Article

Science, Technology, & Human Values
2018, Vol. 43(6) 972-1010

DOI: 10.1177/0162243918763100
journals.sagepub.com/home/sth
SAGE

The Emergence of Individual Research Programs in the Early Career Phase of Academics

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Abstract

Scientific communities expect early career researchers (ECRs) to become intellectually independent and to develop longer-term research plans (individual research programs [IRPs]). How such programs emerge during the early career phase is still poorly understood. Drawing on semi-structured interviews with German ECRs in plant biology, experimental physics, and early modern history, we show that the development of such a plan is a research process in itself. The processes leading to IRPs are conditioned by the fields' epistemic practices for producing new knowledge. By linking the conditions under which ECRs work to the epistemic properties of their IRPs, we identify mechanisms that produce these programs and conditions facilitating or hindering the operation of these mechanisms.

Keywords

academic careers, early career phase, postdoc, research plans, research content, epistemic practices

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Introduction

Scientific communities expect their members to be intellectually independent, that is, to formulate their own research problems and select their own approaches to solving them. Whoever pursues an academic career is confronted with this expectation (e.g., Traweek 1988: 87-88; National Research Council [NRC] 2005; National Academy of Science [NAS] 2014). Interestingly, the phenomenon of becoming independent is not yet well understood. Career studies tend to focus on career success such as achieving a tenured position (e.g., Jungbauer-Gans and Gross 2013; Lutter and Schröder 2016), career satisfaction (see the review by Hermanowicz 2012; Miller and Feldman 2015; van der Weijden et al. 2016), or on the impact of careers on publication performance (e.g., Long and McGinnis 1985; Miller, Glick, and Cardinal 2005; Bäker 2015; Yang and Webber 2015). While some of these factors are likely to have a role in the emergence of intellectual independence, the actual process through which early career researchers (ECRs) become independent remains black-boxed in these studies.

If intellectual independence is discussed at all, it is described in rather vague terms such as "learning to work on their own and discussing questions with senior colleagues rather than merely following their advice" (Becher 1990: 10). This description points to an important reason why career studies are reluctant to address intellectual independence. The phenomenon is closely linked to the content of research, which is usually disregarded by career studies. In contrast, observations by science studies suggest that, at least in some fields, becoming independent involves more than the ability to autonomously decide about and work on individual research problems. Thus, Hackett (2005) observed that new group leaders in molecular biology strive to find research problems that create enough "doable or (or fruitful) problems to form a durable identity" (p. 791), which implies that in order to become independent, a researcher must form a group and define a whole set of interconnected problems. This links intellectual independence to the development of individual research programs (IRPs), that is, research plans whose time horizons exceed a single project.

The conditions under which ECRs develop their first IRPs have changed considerably in the last three decades. The most frequently reported global trends include more casual and project-based employment, increasing dependence on external funding as well as long insecure career phases and the delay of tenure (NRC 2005; Stephan and Ma 2005; Åkerlind 2005; Lam and de Campos 2015; NAS 2014). Discussions of such changes have not systematically appraised their impact on the content of research.

The aim of our study is to explain how—by what mechanisms[1] and with what effects—current conditions for the early research career shape the emergence of the first IRPs. We focus on the development of IRPs rather than their later realization. We utilize a career model that includes progress in the content of a researcher's work, which makes it possible to specify the concept of an IRP as a plan for a specific career phase. Our comparison of early careers in two natural-scientific fields (biology and physics) and one humanities field (history) shows that while the development of IRPs is a widespread phenomenon, the properties of IRPs, mechanisms of their emergence, and conditions under which these mechanisms operate are highly field-specific and closely linked to a field's practices for producing new knowledge. It turns out that much more than having a brilliant idea is required for an IRP to emerge.

Theoretical Approach

Since we are interested in the impact of current conditions for ECRs on the emergence of IRPs, our theoretical approach must support the analysis of causal links between organizational conditions of research and changes in research content. Therefore, we draw on theoretical considerations that have been developed and applied in the investigation of academic careers from a sociology of science perspective (Gläser 2001; Laudel and Gläser 2008; Gläser and Laudel 2015b).[2] Building on insights from the Chicago School of Sociology (reintroduced to career research by Barley 1989) and on research on professional careers (Dalton, Thompson, and Price 1977; Zabusky and Barley 1997), our career model analytically distinguishes between three interrelated careers of a researcher:

(1) The *community career* is a series of positions in the scientific community that are defined by the reputation a researcher has accrued and corresponding role expectations. An *apprentice* learns to conduct research while working under the direction of others. Most PhD students are apprentices although this stage may already end during the PhD phase or extend to the early postdoctoral phase. A *colleague* conducts independent research, that is, selects their own research problems, approaches to problem-solving, and ways to communicate results to the scientific community. A *master* additionally acts as a teacher of apprentices. Members of the elite additionally shape the direction of their community's knowledge production.

(2) The *cognitive career* consists of thematically connected problem-solving processes in which findings from earlier projects serve as an input in later projects. These connected problem-solving processes constitute one or several distinct "research trails" (Chubin and Connolly 1982). They form a diachronic structure that gradually extends a researcher's knowledge base. The evolution of the content of research has distinct stages and structures and is closely linked to other career experiences.
(3) The *organizational career* is a sequence of organizational positions, which, through organizational role expectations, are linked to expectations concerning the conduct and content of research and opportunities to conduct research (access to salary, infrastructure, and other resources). This sequence has been the almost exclusive concern of research on academic careers so far.

We can locate the dependent variable of our investigation, the development of an ECR's first "IRP," in the cognitive career and define it as *a researcher's plan for future research that exceeds the scope of a normal project in its thematic breadth and duration*. An IRP has a time horizon, which varies from a few years to a researcher's entire active research life, and comprises a more or less specific set of problems, methods, and objects. IRPs have varying degrees of *strategic uncertainty* (the uncertainty concerning the existence of the sought-for effects) and *technical uncertainty* (the uncertainty concerning the possibility of producing the sought-for effects with the chosen approach).

The conditions that affect ECR's opportunities to develop IRPs include:

- autonomy, time, and resources for research;
- the knowledge previously accumulated by the researcher;
- epistemic conditions of research, that is, material properties of research objects and methods as well as properties of knowledge that affect the ways in which knowledge is produced in a field; and
- the knowledge of a researcher's scientific community and the latter's expectations concerning IRPs and contributions to the community's knowledge (see Figure 1).

These conditions as perceived by the researcher are likely to shape the development of IRPs by ECRs. Important intervening variables in these decisions are the ECRs' interests, which change in the process of developing an IRP, and conditions beyond the immediate work context, such as family issues.

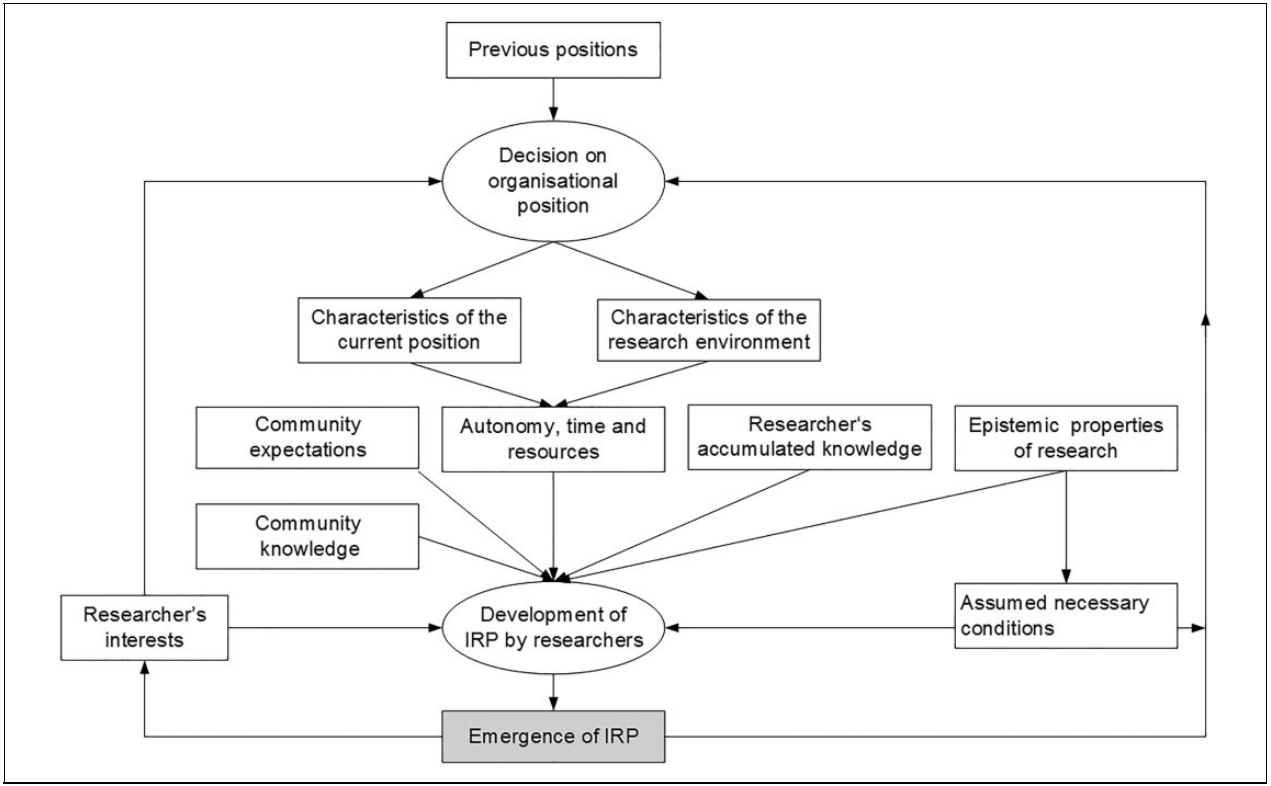


Figure 1. Factors influencing the emergence of individual research programs.

Although we can analytically set some of these variables as independent and others as dependent, there is a complex dynamic at work here because ECRs go through sequences of positions. The conditions at any time affect their career decisions, which in turn make the researcher move to different conditions and so on. This is why we included feedback loops in Figure 1.

Based on these conceptualizations, we can now address our research question by empirically identifying the necessary conditions for developing and implementing IRPs and asking how an ECR's organizational and community careers provide these conditions for researchers.

Method and Data

The project is based on comparative case studies of German ECRs from plant biology, the experimental physics field of atomic and molecular optics (AMO), and early modern history. The selection of these three fields was motivated by the substantial variation between them in important epistemic characteristics such as resource intensity, typical duration of research processes, and degree of competition, which had been observed in previous studies (Laudel et al. 2014; Laudel 2017). We chose relatively small specialties in order to minimize the internal variation of epistemic characteristics and restricted the two science fields to experimental research (thereby excluding bioinformatics and theoretical AMO physics). Including a humanities field does not pose a problem because the (fundamentally different) practices of knowledge production in the humanities can be comparatively described at the level of abstraction defined by the theoretical approach. To the contrary, the considerable increase in epistemic variance strengthens the comparison and improves the scope and reliability of our findings.

The case studies are based on semistructured face-to-face interviews in German with ECRs supported by bibliometric analyses (Laudel and Gläser 2007). We used eighty-eight interviews, eighty-one of which were conducted in the context of this project and supplemented by a secondary analysis of seven interviews from the same fields conducted in previous projects. We interviewed one senior scientist in AMO physics in order to collect background information about epistemic practices in this field. The other 87 interviews represent the cases of our comparative analysis. The number of interviews per field (Table 1) was sufficient to achieve theoretical saturation (Glaser and Strauss 1967: 65) and enabled generalizations through formulating causal mechanisms.

The interviewees were researchers who obtained their PhD between two and nine years prior to the time of the interview. We did not know the career

Table 1. Overview of Interviews.

Location of interviewees	Plant biology	AMO Physics	Early Modern History
In Germany	21 (4)	21 (1)	25 (2)
Abroad	10	11	0
Total	31	32	25

Note. AMO = atomic and molecular optics; in brackets: secondary analysis of interviews.

plans of our interviewees prior to the interview and interviewed some ECRs who had career aspirations outside of academia even after an extended postdoctoral period (see also Sauermann and Roach 2016). However, with our time window, we cast a net that returned a majority of researchers who planned an academic career and had begun to develop an IRP.

Most of our interviewees held nontenured positions, which are common in the early career stage in the German system, as postdocs, university assistants, junior group leaders, or junior professors (for an extended description of the German career system, see Waaijer 2015; Laudel 2017). Most ECRs had positions in Germany during the interview, but we also interviewed German biologists and physicists in Western European countries and the United States. In plant biology and AMO physics, postdoctoral career phases abroad are common, and we wanted to capture the career phase abroad in situ.

The interviews consisted of two main parts. In the first part, the interviewee's research was discussed. Based on the bibliometric analysis and a visual representation of the interviewee's cognitive careers (Gläser and Laudel 2015a), we explored the development of the interviewee's research since the PhD project with an emphasis on thematic changes and the respective reasons. In the second part of the interview, the questions focused on conditions of research and the factors influencing them. This included opportunities as well as constraints provided by the interviewee's organizational position. All interviews were tape-recorded and fully transcribed.

The analysis of the interviews built on a qualitative content analysis where relevant information was extracted from the transcripts by assigning it to categories that were derived from the conceptual framework (Gläser and Laudel 2013). We used the extracted information to create case histories for each researcher, which link basic data of their cognitive and their organizational careers (Figure 2 shows an example). From the comparisons

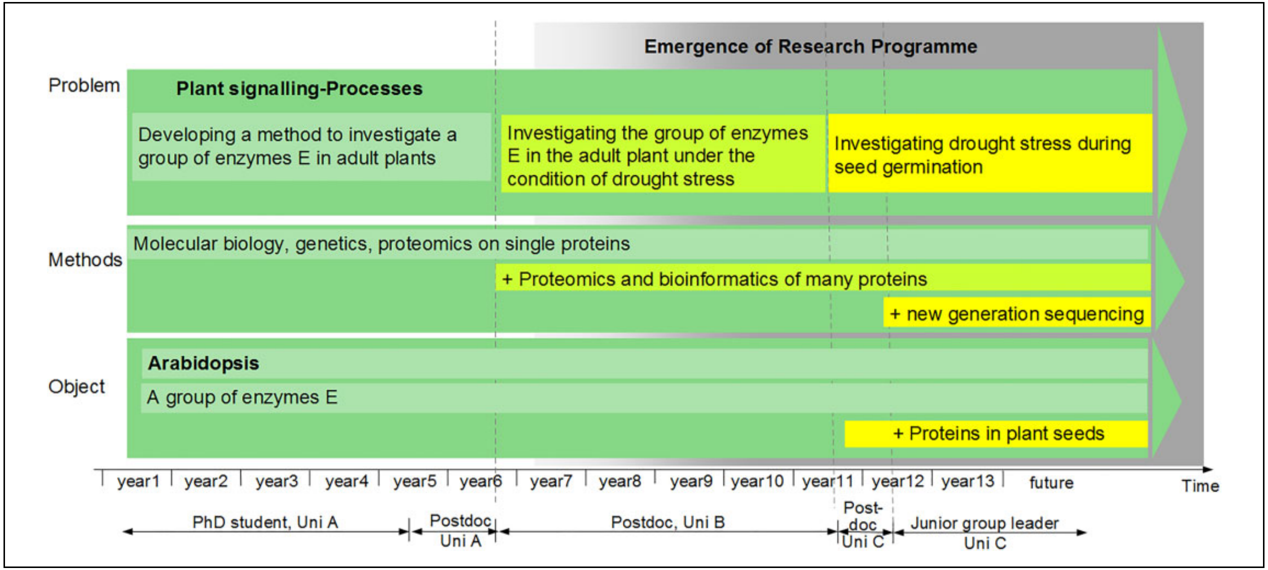


Figure 2. Cognitive and organizational careers of a plant biologist.

of these case histories, we could build and link empirical typologies of phenomena in the three careers.

Within-field comparisons of research practices, practices of developing IRPs, and the conditions under which they took place led to the identification of necessary and facilitating conditions. We established necessary conditions by a systematic analysis of reasons for failures of IRP development. The within-field comparisons revealed typical processes of IRP emergence, which were then used as models against which atypical processes were assessed. These comparisons let us identify mechanisms of IRP emergence.

The empirical material is organized as follows: for each of the three fields, we present its epistemic practices and describe the properties of IRPs and the scripts for developing an IRP. We then discuss variants of how IRPs emerged, including failed attempts, before we analyze the mechanisms that enable IRP development.

Epistemic Practices and IRPs in Three Fields

The three fields differ in their epistemic practices—in how they produce new knowledge—and consequently in the structure as well as intellectual and material bases of IRPs. By comparing our empirical cases, we could distil these practices, typical properties of IRPs, and processes of their emergence. Table 2 summarizes the IRP characteristics and processes, which we will now discuss in more detail.

Plant Biology

Epistemic practices in plant biology are aimed at empirically answering research questions about cellular and molecular processes in plant cells by investigating "interesting objects". Interesting objects are genes, proteins, cell types, or plants whose properties enable empirical studies of a particular process. They are produced in trial-and-error searches in which mutants of plants or plant cells are created and subsequently screened for changed phenotypes, expression or nonexpression of particular proteins, or other changes.[3] The set of research questions that can be answered by investigating such objects is limited and largely standardized. Questions commonly address the functions of genes and mechanisms at work at different levels of the plant. These questions are currently answered by plant biologists for a large variety of objects. Most research uses generic molecular biological methods and takes about three years. The preceding search for suitable objects may take longer. To accommodate the

Table 2. Characteristics of IRPs in Plant Biology, Experimental AMO Physics, and Early Modern History.

IRP	Plant biology	Experimental Physics	History
Based on	New empirical object	New experimental system	Existing or newly delineated empirical object (region and time span)
Aim defined by	"standard" community questions about empirical objects	Unresolved theoretical questions suggested in the literature or developed by the researcher	New question developed by the researcher
Specificity	Object: high Problem: low Methods: usually low	Object: high Problem: low Methods: high	Object: high Problem: low Methods: low
Time horizon	five years to entire research life	five to ten years	five to eight years
Strategic uncertainty	low	occasionally high	very low
Technical uncertainty	high	High	low

Note. AMO = atomic and molecular optics; IRP = individual research program.

epistemic risks involved, a researcher often works on several projects in parallel (see also Hackett 2005: 806; Müller 2014b, para. 28, 32; Fochler, Felt, and Müller 2016: 185).

An IRP in plant biology is a plan for a series of interconnected research projects that typically addresses a question about biological processes (e.g., how is a certain plant signaling pathway regulated under stress conditions) by using an interesting object. Alternatively, IRPs may simply be aimed at exploring the functions of an interesting object. IRPs in plant biology are thus commonly based on a specific empirical object, such as a specific class of proteins (see also Kerr and Garforth 2015: 7, 11). IRPs were therefore highly specific in terms of objects but not very specific in terms of methods and problem. The time horizon for an IRP could be five years or longer. Quite often, our ECRs planned their IRP even for their entire active research life.

The strategic uncertainty of IRPs in plant biology was low in most cases because the questions were generic and rarely focused on hypothesized effects. All IRPs involved the typical technical uncertainty of research in molecular biology as a manipulation of objects in trial-and-error processes (see also Hackett 2005). This technical uncertainty was considerably higher when nonmodel organisms were used that lacked the whole arsenal of manipulation tools or if the development of new methods was required.

Experimental AMO Physics

Epistemic practices in *experimental AMO physics* are aimed at answering theoretical questions by manipulating and measuring the behavior of micro-objects (ranging in size from molecules to elementary particles). Questions are derived from theory, and experimental systems are built specifically to answer these theoretical questions by creating theoretically defined conditions for specified micro-objects and measuring theoretically predicted properties. Once established, an experimental system can be used to answer a set of theoretical questions, which can be extended to some degree by modifying the apparatus.

An IRP in experimental AMO physics is a longer-term research endeavor that is based on a purpose-built experimental system and aimed at answering a set of theoretical questions. The plan consists of a theoretical idea and a corresponding design for an experimental system. IRPs are highly specific in terms of methods and objects, while the specificity of problems is low. An IRP in AMO physics can take up to ten years if it includes the construction of a new experimental system or shorter if a question is to be answered with an already existing system.

The experimental settings AMO physicists build and operate for this purpose are very complex. Building an experiment takes at least three years. The reason is that these experiments are strategically uncertain, that is, the assumptions about effects on which the idea of the experimental system is based may be wrong, in which case it is necessary to start all over again or to make considerable changes. Experiments are also technologically uncertain because many different technologies need to be controlled at the same time and require trial-and error manipulation of equipment (Laudel and Gläser 2014: 1210).

Early Modern History

Research in early modern history answers questions about societies in a specific region at a time period within or coextensive with early modernity.

Researchers collect information from archival sources and construct arguments about the society under study. Data collection is thus centered on archival work. Constructing arguments is largely a solitary activity that consists of analyzing sources and interpreting them through writing. The whole research process (from the first idea to publishing the book) usually takes five years or longer.

Correspondingly, an IRP in early modern history is a longer-term endeavor that is based on a regionally and temporally delineated empirical object, that is, a society or societies in a specified region and a specified time period. The delineation of the object could either follow traditional approaches in history or could be innovative in itself if the researcher draws geographical or temporal boundaries differently from others. In any case, the object of an IRP was always highly specific. Searching for an IRP means finding a general question about this object that could be answered using historical sources, a question that only later is specified. The sources can be entirely new (not having been used before), a new combination of sources, or traditional sources subjected to a novel interpretation. IRPs are planned for at least five up to eight years and are completed with a published book. Parallel to pursuing their IRP, early modern historians continue previous topics, particularly the topic of their PhD project, as minor research lines.

While the IRPs of German plant biologists and AMO physicists mirrored those of their colleagues abroad, the first IRPs of German historians must satisfy an additional expectation of the national scientific community, namely, to investigate an empirical object that significantly differs from that of the PhD with regard to region and time period (Laudel 2017: 360). This change of object is an implicit requirement for obtaining the tenured position of a university professor. Since German professors are expected to teach a broad range of subjects, candidates for the position have to demonstrate that they are able to conduct research on a wide range of topics, a requirement that is believed to be met when a historian successfully researches a topic that is unrelated to the topic of the PhD thesis.

IRPs in early modern history were characterized by a low strategic uncertainty because the sought-after effect is already constructed by formulating the question. Once it was formulated, there was a high likelihood that the question could be answered and that the strategies of archival search would be successful. However, there is a slight technical uncertainty because archival sources might not yield enough information.

This description of IRPs in the three fields shows that developing an IRP cannot be reduced to having the right idea. In all three fields, developing an IRP requires dedicated research activities that consume time and resources.

Major differences to be explored concern the degree to which the IRP is bound to a material artifact, the role of existing theory, the processes by which IRPs are developed and tested, and the ways in which IRPs are positioned in the community's research.

Emergence of IRPs in Three Fields

As a first step in our identification of patterns of IRP emergence, we sorted our cases in each field according to variations of their temporal sequences (Table 3). Among those who had developed an IRP at the time of the interview, three characteristic categories can be distinguished. First, there is the group of "straightforward IRP emergence" where IRP development started after the PhD and no event or process cut short or extended the work leading to the IRP. Using this group as a model, we identified "early IRP emergence" as the emergence of IRPs during the PhD phase and "extended IRP emergence" as processes that stretched out over a longer time period. As Table 3 shows, straightforward IRP emergence frequently occurred in AMO physics and in history, while in plant biology extended IRP emergence was more common.

The second group of cases includes those researchers who had not developed an IRP until the time of the interview. Among them is a category of researchers who must be considered as cases of "potential IRP emergence" because they were interviewed at a relatively early stage of their postdoctoral phase and are likely to develop an IRP at a later point in time. The second category, "IRP unlikely," comprises researchers who were unlikely to develop an IRP in the future. With one exception, they never intended to develop an IRP but instead just wanted to conduct research—which by implication was dependent research—for as long as possible. The exception is a biologist who still wanted to develop an IRP after two failed attempts but is unlikely to be successful given her current situation. A third category is "exit" cases. These ECRs intended to leave academia and pursue careers in industry or research management. From these exit cases, we only include the three researchers in our analysis whose plans of leaving academia emerged after failed attempts to develop an IRP because their cases contribute information on necessary conditions for IRP development.

Emergence of IRPs in Plant Biology

Since an IRP in plant biology is a plan for a series of interconnected research projects that explore an interesting object, finding such an object

Table 3. Forms of IRP Emergence.

IRP emergence	Forms of IRP emergence	Characteristics	Plant biology	AMO physics	History
IRP emerged	Straightforward IRP emergence	IRP emerges immediately after the PhD	5	17	13
	Early IRP emergence	IRP emerges during PhD phase	5	2	1
	Extended IRP emergence	IRP emergence stretched out due to constraints or unforeseen circumstances	12	1	7
No IRP	Potential IRP emergence	No IRP developed at the time of interview but too early to assess	2	6	1
	IRP unlikely	Long ECR career without sign of IRP emergence	5	1	0
	Exit	Intend to leave academia	2	4	3
			31 cases	31 cases	25 cases

Note. AMO = atomic and molecular optics; IRP = individual research program, ECR = early career researcher.

is crucial for developing an IRP. It happens in trial-and-error processes of producing a large number of objects (e.g., by mutation) and screening them for interesting properties. This search is risky because interesting objects might be found late in the screening process or not at all.

> A: And then I had to look at a lot of plants and phenotype them, [to assess] whether their phenotype was altered or not [. . .]. The problem is that this is not easy to see [. . .] and this is why I had to do it by microscope and that has been very, very laborious. And I identified candidates, which then needed to be checked in the next generation, but none of this could be confirmed. So this did not lead anywhere, which was a bit dramatic [. . .] because it took a lot of work . . . it just took a lot of time. You are dealing with several generations of plants and occasionally something dies, then you have to repeat it . . . . It just takes a lot of time, the screening itself, looking at all the plants.
>
> Q: How long did it take?
>
> A: Maybe about one and half years or so . . . . That might be about right, yes. And I kept trying but somehow it did not really work out. (plant biologist)

The search for such an object, which can sustain an IRP, is not completely random but guided by assumptions about the functioning of plant cells or even by a specific hypothesis.

> There was the hypothesis that these [. . .] proteins are the factors that trigger the [. . .] process and other things in the RNA metabolism of chloroplasts. And I wanted to find out whether this is true.

The role of conceptual assumptions varied, still the search for interesting objects through mutation and screening was central for all processes of IRP emergence. This search was usually integrated in projects of group leaders and enabled by the group leader's resources.

We categorized five cases as *straightforward processes of IRP emergence* where biologists found their objects and developed their IRP idea in the postdoc phase immediately following their PhD. One of them found a potentially suitable object during his first postdoc in his former PhD group. He took the object with him to test his idea during his second postdoc abroad. The work on self-defined topics and objects suggests an unusually high epistemic authority of the postdoc, which was probably supported by having obtained his own funding—a fellowship. Since he worked on his own object, there was no direct competition with his new host group. Others found their interesting objects while screening mutants in their group leader's projects and were allowed to take them with them when leaving the group. In all five "straightforward" cases, the quick emergence of an IRP was possible because the trial-and-error procedure involved yielded an early success.

The other cases deviate from the straightforward pattern in that the IRP development occurred earlier, was extended, or did not occur at all. In five cases, *IRPs emerged early*, namely, during the PhD phase and they were a direct continuation of the ECRs' PhD work. As is common for PhD projects in plant biology, the PhD students worked on objects and projects that were assigned to them by their group leaders. These objects supported research questions beyond the PhD project and thus made it possible to base IRPs on them. Further, the PhD students had a relatively high epistemic authority and were able to develop IRPs.

The use of objects assigned by group leaders led to IRPs that were closely connected to the group leaders' research programs. This created a problem that is common in plant biology: competition with the former group leaders. We observed two solutions to this problem. One ECR became a group leader in her professor's laboratory immediately after her

PhD and pursued a joint research program with the professor. She increasingly gained authority over the formulation of research problems and became an equal partner in the development and pursuit of the laboratory's research program.

The second solution was that PhD group leaders split their IRPs and "gave" the objects and part of their research programs to the ECRs.

> Well, I really kept the [enzyme] and still keep it, now that I'm a principal investigator. My PhD supervisor gave it to me. I was lucky that I could continue that.
>
> ***
>
> It actually happened that my former boss authorized me to take my [PhD] topic with me. This is really a very, very nice touch because this way I could continue my research during my postdoctoral phase [. . .]

This solution was possible when the topic and the overall number of interesting objects in the lab were large enough to support more than one IRP.

The second deviation from the straightforward pattern is an *extended IRP emergence*. Twelve ECRs needed more than three years to develop an IRP. We could identify two reasons for this "delayed" emergence of IRPs. First, some researchers used postdoctoral stays to learn additional methods such as proteomics or next-generation sequencing that required extra time. Researchers also had to learn to work with new objects and then had to adapt methods that took time, too. Second, some projects failed. Failed projects endangered a postdoc's reputation if nothing could be published and thus halted the community career, with the possible consequence of not being able to find a new position in the organizational career (see also Müller 2014a: 337). Some researchers abandoned technologically difficult projects and concentrated on easier ones in order to gain publications from their postdoc.

If the failed projects were searches for new objects, months and even years could be "lost" because the time spent on failed projects did not contribute anything to the IRP development. For example, a biologist had a vague idea for an IRP during his first postdoc in the group in which he obtained his PhD. He then applied for a fellowship and moved abroad to learn a method he wanted to use to find an "interesting object" for his IRP. However, the learning process was not successful, and the ECR could not find the candidate genes he looked for.

> Ultimately it didn't really produce positive results. Eventually I found two candidates and that wasn't quite enough. So I didn't really continue to work with them.

He began to search for a new object and IRP and was finally successful six years after his PhD. Another biologist had an idea for an IRP during his first postdoc in his PhD group. With this idea, he applied for a fellowship and moved abroad for his second postdoc where he was granted full epistemic authority in choosing his topic. However, when he tested his idea, he faced methodological problems:

> A: That didn't work at all because the constructs I wanted to make simply didn't work in Arabidopsis. We still don't know why not but it was beyond repair.
>
> Q: When did you notice?
>
> A: About one year into my postdoc fellowship.

He then resumed a topic that he had developed during his first postdoc, which "worked" and became the basis of his IRP.

Taken together, learning, the failure of projects, and the unpredictable duration of trial-and-error searches for interesting objects let first ideas for an IRP and interesting objects emerge only in the third or fourth year of the postdoctoral period. Testing the IRP's sustainability could take another couple of years. This made it necessary to have several consecutive postdoctoral positions.

We include the two cases of *potential* IRP development here because the interviewed researchers already had ideas for IRPs but were delayed in testing them. If the test is successful, they will turn into cases of extended IRP development. The first ECR found interesting mutants during his first postdoc (abroad) that seemed suitable for an IRP but was occupied with his main project and did not have the time to actually test the suitability yet. To develop that topic successfully, he would also need to find a niche in a highly competitive research area. The second plant biologist had spent the first year after the PhD in the same group to finish work there and to publish. He then used his second postdoc to learn a specific method in a different group. For his third postdoc, he moved abroad to a group where he could work in the broader field he was interested in. In this group, his main project did not succeed. However, he had learned new techniques and their application to new organisms, which was sufficient to find yet another postdoc position. After being a postdoc for more than three years in this group, he had an idea about a general thematic area and a potential organism as basis for his IRP but had not yet tested the idea.

The third deviation from the straightforward pattern is the failure to develop an IRP after attempting to do so. One biologist took on a first postdoctoral position on which she learned some methods but which did not contribute to the emergence of an IRP. In her second postdoc, she worked on a gene whose biological relevance turned out to be limited and therefore would not be suitable as a foundation of an IRP. Therefore, she started parallel work on a completely different group of genes. After sometime, it became obvious that this work led her into a highly competitive field where it would be extremely difficult for a junior researcher to keep up with the established groups working on the same topic. She changed her career plans toward an organizational career outside academia.

Developing an IRP may extend over several postdoctoral appointments in different groups. Postdocs who had found suitable ideas and objects for their IRP left their groups in order to establish their own research group, which is necessary to realize an IRP. For that, ECRs needed to take the material basis of their IRP—their "interesting object"—with them. The group leader whose resources were used to find the object has to agree to such a move, which is why ECRs had to negotiate with their group leaders. All interviewed ECRs in this situation already obtained the permission to take their object with them or anticipated that an agreement would be reached. Direct conflicts were rare, very likely because all ECRs were aware of the group leader's research interests and avoided this conflict already by directing their search processes elsewhere. For example, a researcher who had worked on two topics selected for his IRP the one that was less likely to compete with those of his group leader:

> Therefore my hope is that once we find proteins that have this modification, this would open up a new research field which would be more promising for the future than to work on a particular [regulator] on which my boss is likely to continue to work.

Although it was common that IRPs in plant biology were also thematically linked to their developers' PhD work, negotiations between the ECR and the leader of the group where they conducted their PhD were rarely necessary when they had moved to another group after their PhD. The epistemic differences resulting from the work in a different group were large enough to prevent direct competition.

Emergence of IRPs in Experimental AMO Physics

An IRP in experimental AMO physics is a plan that links a theoretical idea to a design for an experimental system. Accordingly, the search for an IRP consists of two interlinked processes, namely, learning of experimental techniques (a process that begins during the PhD phase) and the search for a theoretical idea that can be addressed with these experimental techniques. The theoretical idea could emerge in the ECR's own experimental work, be derived from an analysis of the literature, or even be directly suggested in the literature.

> Exactly, and then I had to consider what I wanted to do in the future [. . .]. And at that time I came across a paper, a theoretical paper, which suggested another effect that sounded somehow fascinating [. . .] And in this paper a possibility was presented how to get those particular particles to interact with each other [. . .] And this fascinated me. And all the requirements they were writing about, that were all things I already knew, at least to some extent. [. . .] And this seemed realistic to me because I already had some experience in all these fields.

The physicists must closely follow theoretical discussions in their community and interpret them in the light of their own experimental experiences. The idea for an experimental system cannot be directly tested in this phase because testing whether the theoretically interesting properties of the chosen micro-object can be measured with the planned experimental setting is impossible without actually building the experimental system. However, gathering knowledge about the individual technologies that will be combined in the experiment later lowers the risk that the system will not work at all or pose insurmountable technological problems.

In contrast to plant biology, the *straightforward* IRP emergence is far more common in physics. We could identify two reasons for this difference. First, the PhD phase of AMO physicists was usually longer than that of their colleagues in plant biology. Owing to the long time it takes to build an experiment and to use it for "doing real physics", PhD projects last at least four and often five years. During this time, PhD students were often involved in deriving ideas from theory papers. In other words, ECRs in AMO physics who had finished their PhD had already more research experience than their counterparts in plant biology. A second reason for the larger number of straightforward IRP emergences is that developing an IRP in AMO physics essentially means combining ideas and trying them out in thought experiments and thus does not include risky experimental research.

The common course of early career AMO physicists' organizational career was that they took up postdoctoral positions abroad either immediately after their PhD or after a short postdoc in their PhD group. They tried to find a group whose research was thematically connected to their PhD project but was sufficiently different to enable work on new objects or to learn new methods.

> And when I came here I first started in an existing experiment that was here and which I had applied for. My main motivation was to learn something about photons.

The typical postdoctoral work was not building an experimental setup from scratch but further developing or using existing experiments. This increased the likelihood that the experiments would "produce" publications during the stay of the postdoc, which was crucial for their community (reputational) and organizational careers. ECRs in experimental AMO physics face a particular problem here because it takes a long time before the experimental systems produce publishable results.

During the postdoctoral phase, ECRs became increasingly involved in decisions about changes of the experimental system. Such changes became necessary for two reasons. A first reason was the high technical and strategic uncertainty of these experiments. Building an experimental setting that "works" still includes "tinkering" because each experiment is a new combination of methods and thus of equipment. The second reason was that the experimental systems supported research on several theoretical ideas but needed to be adapted to the exploration of each new idea. Participating in this development of experiments constitutes the major learning process in experimental AMO physics. By deriving ideas from the literature, adopting methods, and technically realizing them in experimental systems, ECRs learn about the link between theories and experiments as well as the theoretical potential of experimental systems. This knowledge is essential for developing a feasible IRP.

The process of accumulating knowledge is not risky and continues regardless of the actual success of individual experiments (failure also contributes to learning). An IRP usually emerges as a new combination of knowledge about methods and/or objects from the PhD with those from the postdoc.

> Indeed it is typical that technical methods which you learned as a PhD student and as a postdoc . . . that you combine these technical methods and create something new. (Senior group leader)

Although changing groups was common among ECRs in AMO physics, we had several cases where physicists stayed in their PhD groups and continued to work on the experimental setup they had built. Their research gradually changed from conducting dependent research (executing their professors' ideas about building or adapting an experimental setup) toward realizing a joint research program in which they and their professor equally contributed to the development of experimental designs and research questions. These physicists developed their IRPs for experimental systems that already existed in the group in which they conducted their PhD research. Since new theoretical ideas could be easily implemented in the already existing systems, processes of developing, testing, and realizing IRPs merged.

In two cases, IRPs emerged *early*. The two researchers spent their PhD phases building experimental systems that could be used to explore a wide range of theoretical ideas. They stayed in their PhD groups on postdoctoral positions, where they were granted an unusual degree of epistemic authority by their professors, who either had different interests or had taken on more managerial tasks.

> Well, I had the major advantage that my bosses trusted me. Nobody ever said to me, "do this, do that." They gave me at the beginning all freedom to decide what to do. Then I got it, started it and developed it all independently.

The two ECRs became group leaders in the laboratories of their professors and began to realize their IRPs.

Only one case of *extended* IRP emergence occurred. This ECR became a postdoc in a group in which he contributed to building an experimental system from scratch rather than selecting a group with a well-developed experiment that was unusual. Such a decision is risky because the experiment may fail or its completion may be delayed due to strategic and experimental uncertainties, in which case, the postdoc might leave the group before the experiment leads to any publishable results. However, in his case, the extended postdoctoral stay of four years led to publications and to the emergence of an IRP.

Six further researchers will belong to the category of extended IRP emergence if they develop an IRP (*potential* cases). At the time of the interview, they were third-year postdocs who had a general research direction but not an elaborated IRP. Yet, they intended to develop an IRP in the future and to become a group leader.

> I would like to investigate a combination of this [wave length] and optical things. [. . .] It would be nice to find a physical question that could be done at this [wave length] where it can be conducted in a simple, easy way and where it can be developed step by step. Or to find a combined question between the two. But I cannot tell you a concrete question yet.

One had a tenured position below the professorial level and conducted dependent research in the research program of his professor. He described initial ideas for a new experimental system.

> These are setups which are technically complex and from which we now want to get results, in particular in this surface trap. And that's why we decided to push these projects and to postpone the development of my own concepts. Doing this, I'm also learning and will later know what a good concept is.

If these ideas are realized, a joint IRP of the ECR and his professor is likely to emerge.

Another case must be considered as a failed development of an IRP because the development was clearly intended but did not succeed. The postdoc worked on an experimental system that did not function as expected. After one and a half years of coping with technical problems, it became obvious that the experimental system was fundamentally flawed and needed to be rebuilt from scratch. He considered the damage to his community career to be severe—he had invested two years with no prospect of producing publications—and decided to leave for a nonacademic career.

> And I thought the problem with the mirrors is solved and I planned my experiment accordingly. But after one and a half year they [the other group members] realized: we can't solve this problem, at least not the way we have tried so far. And my whole plan became irrelevant because it was impossible to build upon that. First, the problem needed to be solved. That's why I didn't get to the point where I wanted to be after these two years. Because as a postdoc you somehow need to "shine" with your own research, to develop something for your own research in order to be able to apply for a grant.

In contrast to their colleagues from plant biology, ECR physicists rarely negotiated IRP topics with their group leaders. This can be explained by the fact that ECRs took only conceptual designs for new experimental systems with them when leaving the group.

Emergence of IRPs in Early Modern History

Developing an IRP in early modern history involves delineating an object and formulating a relevant question about it that can be answered based on existing archival material. ECRs had to develop these ideas on their own rather than taking them from the literature or taking up concrete suggestions made by their professors. They developed ideas for IRPs by going through their personal repository of copies from archives, reviewing topics they had previously encountered (e.g., topics they worked on during their studies, thematic areas of their professors, or other specific interests they developed), and by receiving inspirations from the literature. Current trends in historical research did play a certain role when historians searched for an IRP but had to be "processed" into a unique individual perspective.

Although there is little competition in early modern history, researchers face the risk of redundancy, which they avoid by observing the community's research. They must also check the availability of sources and their yield for making the intended historical argument. Whether a topic is sufficiently substantial in these respects to provide a sustainable IRP may become apparent only after a lengthy process of checking the literature and original sources.

> [. . .] and I started research on this topic. And it quickly became frustrating because I realized that it already had been researched to death. Which is something you do not realize when you invent a [grant] application within six weeks. This topic has been fashionable in the cultural sciences since the 1980s. And it was not only addressed by historians but also by lots of German philologists and other literary scholars [. . .], so the topic really was exhausted. Thus, after about one year and a half I contemplated what can be done with it. And then I went into the direction I was more interested in anyway. (Historian)

Developing an IRP in history means finding an idea for a "habilitation" project, the second major book that is expected in the German humanities and many social sciences.

Straightforward cases dominate IRP emergence in early modern history. The vast majority of interviewed early modern historians developed their IRP very soon after their PhD, usually on a research and teaching position or on a postdoctoral fellowship. Occasionally, the phases of nonacademic employment or unemployment were also used for the search for an IRP. In the first year after completing their PhD, ECRs were often still occupied with publishing their PhD thesis as a book. They began the search for an IRP immediately after or parallel to the publishing process.

Early IRP emergence is uncommon in early modern history, mainly because it would require researchers to develop a completely unrelated topic while still working on their PhD projects. Only one PhD student had an idea for an IRP that was sufficiently distant from his PhD and began testing its sustainability by checking the literature. At the end of the PhD, the topic was still rather vague but already substantial enough to secure him a postdoctoral grant. He bridged the time between the PhD completion and the postdoc abroad by taking a position at his PhD institution, which allowed him to develop the topic further.

We observed seven cases of *extended* IRP development and identified three characteristic reasons for that. Two ECRs were not interested in developing an IRP for a while because they became distracted. One of them worked outside academia and initially planned a nonresearch career until she was offered a postdoctoral position. The other became sidetracked by the topic of his postdoc position, which was interesting but unrelated to his plans for an IRP. The second reason was that the idea for an IRP was not sufficiently different from the PhD project, as in the following example of a historian who had to develop a topic for an IRP under time pressure.

> This was planned as a postdoctoral project. And I applied for postdoctoral fellowships but was always rejected. I didn't have a position and needed to continue immediately. I could bridge half a year, but this meant I needed to apply quickly for postdoctoral projects. [. . .] And therefore I had maybe one, two months to design a project. And I had no chance to start a completely new topic. [. . .] I got the reviews of the professors from [the funding agency]. That was interesting. They said that it was obvious that the project was designed under great time pressure and it was so close to my PhD topic that I wouldn't have any chance in the German research system.

The researcher had to move to a nonacademic position, on which he developed a new plan. Due to the lack of time for research on this position, the IRP development took several years.

The third reason is that not all of the ideas for IRPs survived the test of sustainability. One researcher had to drop his idea for an IRP after a year and a half because he discovered that others had already extensively studied the sources he had selected. Another historian worked as a postdoc on a research topic set by her professor and faced a similar problem of "exhausted sources." After two and a half years, she moved to a different position and substantially changed the focus of her research. A third researcher's IRP failed because it didn't pass the "archival test." The research question could not be answered with the available sources. Finally, a researcher only realized after a while that her idea for an IRP didn't really match her interests.

> My boss said: "this is my project that I want to do and I would like to have somebody for that." And then I did it. And I believe I wrote a fairly convincing proposal for the job application. I also was a couple of times in the archive for that. But it was nothing that I felt really comfortable with.

She abandoned the IRP after a year and turned to a new idea she had begun to develop in parallel. The fact that she did so with the approval of her professor, who initially had other ideas for her work, indicates the field's great emphasis on individual decisions on topics.

Again, the case of *potential* IRP development will also turn into extended IRP development if successful. The ECR in question had started to develop an IRP shortly after her PhD. She found a topic, had secured a grant to conduct archival studies, and tested the IRP's sustainability. After taking up a research and teaching position, her new professor discouraged her to pursue this topic:

> Uh, there was a whole list of arguments. [. . .] It should be international [. . .] It would have been just German [. . .] because it is all German territory. The second was that it does not fit in the research context here. And then there were also concerns that it is too close to the previous topic [. . .]. The "century" was not far enough away. This is a question how you present yourself career-wise. That you cover all centuries and show that you are proficient from 16th to 18th [century]. [. . .] Then you must show that breadth, meaning in terms of time and thematically. And I must say that made sense to me.

The topic obviously did not fulfill the necessary criteria of thematic breadth that is expected from historians in the German career context. The fit to the local research context was a rather unusual demand that we did not find in other cases. The historian has started to search for an IRP anew.

IRPs in early history did not need to be approved by professors. The latter could discuss a certain topic and give advice but could not prevent an IRP from being developed and realized. Since professors worked on topics that differed from those of their postdocs, we did not observe negotiations about IRPs. The postdoc positions themselves set few thematic constraints. Researchers who applied for positions in larger research clusters had to fit into the latter's thematic framework. Since the framework was usually broad enough, thematic constraints did not prevent ECRs from finding their own topic for an IRP.

Conditions for the Emergence of IRPs

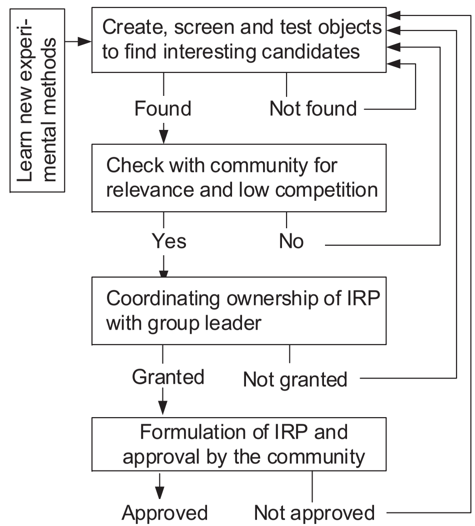
The preceding discussion of processes of IRP emergence demonstrated that developing an IRP is a research process in its own right, which may extend over a considerable period of an ECR's career. Finding an idea for an IRP and testing its sustainability requires systematic work. In plant biology and early modern history, the search for an IRP and its test include empirical work. In this section, we will discuss the social mechanisms of IRP emergence that we identified through our comparative analysis. We specify the initial conditions, the sequence of events, and the operating conditions of these mechanisms (Table 4).

A first set of initial conditions consists of the intentions and strategic knowledge of ECRs. An ECR must be interested in pursuing an academic career, must believe they need an IRP to do so, must know what counts as an acceptable IRP in their community, and must intend to develop one. These conditions did not coincide in all our cases. We found researchers who wanted to pursue an academic career but never tried to develop an IRP. At least some of them were not aware that IRP development was required and how this is done in their field.

An ECR's capabilities, which are partly intrinsic and partly produced by their previous cognitive careers, form a second set of initial conditions. Being able to develop an IRP depends on sufficient knowledge about empirical methods and theoretical approaches. In biology, ECRs need to have excellent skills in experimental methods. Developing an IRP in experimental AMO physics requires knowledge about experimental approaches and theoretical knowledge. In history, knowing how to delineate an object for study and how to work with historical sources are paramount, while theory is less important.

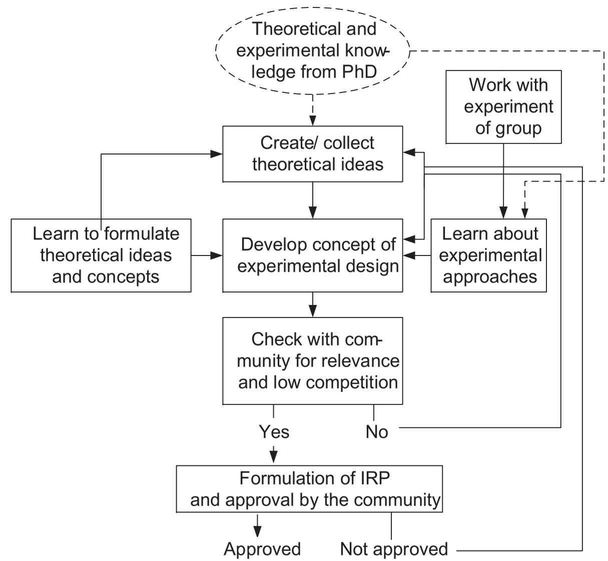
Since the development of an IRP is a research process, conditions for conducting such research constitute the third set of initial conditions. This

Table 4. Mechanisms of IRP Emergence in Three Fields.

Type	Initial conditions	Sequence of steps	Operational conditions
Emergence IRP (biology)	Interest in pursuing an academic career Knowledge how to develop an IRP Knowledge about experimental approaches Access to experimental infrastructure and resources Time for research		Persistence of initial conditions Discretion over time for search of new objects Sufficient reputation to get the next position

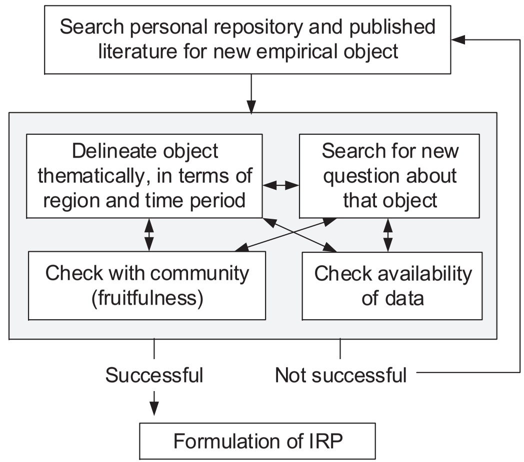
(continued)

Table 4. (continued)

Type	Initial conditions	Sequence of steps	Operational conditions
Emergence IRP idea (Physics)	Interest in pursuing an academic career Knowledge how to develop an IRP Knowledge about experimental approaches and about Theory Access to experimental infrastructure Time for research		Persistence of initial conditions Partial authority/ opportunities to shape experiment Discretion over time for developing own ideas Sufficient reputation to get the next position

(continued)

Table 4. (continued)

Type	Initial conditions	Sequence of steps	Operational conditions
Emergence IRP (history)	Interest in pursuing an academic career Knowledge how to develop an IRP Access to infrastructure/ sources/ data Time for research		Persistence of initial conditions Epistemic authority over research Sufficient reputation to get next position

Note. IRP = individual research program.

includes access to research infrastructure, which means equipment, consumables and breeding facilities in plant biology, experimental infrastructure (laser systems, etc.) in AMO physics, and libraries and archives in early modern history. Furthermore, the development of an IRP requires time for research.

The sequence of steps leading to an IRP varies considerably between the three fields. In *plant biology*, the typical sequence includes four major steps:[4] First, ECRs create, screen, and test objects in order to find interesting candidates. As we have seen, these activities are characterized by trial and error and may fail, in which case they need to be repeated. The search for interesting objects is accompanied by continuous learning of new experimental methods for manipulating biological objects. Some ECRs dedicated a whole postdoctoral stay in a group to this learning. Second, ECRs need to check with their scientific community whether the object they found is relevant (i.e., addresses a biologically relevant process) and the competition is low (see also Hackett 2005: 812-814). Third, the ownership of the object must be negotiated with the group leader whose resources were used and whose research program often initiated the search. The final step is the formulation of an IRP. The IRP often takes the form of a grant proposal or an application for the position of a group leader. This means that the IRP needs to be approved by the scientific community, which controls the allocation of positions and resources. In this multistep process, failure may occur in each step and may force the ECR to start again.

The described mechanism functions if the initial conditions persist as operating conditions during the whole period it takes to realize this sequence of steps. A second operational condition is access to material resources with which ECRs can realize their own side projects. Furthermore, ECRs must accumulate reputation. Developing an IRP in plant biology usually extends over several postdoctoral positions. For the mechanism to operate successfully, the work on each position must yield reputation (through publications) that is sufficient to secure the next position. When the researcher has an IRP, the accrued reputation must support the successful application for the position of a group leader. Given the high uncertainty of projects in plant biology, not all ECRs met this condition.

In *AMO physics*, the first step toward an IRP is creating and collecting ideas for interesting theoretical questions and corresponding experimental systems. This includes learning to formulate theoretical ideas and concepts, a process that starts with the PhD phase and continues in the postdoctoral phase. In a second step, the general idea for an experimental system must be developed into a detailed design. This required ECRs to accumulate knowledge about experimental approaches, which they did by working on experiments of their group leaders. Since IRPs in AMO physics often combine experimental technologies the ECRs acquired in their PhD project with technologies learned in the postdoc phase, the PhD directly contributes to IRP development. In a third step, ECRs need to check the relevance and originality of their idea. If the idea is original, it differs from the IRPs of the other known groups in the field, which means that direct competition can be avoided. ECRs monitor the literature and discuss their idea with their group leader in order to assess its relevance and originality. Negotiations were not common. Instead, early career physicists observed the research of their group leaders and other community members and adapted their IRP accordingly. In a fourth step, the IRP is formulated and must be approved by the scientific community. In AMO physics, this fourth step is particularly risky for the community because empirical tests of the conceptual idea prior to realizing the IRP are rarely possible. This is why the community must decide to give a young physicist a group leader position and considerable resources (half a million Euros or more) solely on the basis of a grant proposal and the ECR's reputation. Failure to gain community approval leads to a new search for another theoretical idea or to adaptations of the experimental design.

This mechanism operates under conditions similar to those in plant biology: the initial conditions must persist, and ECRs must gain sufficient reputation through publishing results from their postdoctoral work. Not being able to publish enough was a constant worry for ECRs in AMO physics, too. They could influence this condition only by carefully selecting groups with "productive" experiments for their postdoctoral stays. In addition to these conditions, access to an experimental setup where ECRs can acquire new experimental knowledge and partial authority over the experiment are operating conditions for this mechanism. ECRs must be able to contribute ideas that advance theory or improve the experimental system. Changing an experiment for theoretical reasons and testing the impact of the changed setting on the research object is the central process through which ECRs in AMO physics learn. In this process, they learn to identify scientifically doable problems (Fujimura 1987).

In *early modern history*, the first step of IRP development is the search for a new empirical object. In this search, ECRs utilized their personal repositories of literature and archival material gathered in previous research. This step was followed by several interrelated simultaneously occurring processes. German historians must delineate the object thematically, regionally, and temporally in order to create a doable IRP that is sufficiently different from their PhD topic. At the same time, they must formulate questions about that object and visit libraries and archives to ascertain the availability of data. As in plant biology and physics, a calibration of the IRP idea with the community is required. Since there is little direct competition in early modern history, only the fruitfulness and newness of the idea must be checked to avoid selecting a topic that is already exhausted.

The last step is the formulation of an IRP. In contrast to plant biology and experimental AMO physics, early modern history is not a field in which IRPs must be formally approved and funded by the scientific community. While some researchers applied for positions where they needed to outline an IRP idea, many did not, and initial ideas could easily change. The individualized nature of research processes in history and the need to develop one's own perspective limit opportunities for judgment by others. Failure may occur—as we observed in several cases—because necessary data are not available, the topic is exhausted or the IRP is too close to the PhD topic.

Operational conditions in history also include the persistence of the initial conditions and the necessity to gain reputation. For the latter, publishing the PhD thesis as a book is crucial. Epistemic authority is not only an essential condition but also commonly granted to all researchers. Interviewees emphasized that any research topic they work on must primarily match their own interests. A topic that is not autonomously formulated by the researcher from their own perspective does not "work" and cannot be the basis of an IRP.

Having established the mechanisms of IRP emergence in the three fields, we now turn to a last critical point and ask how the positions that ECR held met these conditions. IRPs emerged on different types of formal positions: on postdoctoral positions funded by the group leader, postdoctoral fellowships, on research, and teaching positions of varying length. Interestingly, we could not find any systematic correspondence between types of positions and IRP emergence. In some cases, epistemic authority may have increased for postdocs who brought their own salary in form of fellowships. However, these postdocs still depended on the group leader for access to resources. Fellowship applications were also often cowritten by the group leader and the ECR to support the group leader's research program. In history, fellowships could give researchers a kick start because they provided more time for research. However, we did not find striking differences between IRP emergence in fellowships and research and teaching positions in history either.

Overall, the actual conditions for IRP emergence did not depend on an ECR's formal employment position but on the authority over research time and resources informally granted by the group leader (see also Owen-Smith 2001 for postdocs' varying autonomy in a neuroscience lab). We found some professors in AMO physics to be very generous in that respect. Due to the group structure of plant biology and AMO physics research, ECRs in these fields were much more dependent on senior researchers (group leaders and professors) than early modern historians. ECRs in modern history were even able to begin the development of IRPs on nonacademic positions.

Conclusions

Our study demonstrated how the transition to independent research in the early career phase is linked to developing IRPs, what field-specific mechanisms produce such IRPs, the conditions that trigger these mechanisms, and the conditions that keep them operating once they have been initiated. The study has been conducted for a specific national career system, namely, the German chair system (see Laudel 2017, for a comparison of career systems). Nevertheless, we believe some generalizations are possible. In particular, whenever becoming independent (in the sense of formulating and researching one's own research problems) is linked to becoming a group leader, it is very likely that scientific communities grant the necessary investments for building a group only if a research program is provided that justifies the investment. This would make developing an IRP essential in the group-based research fields in all major career systems, that is, the chair system, the tenure-track system, and the tenure system.

Our findings show the importance of the scientific community for the academic career. The development of IRPs depends on the knowledge accumulated by communities, the competition ECRs face with particular IRPs, and the reputation ECRs accrue in these communities. Our findings thus confirm the body of empirical research that proves the scientific community to be the primary referent of researchers' actions. More specifically, our findings agree with observations in US research groups (e.g., Hackett 2005; Owen-Smith 2001), and our interviews showed no difference between the situation of German postdocs in German research groups and German postdocs in France, the UK, Switzerland, Austria, or the United States. National science systems appear to differ primarily in the number of researchers and groups whose participation in international scientific communities and their epistemic practices they can support. Experimental AMO physics in particular is a field for the rich, that is, for the global North/West.

We interviewed researchers who wanted to stay in academia but did not want to become group leaders. These ECRs had specialized knowledge and important functions in their groups' research, yet could not stay because they worked on fixed-term contracts. Further empirical research would need to show what career options for dependent research (which can be very valuable, see Gläser, Spurling, and Butler 2004) exist in different career systems.

Although we could confirm our assumption of a strong community expectation linking independence to individual IRPs, we also found several cases of joint research programs. This raises the question whether the increasing collaborative character of research erodes the strong emphasis on IRPs. Future research must show how the different career systems support the conduct of such joint programs, which effectively require joint group leadership.

Our investigation leads to three conclusions. First, understanding the early career is impossible without understanding the changes in ECRs' research that occur during this time. The exclusive focus on sequences of organizational positions, which has dominated research on academic careers, is of limited use for understanding what ECRs do. The dynamics of research content (the search for an IRP while conducting dependent research), of the growth of an ECR's reputation, and of sequences of organizational positions are interlinked, and the coupling of the three dynamics varies between fields and within fields over time. For ECRs to be successful, all three careers must be aligned.

Second, this interaction among the three careers of an ECR challenges current political concerns about the length of the early career phase. We saw that ECRs cannot always predict the suitability of a position for IRP development because suitability depends on both epistemic factors (the group leader's research program and the knowledge available in the group) and idiosyncratic factors (such as the authority group leaders grant their postdocs). The search for IRPs in all three fields also includes an element of chance: parts of the search or whole searches might fail and need to be repeated. While the increasing length of postdoctoral phases is often exclusively ascribed to decreasing numbers of tenured positions and considered as an unfavorable "holding pattern", our analysis provides an additional explanation. Increasing epistemic uncertainty and competition may lengthen the search for the IRPs that enable independence, thereby creating longer phases of postdoctoral employment. Thus, science policy measures to limit the postdoctoral period to a maximum of five years (Nerad and Cerny 1999; NRC 2005: 4; Powell 2015) or six years (current German law) appear to be ill-advised unless favorable conditions for the development of IRPs can be guaranteed within that period. This seems unlikely, not least because increasing competitive pressure on current group leaders pits the interests of group leaders against those of their postdocs.

Third, our investigation clearly demonstrated that it is rather pointless to attempt theoretical conclusions about the early academic career without taking field-specific processes and conditions into account. Developing an IRP is a research process and thus consists of field-specific epistemic practices. These empirical practices and their inherent uncertainties are an important reason why IRPs often take a long time to emerge, most notably in plant biology. In the two science fields, another reason for the long duration is the dependence on the scientific community's approval of IRPs in terms of granting resources. In early modern history, the far-reaching decoupling of IRP development from the authority of professors and organizational resources meant that nonacademic positions played a significant role in the early academic career. If these and further differences are not taken into account, outcomes of career research will remain limited to descriptive statements about "success" and "concerns" of ECRs.

Acknowledgments

We would like to thank Jochen Gläser, the participants of the workshop "Academic Careers and Knowledge Production" (Berlin, September 2016), the science studies group at the Center for Technology and Society (TU Berlin), and the two referees for their helpful comments on earlier versions of this article.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: This work was supported by the German Ministry of Education and Research, grant no. FoWin004.

Notes

1. Following Mayntz (2004: 241), we define a social mechanism as a sequence of causally linked events that occur repeatedly in reality if certain conditions are given and link specified initial conditions to a specific outcome.
2. Further empirical applications of this model can be found in Wöhrer (2014), Höhle (2015), Laudel (2017), and Holley (2018).

3. Similarly, Knorr-Cetina (1999) observed epistemic practices in molecular biology as characterized by trial-and-error approaches and “blind variation,” with failure being part of everyday experience (pp. 84-98).
4. The sequence of steps outlined here is of course an ideal-typical description. In real life, these steps may occur—and do sometimes occur—in a less linear way.

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