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Tales of serendipity in highly cited research: An explorative study

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Abstract

Research and innovation are attributed a growing role in maintaining global competitiveness; in particular, research advances are seen as important catalysts for innovation and growth. However, our understanding is still limited concerning how important research results are achieved. This is particularly the case for the role of serendipity, where discoveries or the path towards them are unexpected. This paper explores through the use of a narrative approach the role of planned and unplanned factors and presents elements for understanding how and when serendipity occurs in highly cited research. In this explorative study, we have interviewed 12 first authors, each of whom has played a key role in a highly cited piece of research. Their own perceptions of how research progressed, key turning points and conditions for the research are important in illustrating what motivates and influences the researchers' pursuit of new discoveries. The narrative approach, by introducing a temporal element, is both able to characterize the stories behind the advances, including key turning points in achieving research accomplishments, and to analyze cross-cutting themes related to researcher behavior and environment for the research.

Keywords: highly cited research, serendipity, narratives, research climate, academic freedom

Total word count: 9833

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Introduction

Research and innovation have been attributed a growing role in maintaining global competitiveness and socio-economic progress; in particular, major research advances are seen as important catalysts for innovation and growth. This has heightened focus on monitoring scientific excellence and how it can be fostered (OECD, 2014; Sørensen et al, 2016; Young, 2015).

While there clearly is a renewed interest in scientific advances, studies of science have long been concerned with new discoveries, how they occur, and their impacts. For example, the pursuit of new discoveries was a focal point in much work on the sociology of science in the 1950s and 1960s by Robert K. Merton and others². Merton (1957) argued that the main driver of research advances is priority of discovery and the recognition of the scientific community as being first to create an advance, while Hagstrom (1965) emphasized the intrinsic satisfaction of making a discovery.

Merton and Barber (2004) review a number of personal accounts and opinions by researchers that emphasize the unanticipated nature of many advances, arguing that deliberate and planned research may oversee important discoveries, or that discoveries may be substantially different from what was planned – for which they introduce the term serendipity. In defining this, they quote Irving Langmuir (former director of General Electric laboratories in the 1940's and 1950's): “*You can't plan to make discoveries. But you can plan work that will probably lead to discoveries [...] you can organize a laboratory so as to increase the probabilities that things will happen there. And in doing so, keep the flexibility,*

² For example, H. Zuckerman, S. and J. Cole, R. Andrews, B. Barber and W. Hagstrom.

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keep the freedom.”³ This distinction between planning work processes and constructing work environments is important to further work on serendipity.

Serendipity can be defined as “the act of finding answers to questions not yet posed” (Stephan 2010). However, serendipity can also be linked to the discovery process in different ways (Murayama et al. 2015). Roberts (1989) distinguishes between “pseudo-serendipity”, “the accidental discovery of ways to achieve an end sought for” and “(true) serendipity, which describes accidental discovery of things not sought for”, though he notes that both can be equally important in the contributions that they make. Yaqub (2018) seeks to characterize the different variants of serendipity and how it is perceived by examining examples of serendipity documented in Robert K. Merton’s archive of notes and materials. From these known examples of serendipity in the archive, he identifies four types of serendipity based on the motivations (did the research have a specific problem in mind) and outcomes (did they solve that problem or an unexpected one) of the research: Walpolian serendipity, targeted search solving unexpected problems; Mertonian serendipity, targeted search solving expected problems via unexpected routes; Bushian serendipity, untargeted search solving immediate problems; and Stephanian serendipity, untargeted search solving later problems.

Even with this recognition of a potential role for serendipity, our knowledge of how scientific advances occur is fairly limited. Heinze et al. (2009) examines 20 cases of highly creative research accomplishments within nanotechnology and human genetics. They analyze group level and broader organizational and institutional factors. The case studies confirm widely held beliefs that small group size and autonomy are important for scientific excellence, and also found that intensive within-group communication was vital, though close contact with

³ Irving Langmuir, address to the G.E. Research Colloquium (December 1951), partly paraphrased and partly quoted in G.E. Shareholders’ Quarterly (25 July 1952). Taken from Merton and Barber (2004), pp. 200-201.

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external groups were also important. Hemlin et al. (2008) also consider the role of research environments in promoting scientific creativity. They develop the concept of “creative research environments” and outline a framework that identifies the main institutional, individual and group-related factors that act to promote scientific creativity.

Stanley and Lehman (2015) use computer science research into artificial intelligence to develop a theory about “non-objective” search and discovery, which has a number of implications for understanding how research advances come about. They argue that advances are more often the result of searches for novel findings as opposed to research driven by broad-reaching objectives. Given this, an over-reliance on goals and objectives can be counterproductive: “Objectives might sometimes provide meaning or direction, but they also limit our freedom and become straitjackets around our desire to explore (Stanley and Lehman: 3).”

Muruyama et al. (2015) explore the role of managerial and organizational factors for serendipity. They examine whether the pursuit of serendipity has a positive effect on the quality of research and how increased division of labor and management influences researchers’ ability to pursue unintended findings. Their analysis is based on a survey of researchers, both authors of top 1% highly cited papers and a randomly selected control group, in the US and Japan. They find that the pursuit of serendipity leads to greater citation impact, and that managers that are also leading scientists have positive impact on serendipity. In contrast, professional research managers are associated with higher productivity, but at the cost of less opportunity to pursue unexpected results.

This paper approaches serendipity differently than the authors in the brief literature review above in two respects: first, the cases are not pre-selected for serendipity and second, by introducing a temporal element through a narrative approach, we explore the micro-level of

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elements that occur within the research process, the when and how of serendipity. In contrast with Yaqub (2018) who begins with known examples of serendipity from Merton's archive, we approach the topic with a small number of cases that have not been pre-identified as serendipitous; instead they are pre-selected by their high-citedness. Through a narrative analysis, we seek to learn more about how the first authors of 12 highly cited papers have experienced the research process behind these papers and especially the role of planned and unplanned factors. We look at their perceptions of the process from idea to outcome, key turning points, and factors that influence how the process unfolds. By interviewing the lead authors of highly cited papers, we hope to generate insights for refining future studies of serendipity and the role of planned and unplanned factors in highly cited research. The aim is to learn not only about the role of serendipity in the 12 concrete cases, as the lead authors of these papers have experienced them, but also to explore how serendipity can be understood and characterized, about possible variations within existing typologies and additional types of serendipity, and the stages in the research process at which serendipity can occur.

We examine the above listed questions through the narratives of first authors of 12 highly cited papers. We believe that their perceptions of how the research progressed, the experience of key turning points and the conditions for the research are important in illustrating what motivates and influences researchers' pursuit of new discoveries. However, it is important to emphasize that our study should be seen as an explorative step in refining how to go about researching these questions. Thus, we hope our study will inspire other researchers to examine these questions in more depth. Especially, we imagine that the typology, presented in the 'Discussion' at the end of the paper, can be developed further and tested empirically in larger samples by other scholars.

We have chosen to focus on the narratives of highly cited research for a number of reasons.

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First, high-citedness is valued by both researchers and policy communities and is also the subject of indicators of excellence, making it very relevant to learn more about the research that lies behind highly cited work. Second, while papers can be cited for a number of reasons and high citation is no guarantee of quality, high-citedness nevertheless reflects strong visibility and is often associated with important research results. As described below, we have manually eliminated articles that were highly cited for reasons that *prima facie* did not have to do with quality or novelty, i.e. review articles or software. Third, we view highly cited papers as an interesting subset of research work that is restrictive but at the same time holds more variation than, for example, if we were to limit ourselves only to high profile work that has been recognized through prizes and related review processes. Finally, it is also interesting how researchers themselves perceive the fact that their paper is very highly cited, a point that is explored in the analysis.

The next section outlines the approaches used in the analysis. Following this, we present the results of the study and give examples of how serendipity shows up in the examined cases.

The final section concludes with a discussion of implications for future research into serendipity's role in highly cited research. Although serendipity has been researched for more than 60 years, we still know very little about its role in highly cited research. We hope that our paper will inspire others to examine this question further. Only in this way can we redesign funding schemes so that they are open to serendipity, when this phenomenon occurs.

Methodology

This interview study explores how highly cited research is created by using narrative methods. It takes a closer look at the factors that in the eyes of the first authors have helped make this research possible. It looks at the factors that have stood in the way, and other

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conditions that have influenced how the research was conducted. The purpose of the study is to a certain degree dual in nature. We are first interested in unfolding the stories behind the advances and the sequence of events that took place, from its beginnings to achieving a result and its publishing. The second concerns a number of cross-cutting themes and how they influence highly cited research. Both the structuring of the interviews and in particular the coding and analysis procedures reflect this dual purpose.

The interviews are semi-structured where priority was given to allowing the researcher to tell the story behind their major advance. The main questions from the interview guide are shown in box 1.

Box 1. Interview guide– main questions

- Q0. To start out, we would like to hear your assessment of the importance of this paper. It was highly cited within its area, but would you also consider this paper to document an important advance? Can you describe the most important contribution(s) of the paper in layman's terms?
- Q1. What would you consider the origin or starting point of this particular research? When did the idea come up?
- Q2. Can you describe the conditions for the research at that time? (work position and conditions, ambitions, funding, etc.)
- Q3. How did the research progress over time? What went right and what went wrong? Did you have the freedom to change directions or redesign research plans during the process?
- Q3b. At what stage in the process did it become clear to you that you had achieved an important advance? Did you know what you wanted to achieve from the beginning? Or did the idea come up unexpectedly from other results? Would there have been any negative consequences if you had not been successful?
- Q4. What kind of impact did this paper have on careers and access to funding for you and the other authors? Promotion, new position, or did it lead to a large grant?

We employed the following procedure to select cases for the interviews. Drawing on Schneider and Costas (2017) for each sub-discipline, the most highly cited paper was

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identified for the two-year period 2010-2011 (using a three-year citation window⁴). Reviews, meta-analyses and reports of statistics were removed. Furthermore, we chose to exclude papers within the social sciences and humanities due to concerns that advances in these areas would be fundamentally different and would make it too difficult to cover given the relatively small number of interviews in this study. Finally, we focused on papers that included a small number of institutions (however, no restrictions were made concerning the number of co-authors), limiting to at most three research institutions. Papers with extremely large number of authors and institutions are likely based on very different research processes (in some cases, multiple parallel processes), and we wanted to avoid adding this additional dimension to our analysis. After the above exclusions, we drew our sample from these most cited papers. These were then manually assessed to determine whether the paper appeared to document novel research results and were not reviews, software or other forms of reference to other studies.

We invited in all 20 researchers, with 12 accepting our invitation to participate in the study. In all cases, interviews were conducted with the first authors of the papers to be discussed. All interviews but one were conducted over Skype and lasted between 45-60 minutes. The interviewees (10 men and 2 women) came from universities in seven different countries (Belgium 1, Germany 1, Italy 1, Netherlands 2, Sweden 2, UK 2, and USA 3). The cases fall within the following disciplines: theoretical chemistry and physics, neurosciences (2), chemistry (2), surgical oncology, life sciences, biology, astronomy, environmental biology, computer science, and material and mechanical engineering.

⁴ This choice was made in order to examine the most recent papers possible (at the time of case selection). However, this choice of three-year citation windows does have the drawback that we do not select any papers that are slow to be cited and only become highly cited after 5 or more years.

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As noted above, we recognize that high citation frequency cannot be directly equated with major scientific advances (Tijssen et al 2002) and that our approach potentially allows for some diversity in the scientific importance of the results documented in these papers. In addition, our approach results in interview subjects coming from a broad range of disciplines. Both this epistemic diversity and potential diversity in the importance of the advances have benefits and shortcomings. Our main reason is to include an explorative element in the analysis, which allows greater opportunity to capture potential diversity in research processes that lead to scientific advances. A key shortcoming is though that this epistemic diversity is difficult to fully handle in a limited number of interviews. This diversity is in terms of theoretical vs. empirical work, group size, use of equipment and infrastructure, the role of competition and funding availability, research time horizons and in opportunities for employment as a researcher in the private sector. Our approach is thus well suited as an explorative approach to capture the breadth of different forms of serendipity, but poorly suited to capture depth for individual disciplines.

In analyzing the narratives, we draw on Labov's model for narrative analysis (Labov 1972), as its method of temporally organizing narratives fits well with the purpose and content of the interview material. Labov's model has two main functions: a referential function that contextualizes the story by referencing events in sequential order, and an evaluative function that describes the storyteller's purpose in telling the story. Our focus is clearly on the former as we are interested in the sequencing of events and their context. We are not focused on the latter as our structuring of the interviews influences this and further, Labov's model assumes that the narrative is sequenced in the same way as the actual events. This, we acknowledge, is not necessarily the case for the narratives in our study, as we have allowed a certain degree of 'jumping around' temporally in the course of the interviews. Labov's model divides narratives into six sections. We have interpreted each of these sections in the context of our

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study. Box 2 shows the six sections and their specific content for the present analysis (original category names are shown in parentheses).

Box 2. Tales of serendipity in the context of Labov's model of natural narrative

Category	Narrative questions
TOPIC (ABSTRACT)	Short summary of what the main research result was and how/why it was important
CONTEXT (ORIENTATION)	History or background to the story Origin – when the story (or research process) begins Conditions at that time: institution, group, leader, funding, career situation
MOMENT OF DISCOVERY (COMPLICATION)	The “defining moment” of the research result How did it happen, element/degree of planning (unexpected, planned, etc.) Other <i>transitions</i> and <i>drivers</i>
ACHIEVING THE RESULT (EVALUATION)	Achieving the result Efforts to get it published, competition with other groups Attitude and ambitions of the researcher throughout this process
TURNING POINTS (RESOLUTION)	Critical or decisive factors (the result or its subsequent success would not have been possible without...)
IMPACT (CODA)	What happens afterwards? Career impacts; impact on choice of research path, area; funding

In the narrative analysis, each interview was manually analyzed and coded according to Labov's model and rewritten according to his six-category structure. Thereafter these narratives were compared to identify common patterns and significant differences. The narrative analysis is also informed by systematic coding according to key themes that lent themselves to coding. These include the moment of discovery and key turning points in the story. Thematic coding follows a three-step procedure. First, a predefined list was used as a starting point in the mapping of key themes. Themes were then related to theoretical concepts and conjectures concerning researcher behavior and environmental and organizational factors. Finally, the coding and grouping from the first two steps were used to develop analytical themes concerning key factors: the role of specific factors or different classifications of serendipity in the research processes.

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By focusing on narratives, we direct the reader's attention to the author's construction of how their research came to fruition. A narrative, in Donald Polkinghorne's (1988) words, "exhibits an explanation rather than demonstrating it (p.21)". The point in this article is not to prove demonstrably that the story told by the lead author is true, though we do believe that those stories we present have narrative rationality (Fisher 1987); rather we are interested in the way researchers emplot and explain the episodes that have led to their highly cited research. Narrative rationality differs from the sort of formal logic used in typical social science to prove or verify an argument (Czarniawska 2004), but given that our purpose is to categorize and explore, we argue that it is appropriate. We believe the narratives of the scientists we interviewed demonstrate both narrative probability in terms of their coherence and integrity, and narrative fidelity in terms of their credibility in the sense that the narrative makes accurate assertions about social reality (Fisher 1987). Further, since, the narratives were reconstructed using the answers from different questions, they are not simply 'what the authors said', but are a reconfiguring of that story. When there was lack of clarity, the interviewers were able to follow up and triangulate with additional questions. While there is still the possibility that the information given to us in the interviews is false, even so, it has provided us with the data we need to construct a new typology, which can be tested in future research.

Narrative analysis

This section presents the narrative analysis of the interviews. We first examine each element of the narrative and then discuss different types of stories that emerge from the analysis.

Topic

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“Topic” involves a short summary of the main research result and how or why it was important. In order to maintain the anonymity of the researchers interviewed, we do not provide a description of the results, and here only discuss researchers’ own views on the importance of their result.

The first question that arises when interviewing the first authors, and even for some when they received the interview invitation, is whether these papers are considered important advances by the researchers themselves. It was apparent that the researchers appear to have different perceptions of how important their own paper was. For most of the interviewed researchers, it was clear that making a major new discovery or an important advance to existing work in their field was extremely important to them and guided their work. We will see examples below where the researchers are discussing key moments of discovery.

However, this was not the case for all researchers. One researcher did not consider the paper to be truly novel work, in particular compared to the person’s other projects which had been far more challenging intellectually. However, this person recognized that the paper ended up having a large impact. Another researcher did not seem to be fully aware of the potential novelty and impact of their research at the time of discovery, nor was the person’s advisor.

A second question is whether in fact the first author was the main person behind the research in the paper. The interviews confirmed that that was the case for all 12 first authors.

Context

There is large diversity among the 12 interviewed researchers in terms of experience within their field, ranging from being an acknowledged leader with their field to still being a student. In two cases, the persons had just recently finished their master’s and begun their PhD at the time of the advance (which in part built on work done in their master’s thesis). Interestingly, a number of the researchers had just recently changed specific areas prior to

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the achieving the results behind their article. For example, one person had just started a PhD in a different area after having done research for many years. Another had just finished a PhD in one area, but was interested in pursuing research in a slightly different area, while another with a postdoc position had just switched areas to work in a different research group. For these researchers, their knowledge and approaches from related areas appeared to provide them with a novel dimension in which to approach research problems. Others had strong knowledge and expertise within their area, which proved to be crucial to their result. In total, the 12 researchers include (at time of research result) 4 PhD students, 6 postdocs and 2 senior level researchers.

Hence, for the most part, our interviews are with junior researchers, so their autonomy to pursue own ideas were determined to a large degree by group leaders, and they were generally not directly responsible for securing their own funding (though some had secured individual stipendiums). This clearly restricts our view of the role of funding conditions, allowing mainly an indirect view via the junior researcher.

Two things characterized the working conditions for the majority of the interviewed researchers: an open and supportive leader that gave the researcher broad freedom to pursue the research questions that truly interested them; and ample funding.

While these researchers knew that funding issues would be looming in the horizon, researchers felt shielded from funding considerations, generally with freedom in their choice of research to pursue, and where their research was not dictated by a criterion of whether it would be 'fundable' or not.

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"I... Let's see... (long pause)... Yeah, I don't know. At the time I was blissfully unaware of money, so. I just needed, I was paid for it as long as I didn't spend too much money everything would be okay." (Chemistry2)

Freedom was also linked to the relationship to the group leader, and also to general conditions that allowed the flexibility needed to change course in one's research plans.

"First one was published in 2006 and last one published in 2011. So it is a long time period. So at the beginning, because we had the xxx project, we focus on that. Also because we had that xxx compound. So, we have to build up a trust. So that takes time... At the beginning we have some freedom, but later we have more and more freedom. I think that is normal in science" (Chemistry1)

"The reason we were able to do this study was that the research unit we were a part of has a really unique funding model... if you are a principal investigator in the building then you don't have to pay for scanning... you [only] have to bid for time on the scanner." (Neuroscience2)

In one case, the researcher clearly prioritized the freedom of doing what s/he wanted to do over funding by pulling out of the second round in a postdoc grant competition in order to submit an alternative (and uncertain) application for a project that better interested them.

"So I think it was a bit of a high risk maneuver, looking back. They were surprised that I was asking to pull out and submit all over again when I might not have got through that first round, but my mind was made up. I was like, 'there's no point in me getting this grant anymore because it's not on what I wanted.' So in hindsight I think it was the right decision and I was lucky enough to be able to then get a second one." (Neuroscience2)

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Another aspect that characterized several cases was multidisciplinary or diversity among the group of researchers involved in the result. For one researcher, all other members of the research group had a completely different disciplinary background, which provided the researchers with fresh new perspectives on many problems. This multidisciplinary was present in a number of cases. In other cases there was important diversity in competences within the same or related fields. Examples are people with specific technical knowhow that provided essential inputs to making the discoveries.

In a number of cases, the advance addressed a problem that had been known for many years. For these, there were different degrees of uncertainty concerning both method and the desired end-result. For some, recent advances and a general maturing of the area increased chances of developing a major advance. This fueled increased efforts to find a solution which in these cases were successful.

“Around the year mid-90s to the 2000s it was already there. Some people recognized that there were some problems and people started to develop corrections for it. [...] It was driven by a failure by the theory and the growing power of computers to do larger and larger systems (which made the problem more critical). [...] So it is driven by technical issues. [...] I saw that there is a problem and I wanted to solve it. This is a very personal decision, very intuitive. Others ignored it for many years and now they are kind of angry of themselves. Why they didn't consider it earlier.” (Theoretical Chemistry)

In other cases, this was further helped along by an important advance in this or a related area that suddenly presented new opportunities, triggering driven efforts to achieve the result. In yet others, a very novel, exploratory approach was pursued that was uncertain and without a clear idea of what could be found.

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Also linked to this freedom was a lack of pressures for the delivery of immediate results. As we will see below, this picture changes markedly in the moment that a discovery is made.

Moment of discovery

We would like to think that there is a dramatic moment of discovery to all these stories, a ‘eureka’ moment where the researcher realizes that they have made a major discovery.

Realistically, we did not expect that all or even some of these cases involved a key moment of discovery, though a few of these scientists reported narratives that fit this idealized picture strikingly well.

In some cases, the advance was not really acknowledged until afterwards. This was for example the case for the two PhD students who, while possessing valuable knowledge on how to make improvements, did not have full awareness of what the implications of these improvements would be or how substantive the contribution would be for the area as a whole.

“The work that I described in the articles is based on experimental research.... We produced different samples with different process settings on the machine.

(Interviewer) And was it hard work? Or did it go very smoothly?

Well, it's difficult to say that because it was my master thesis project, so it was the first time that I had to do such a project on my own, so it's difficult to compare. But I think that experiments which I've used are very basic and well known techniques within the xxx science world. In that way it was not really hard work, it was just... It had to be worked on. ... The new thing was just linking how the process works to the xxx which we saw and to combine the theories from xxx science to explain it.”

(Materials engineering)

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In three other cases, there did not seem to be any significant degree of uncertainty in the work. While the researchers in question appear to possess unique abilities to produce these advances, they were not unsure of the outcome over the course of the research process.

“So this paper was not risky. [...] There's nothing, I guess there's a little bit unknown about it in that we had never used this technique to measure xxx before. But we did know that as soon as we could make those xxx measurements, we had a publishable result.” (Biology)

In the remaining cases, there was a moment of discovery, in some cases after long hard work to pursue the goal in question and in other cases due to some event that suddenly made everything fall into place.

In one case, a new related advance suddenly offered a new and promising approach for solving a long-standing problem. This ‘event’ triggered the idea for the new research that quite rapidly reached a point where the researcher was convinced that a breakthrough would be achieved (as it was).

Two cases are very fitting examples of serendipity, where the pursuit of a side area, a partnership with someone with common interests but very different capabilities, and a random event accidentally triggered the moment of discovery.

In one case, the two researchers had agreed to pursue a common interest and what appears to have been the key here was the right (and unique) combination of capabilities between the two. In the other case, the pair of researchers discovered the missing element to their work at a conference, which fully convinced both of them that they would succeed in achieving the breakthrough.

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The first xxx (test), and I still view it to some extent that was some of the most emotional moments I ever had, besides my own children and my wedding. It's enormously and I still feel it, and together the team too. Everybody was very silenced and it was amazing what we saw [...] It was one of the most unique moments, also because it takes so much effort to go all the way, knowing that the concept works is still I regard one of the best moments, scientifically, professionally in my life." (Surgical Oncology)

As we mentioned above, we have not structured the interviews in a way that we were able to focus on or capture differences in how the respondents tell their story and what they emphasize. However, it is interesting and also telling how important this moment of discovery was for many of the researchers. Many of them have directly stated that the moment ranked as one of the best in their professional lives. What is also interesting is that for the cases where this moment did not take place, the researchers did not value the result nearly as highly. These other cases all appear to have had substantial impact – and as a result, researchers gradually increased their appreciation of them – but they did not involve a novel scientific discovery of something they felt was extremely new or risky. Rather, in these cases there was a quite obvious gap in the field that they filled with their paper. Based on these interviews, this difference would appear to be extremely important in terms of academic motivations to pursue new research discoveries.

Achieving the scientific result

Here we look more specifically at the process of getting the result published and disseminated within the scientific community. Just as it often (but as we have seen, not always) takes great effort to achieve an advance, success is in many cases equally dependent on efforts both to document the results in a top journal and gather acceptance of the results

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following publication. Competition typically presented itself at the latter stage of the process, where the discovery had been made and it was now a question of finalizing work and writing a convincing paper presenting the results. A number of the cases stressed the importance of marketing the results in the scientific community to create greater visibility encourage further use and application of the results. One researcher also notes the costs of not having made a great effort to promote their paper and results. The authors of a competing paper which was published at the same time have been much more active in marketing their results and – despite arguably being a weaker paper in certain respects – have achieved greater recognition both for themselves and for their paper.

Turning points

Moments of discovery constitute key transition points in the research processes behind advances. A number of other events (turning points), both internal and external, may influence the research process and decision making by the researcher. The first is set-backs. Many, though clearly not all, of the advances are also the results of persistence in the face of negative results, particularly failed research experiments, but also paper rejections and skepticism concerning results. For these, persistence and drive to achieve advances despite set-backs is a key ingredient in many of these advances. However, there are also cases where the process was very rapid and without any major hitches, following a random event that facilitated the discovery.

The second is collaborations, which typically took place at the beginning of the process. In a very large number of the cases, the meeting of different capabilities, be they in terms of technical skills or disciplinary background, was a major catalyst to the process of reaching the result. A third transition in a number of cases is the sudden onset of competition from

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other groups. In these instances, the research group enters into a period of frenzied activity where all time and resources are devoted to being the first to publish in a specific top journal.

Hence, while there was little indication of a rushed or rapid pace of research prior to discovery, this picture changes for essentially all cases that experience some major turning point. This was typically driven by competition with other groups and a race for priority.

“Actually, of course, yes, because what I thought is that if I could find the xxx even then, of course we are not stupid... so and of course the other reason why I almost immediately involved other people is of course, you need to do it in a hurry.” (Life Sciences)

However, in other cases, this rapid pace may simply be driven by the fact that they were on to an exciting result.

“Yeah. We knew, although we really sped up, and we did speed up quite significantly, we also knew, and that was because you know your competitors. There was no one in the world able to do this. So, that helped quite a bit.” (Surgical oncology)

In one case, the researcher knew that they had an interesting result but were unsure about what it meant or how it should be interpreted. And given that they did not know what the end of the story would be, the moment of discovery was not fully registered. It was only after pressures from a competing group and strong interest from a top journal that they became fully aware of the fact that the results they already had were a major discovery in itself.

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“I think we had first data around 2007. And then we didn’t really know what to do. We had this key result that something goes on but we didn’t know how to approach it. [...] and now when I think back I think xxx, you know, they have this paper, and we would never had sent our paper to xxx. And we also would not have a manuscript because we had no idea what was going on. But xxx sending in the paper forced us to send our manuscript to xxx and it was sent out for review. [...] Actually one of the persons who had been very important for the field, he told us when our paper was published that this was the most important thing that has happened to the field during the last 20 years.” (Neurosciences1)

Impact – what happens afterwards

While our sample varies greatly in terms of experience prior to the result behind their paper (in effect only one out of the 12 interviewed first authors was truly established within their field and led their own group, one other was in a tenured position, while the remainder were either PhDs or postdocs), a large number of researchers note that the paper has had a positive impact on their careers in one way or another, and a number of the researchers now lead their own group 5-6 years after the result (including the researcher above that did not strongly market their research results). However, there is a clear difference here in terms of how passionate the researchers were about the advance. In the cases where both the discovery itself and the research surrounding it were very dear to them, the advance has opened doors to continue pursuing research in this area. In other cases, the advance is seen simply as a good paper with strong impact in an area that the researcher had never really had any intention of pursuing further. Finally, we also have two cases where the researcher left academia shortly after the advance, either fully or partially, with a shared university-industry position. While these two are interested in conducting research, and still do so on the

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industry side, they were not interested in the academic ‘game’, with focus on publication in top journals and citation impact and the need to become a group leader. Their reasons for leaving thus appear to be related to pressures of today’s academia, though they were not forced to leave: in both cases a postdoc-position was available. It was the long-term career perspective that they did not like.

Types of tales

The narrative analysis above broke down the narratives into key elements for comparison and analysis. By restructuring the narratives into Labov’s categories, we saw that while many aspects of these highly cited papers came from a planned research process, many of them also included serendipity that entered the discovery process at different points. In this final empirical section, we suggest a typology of discovery using several of Labov’s categories as a rough framework to distinguish how and when serendipity appears. We illustrate it with brief accounts using four of the 12 stories.

The first type is what we would call ‘purposeful advances’. In these cases, there is no serendipity, the research is well-planned and while there may be great uncertainty, a target or end-outcome for the researcher has been chosen. The process does not appear to have been influenced by ‘lucky breaks’ or completely unanticipated factors. The second we call ‘serendipitous contexts’, which relates to Labov’s orientation stage. The unanticipated aspect of these discoveries come at the outset, where a unique set of factors (people, funding, etc.) happen to come together. The third we call ‘serendipitous turning points’. The goal or challenge might be well-known or not yet established, but it is somewhere in the midst of the research process, at an impasse or what Labov calls a “complicating action” that some completely unanticipated event suddenly makes the advance possible. We distinguish

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between actor-based (people) and process-based (methodological) variants of this type. These correspond well with Roberts' (1989) "pseudo-serendipity" and with Yaqub (2018)'s "Mertonian serendipity". The final type we call 'serendipitous resolution', where research results in an unanticipated discovery. This represents the classical type of serendipity in which researchers are looking for one thing and by chance stumbling upon something else. In our research we did not find this type, so we do not provide an example.

Purposeful advances

A key driving force behind X's research was to find solutions to problems with great societal importance, so his ambitions in doing research went beyond making important advances within his field. X joined a transdisciplinary group as a postdoc, shortly after finishing his PhD. All others in the group of around 10-15 researchers had different disciplinary backgrounds than X. As a side project (and with the strong support of his group leader), X set out to find a solution to a key challenge within X's field. X approached the problem by first amassing a comprehensive knowledge of all existing work within the field (which took around two years) and thereafter experimented with new approaches that had not yet been attempted. So, the key elements here were X's own talent, an exhaustive knowledge of the field, persistence and ambition, and the support of the group leader to do this independent work. X ended up finding and developing a completely new method, so at some point a new discovery was made based on uncertain, explorative work. However, it was based on very carefully planned, hard and persistent work.

Serendipitous contexts

The discovery came about at the end of the researcher's PhD work, though the discovery was not a central part of his PhD research. Instead it was a side project on a topic of long-term

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interest that Y managed to squeeze in. There were several key people which enabled the research to happen: first, Y's PhD advisor for allowing him to pursue this side project. *"So I was working on that and it was mainly just for my own interest. It was something I had running in parallel, but increasingly it made me realize that this was really what I wanted to study."* This flexible leadership exhibited by Y's supervisor was complemented by finding the right research partner, a xxx who was also on the cusp of finishing her PhD in a different but related field, and with a bit of extra time on her hands. She was planning to use that time, and remaining funding, for one last experiment. *"I remember, we were having lunch in the summer and we were sitting outside in the park, and she said to me that she wanted to do this project on xxx but didn't have any idea what to relate it to [...] and so I immediately jumped on that, I said, 'Wouldn't it be cool to look at these xxx."* Still, even with their advisor's support they needed to obtain time on the equipment to conduct the actual study, but in this case within weeks their project had been approved within their internal research unit and they were able to begin testing.

Once the data was collected and the code written, the process of running the statistics was very fast.

"The bottom line is, that if you hit the run on these statistics and you see an empty xxx, then you've got nothing. And that's kind of what we were expecting... but we saw a really prominent xxx that was related to [our topic]. When we saw that, we did kind of high-five and get really excited. I think people in the room were saying: 'What the hell was going on?'" The excitement was palpable: *"In projects I have done since then that may have been more intellectually satisfying in terms of working out the mechanisms, it has not really matched that level of excitement because that was really just going from nothing to everything."* (Neurosciences2)

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Serendipitous turning points - Unplanned meetings

The researcher became acquainted with xxx technologies during an extended stay in the US. Z realized that this technology could be applied in very innovative ways within Z's field. Z sought contact with a person with expertise related to the technology about its possible application.

They came together at the first worldwide xxx conference in Boston, and after the conference both were convinced that they would be able to produce a functional prototype for testing, and they were further convinced that this testing would produce positive results. There was a moment of extreme 'serendipity' when they became aware of the huge potential of their result at a conference in Nice in 2006. This is where they learned that a group in the US would be able to provide a key element that would make application of the new technology possible.

Both the events when they realized that achieving the important advance was feasible and when they were able to confirm that it worked were seen as powerful moments in the researcher's professional career.

The study paved the way for the introduction of their new technique in practice, with substantial improvements compared to existing techniques.

Serendipitous turning points - Methodologies

Ever since the 1960s different research groups had tried to find the xxx, but to no avail. A competing group had published a paper in *Nature* in 2010, describing a related process of xxx, using a methodology that was quite innovative. G was struck by the method, and immediately started to experiment with it. In a short period of time G reached a key point, realizing that he was very close to making a major discovery and that the discovery was

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within reach. G abandoned all other projects and focused all efforts on making the discovery, and succeeded.

After submitting his paper to a top journal around Christmas 2010, G already anticipated that the reviews would take a while, and that they would be fierce (because of the importance of the result). G and colleagues used the two-month window before the reviews would be back to keep working on the findings, and on building a stronger case. Among other things, G asked two experts with a different background to collaborate on the paper, as their competences were relevant to confirming the finding. This turned out to be a very important decision, as one of the reviewers focused mainly on this part of the work. After the reviews came back, the editor didn't immediately agree to accept the paper, probably because the competing group was also in the race. They were given a short period to revise and resubmit the paper, which was subsequently accepted.

Discussion

This paper has explored the tales of 12 highly cited papers across a wide range of disciplines, how the first authors of these papers themselves view the process behind the paper and whether their stories contain elements of serendipity. In all cases the first authors were central to the research process in terms of forming the idea, making the discovery and achieving the main result. In most cases, however, these first authors were early career researchers working under the direction of a group leader. This result was not fully unexpected and is in line with studies that show that early career researchers tend to be more innovative (Callaway 2015). This would be something to test more explicitly in further studies, that is, do younger researchers experience more serendipity, and if so, what reasons lie behind this? Do more experienced researchers get too 'disciplined', too conservative in their approach, to experience serendipity? A related issue is the career effects of researchers

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that produce serendipitous research; i.e. does this create a platform for further success?

Career outcomes were addressed briefly in the interviews, which found that many (but not all) went on to start their own research groups. This is an interesting issue that could be pursued in greater detail in subsequent research.

Other studies have identified a number of elements that appear to be common to a large share of the cases we examined. This includes the freedom to pursue own research interests and also to change course if new results are found. In most cases, this freedom was thanks to a group leader that helped create these conditions and encourage exploratory thinking. This suggests that these leaders are effective in navigating research funding without surrendering research freedom. At least one of the early career researchers behind the advances remarked that this freedom was difficult to sustain once they had established their own group.

At the same time, the interviews demonstrate more than anything the large diversity in how the processes behind discoveries take place. Key dimensions here are the degree of planning involved in research processes and objectives, and the role of serendipity at different stages in the research process. A large number of cases involved some form of new impulse or perspective that made the discovery possible. The background for these results varied considerably, from planned research to achieve long-term goals to fully explorative research that did not have a clear objective prior to discovery. New models and typologies of serendipity should account for the temporal dimension, examining how unexpectedness at different points in the research process can be analyzed.

Our cases reflect many of the characteristics highlighted in Yaqub (2018)'s typology. Most common among our examples is "Mertonian" serendipity, where research is planned and

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leads toward the anticipated objective, but was made possible by an unexpected, serendipitous event during the research process. However, our interviews suggest that there are multiple types of Mertonian serendipity that can be distinguished based on when and how they occur at different points in the research process. Arguably, we do not find any clear examples of the classic (Walpolian) type of serendipity, where carefully planned work lead to the discovery of something completely different, but this can be qualified somewhat, as a number of the cases can be seen as achieving their overall objective, but in unexpected ways and along very different paths than originally planned. It must also be noted, that we did not find any examples of ‘Stephanian serendipity’ among our cases. This was also not to be expected, since we examined recent examples of highly cited papers and conducted the interviews approximately five years after the date of publication. This did not allow much time for ‘Stephanian’ discoveries to happen.

Overall, while we find the existing typologies useful, we show that the distinctions they create can often be very fuzzy in practice. An example is the distinction between targeted and untargeted research. Untargeted research can be described as ranging from having no objective in mind to exploratory work with a goal, but with great uncertainty as to what form it will take. Our cases show a variety of ways in which explorative research sets off a key discovery or realization that initiates research, leading to an important result or outcome. Likewise, the distinction between solving the given problem and a different problem (Yaquub 2018), has a temporal complication. At some point the different problem can become the given problem, as the researchers recognize the serendipitous moment and adjust their focus towards a revised goal. Here again, new models for serendipity should further refine and unpack the concept of targeting and planning, and the effects that serendipity has on the processes and outcomes.

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As noted above, our study had a dual approach, both on the stories behind the advances and the sequence of events that took place, and a number of cross-cutting themes and how they influence highly cited research. Our assessment is that such a dual approach is likely impractical on a larger scale. More comprehensive studies should either single focus on the narrative of the research process, or on a cross-cutting theme such as the role of funding conditions.

Reflecting for a moment on methods, our impression from this study is that narrative analysis works well as a tool to analyze serendipity and unplanned elements of research processes as it allows us to examine different events at different stages of the research process. Most importantly, it seems that serendipity is far more common than generally acknowledged, particularly in work that goes on to be highly-cited. Furthermore, in all cases, the first authors were very much able to recount the story behind their research result, both the events themselves and the background for how and why they occurred. And, our approach was able to capture a substantial amount of variation in terms of serendipity (including cases that did not involve serendipity), which was helpful in identifying different aspects of unplanned events. A more comprehensive study could both examine a larger number of cases and potentially also seek validation of narratives through interviews with an additional co-author of each examined paper.

Cross-cutting themes are also interesting to examine through a narrative approach, but essentially require a different strategy in terms of data collection. An important example here is the role of funding in fostering or creating space for serendipity. In-depth analysis of these types of themes requires a broader approach, likely involving interviews with a

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minimum first authors and group leaders, and also further information on funding conditions for the group as a whole. Understanding the epistemic conditions under which different types of serendipity occur (and even more importantly allow it to be recognized and exploited by researchers) should prove important for fostering high impact research. In this regard, we think that the freedom to change directions and even to fail, are important themes to explore, as are better understanding how chance encounters with different people and methods can be capitalized on. How many chances at serendipity are missed or overlooked? Is there a way to better harness them when they are recognized?

There are also policy implications that emanate from the research. Recent policy innovations promoted under the terms ‘grand challenges’, ‘mission-oriented research’, ‘smart specialization’ among others, imply that research would benefit from a more planned, strategic process that incorporates more targeted outcomes (Young et al. 2017). These attempts to efficiently allocate and prevent waste in funding resources are understandable as political aims; however, if our findings are generalizable these types of policy may have the perverse effect of preventing high impact results. It appears that there needs to be space for serendipity within research processes. The question, which requires further study, is how to define and foster that space? A first principle, coming from our explorations, might be that science policy should not lock researchers into either processes or goals. It follows that the evaluation of results, particularly project results, might need a standard other than the aims set out in the original project proposal. A revised understanding of waste is perhaps also needed, one that can be grounded in the economic concept of opportunity costs. What is the cost of not having the flexibility to pursue serendipity?

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Overall, what emerges from this study is the need for a more complex understanding of serendipity that is temporally nuanced. While previous studies have given us useful typologies that distinguish types of serendipity by zeroing in on the various gaps between the research target and result, our research shows that to fully understand this phenomenon we have also to look beyond just the beginning and ending points. It is what happens along the way at various stages in the process that matter if we hope to eventually harness the power of serendipity.

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