspects characterize the MO. And these aspects differentiate different MOs.

The specific type of plurality I advocated in the previous paragraphs is exemplified by the boundary work observed in the case study. Interviewees criticize findings on small RNA inheritance from the perspective of a specific MO. This circumstance can be explained because the "representational scope" of experiments on small RNA inheritance in one MO is not well defined. It is thus not clear what specific small RNA findings in one organism mean for small RNA inheritance in other MOs. The absence of consensus on "representational scope" causes the excessive boundary work observed. In conclusion, the plurality of repertoires is not trivial or "pluralism light." The constitution of the different MOs as different repertoires makes monity impossible. In SRIR, there is a lack of pluralism, which causes an insistence on the homogeneity of outputs across MOs. This lack of repertoire pluralism amongst researchers also causes the observed boundary work and is a source of instability.

Defining such epistemological factors to determine what causes the plurality of MOs suggests a particular perspective on "consensus" about small RNA inheritance. Accepting the plurality of MOs in the epistemological sense that I defined, we cannot expect a unification of the field through consensus. Consensus about evidential standards, methods, and the "controlled" vs. "natural" debate, amongst others, will not be reached. Small RNA inheritance is a heterogenous subject because it is studied in different MOs. These MOs differ by evidential standards, components, alignment of different repertoires, and disciplinary histories. Attaining unity regarding these aspects in small RNA inheritance is impossible.

What about theories, though? Strategic bilingualism or strategic pluralism regarding theories might be a helpful device used by SRIRs to allocate funding and establish a particular emotional culture within their field. One of the main findings of my case study was the particular "Janus-faced" character of theory. While theory is used to represent the field to the outside, it is not of central importance within the SRIR field. Several excellent works on SP are based mainly on written sources. If I had relied on written material only, I would have probably concluded that theoretical plurality and maybe also SP play a considerable part within the small RNA inheritance field. Analysis of interviews reveals that while theory has a rhetorical and strategic function, theoretical plurality is not of central importance within the field. Thus, while theories and explanations have been mainly analyzed for their propositional bearing within the SP literature, their rhetorical and social functions still need to be uncovered to provide a more comprehensive account of SP in practice. I believe thus that it is essential that philosophers of science complement their work on written sources with methods to contrast or check their assessments. Methodology imported from sociology and anthropology designed to trace sci-

ence in action might reveal crucial insights for the philosophy of science after the practice turn. Given a link between the propositional and the social, the social needs to be studied as well, to provide an account of the propositional aspects of science. Tracing plurality in practice helps to understand plurality in action and informs philosophical positions such as SP about such instances of plurality.

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