SCIENTIFIC PROGRESS – INDIVIDUAL & COLLECTIVE

24 – 26 AUGUST 2022

Seminar Program

Abraham Kuyper Center

Schedule

Wednesday, 24th August

Time		Theme	Room
08:30	**Arrival**		
09:00	**Welcome & introduction**		Agora 1
09:15	**Darrell Rowbottom** Is scientific progress just what you'd like?	Theme 1	Agora 1
10:25	Coffee break		
10:40	**Felipe Romero** The conceptual origins of metascience: Fashion, revolution, or spin-off?	Theme 1	Agora 1
11:50	**James Evans** The social, biographical and demographical locus of innovation	Theme 1	Agora 1
13:00	Lunch break		
14:00	**Insa Lawler** Scientific progress and idealization	Theme 1	Agora 1
15:10	**Aydin Mohseni** Intervention and backfire in the replication crisis	Theme 2	Agora 1
16:20	Coffee break		
16:35	**Plenary discussion**		Agora 1
17:20	- End of Day 1 -		
18:20	**Social event & dinner**		

Thursday, 25th August

Time		Theme	Room
09:00	**Introduction**		Agora 1
09:15	**Hannah Rubin** Structural causes of citation gaps	Theme 2	Agora 1
10:25	Coffee break		
10:40	**Arnout van de Rijt** The “Proposal Effect” in science funding	Theme 2/3	Agora 1
11:50	**Hanne Anderson** Collective Knowledge production – What does granularity mean?	Theme 2	Agora 1
13:00	Lunch break		
14:00	**Daniël Lakens** Which research is valuable enough to do well?	Theme 3	Agora 1
15:10	**Mike Thicke** Modes of collective knowledge production in science	Theme 2/3	Agora 1
16:20	**Poster & lightning talk session** (coffee served)		Agora 5
17:40	**Plenary discussion**		Agora 1
18:25	- End of Day 2 -		

Friday, 26th August

Time		Theme	Room
09:00	**Introduction**		Agora 3
09:15	**Leo Tiokhin** Science as an evolutionary process	Theme 3	Agora 3
10:25	**Roberta Sinatra** Quantifying the biases of scientific impact	Theme 2	Agora 3
11:35	Coffee break		
11:50	**Duygu Uygun-Tunç** Collective vice in science: Lessons from the credibility crisis	Theme 3	Agora 3
13:00	**Plenary discussion & closing**		Agora 3
13:30	Lunch		

- End of the seminar -

Talk abstracts

Darrell Rowbottom

Is scientific progress just what you'd like?
The literature on (cognitive) scientific progress in the philosophy of science is dominated by competing theories, involving general claims that progress is constituted, wholly or centrally, by increases in knowledge, or verisimilitude, or understanding, or problem-solving capacity, or some appropriate surrogate. (Invent a new theory, and get a new paper published.) But underlying these theories is typically an unspoken, and invariably an undefended, presumption: that there is an objective, temporally invariant, 'aim of science'. The fundamental idea is that when this aim of science is furthered, then progress occurs. So the relevant authors typically disagree, ultimately, on what said aim is.
However, no philosopher has yet succeeded in making sense of the notion of an aim of science; and the most promising accounts thereof suggest that sociological (rather than philosophical) study should be used to determine such an aim *and* that it would only be temporally invariant as a matter of contingency.
Given also the widespread disagreement about what constitutes scientific progress, moreover, we should be open to considering an alternative view which has, rather remarkably, been neglected; I call it 'cognitive scientific progress anti-realism'. This view, which is analogous to moral anti-realism, is that the values involved in assessing whether progress has occurred in science are fundamentally subjective (and perhaps by extension intersubjective) in character. Cognitive progress is what you (or we) make it. Or so I shall argue.

Felipe Romero

The conceptual origins of metascience: fashion, revolution, or spin-off?
The term 'metascience' precedes the replication crisis. However, only now is metascience becoming institutionalized. This institutionalization and its perils require philosophical attention. How did metascience emerge? Where does the novelty of metascience lie? How does metascience relate to other fields that take science as their subject matter? In this talk, I explore different models of discipline creation and change, and seek to understand whether they can make sense of the emergence of metascience. After examining these models, I suggest that we should question the increasingly popular perception of metascience as a fully authoritative field.

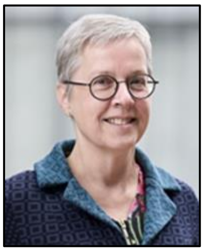
James Evans

The Social, Biographical and Demographical Locus of Innovation
In this talk, I explore the social, biographical and demographical locus of innovation in science, scholarship and technological development. With teams growing in all areas of scientific and scholarly research, I first explore the relationship between team structure and the character of innovation they produce. Drawing on 89,575 self-reports of team member research activity

underlying scientific publications, I show how individual activities cohere into broad roles of (1) leadership through the direction and presentation of research and (2) support through data collection, analysis and discussion. The hidden hierarchy of a scientific team is characterized by its lead (or L)-ratio of members playing leadership roles to total team size. The L-ratio is validated through correlation with imputed contributions to the specific paper and to science as a whole, which I use to effectively extrapolate the L-ratio for 16,397,750 papers where roles are not explicit. I show that relative to flat, egalitarian teams, tall, hierarchical teams produce less novelty and more often develop existing ideas; increase productivity for those on top and decrease it for those beneath; increase short-term citations but decrease long-term influence. These effects hold within-person — the same person on the same-sized team produces science much more likely to disruptively innovate if they work on a flat, high L-ratio team. These results suggest the critical role flat teams play for sustainable scientific advance and the training and advancement of scientists–tall teams and older scientists have more influence (and receive more credit) than ever before.
I then explore how with rising life expectancies around the world and an older scientific workforce than ever before, what aging means for individual scientists and what aging scientists mean for scientific and technological progress? Prior research focuses on star scientists, their changing dates and rates of breakthrough success across history. I examine how all scientists and scholars age in terms of how their stream of ideas and contributions relate to the evolving frontier of knowledge, and how demographically aging fields relate to field-level progress. Analyzing more than 244 million scholars across 241 million articles across the last two centuries, here I show that for all fields, periods, and impact levels, scientists' research ideas and references age linearly over time, their research is less likely to disrupt the state of science and more likely to criticize emerging work. Early success accelerates scientist aging; while changing institutions and fields and collaborating with young scientists slows it. These patterns aggregate within fields such that those with a higher proportion of older scientists experience a lower churn of ideas and more rapid individual aging, suggesting a universal link between aging, activity, and advance. The work demonstrates how tracking the demography of scientists can forecast areas of growth and maturity, and suggests how managing it through policy could help modulate science between crystallization and chaos.

Insa Lawler

Scientific progress and idealization
Intuitively, science progresses from truth to truth. A glance at history quickly reveals that this idea is mistaken. We often learn from scientific theories that turned out to be false. This paper focuses on a different challenge: Idealizations are deliberately and ubiquitously used in science. Scientists thus work with assumptions that are known to be false. Any account of scientific progress needs to account for this widely accepted scientific practice. I examine how the four dominant accounts—the problem-solving account, the truthlikeness account, the epistemic account, and the noetic account—can cope with the challenge from idealization, with an eye on indispensable idealizations. One upshot is that, on all accounts, idealizations can promote progress. Only some accounts allow them to constitute progress.



Hanne Andersen

Collective Knowledge production – What does granularity mean?
Science has developed immensely over the last century. For example, in addressing a wider and wider range of topics, and requiring more and more resources, scientific research has become increasingly collaborative and interdisciplinary. In this talk, I shall analyze some of the epistemic implications of this development.
I shall focus on how groups of different kinds and different sizes can provide different conditions for collective knowledge production, and I shall discuss how philosophical analyses of knowledge production in groups can provide input for ongoing debates in science policy on research organization, research funding, and strategic research planning.

Hannah Rubin

Structural Causes of Citation Gaps
The social identity of a researcher can affect their position in a community, as well as the uptake of their ideas. In many fields, members of underrepresented or minority groups are less likely to be cited, leading to citation gaps. Though this empirical phenomenon has been well-studied, empirical work generally does not provide insight into the causes of citation gaps. I will argue, using mathematical models, that citation gaps are likely due in part to the structure of academic communities. The existence of these 'structural causes' has implications for attempts to lessen citation gaps and for proposals to make academic communities more efficient (e.g. by eliminating pre-publication peer review). These proposals have the potential to create feedback loops, amplifying current structural inequities.

Arnout van de Rijt

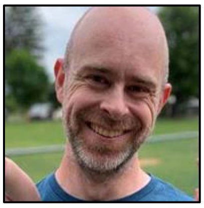
The "Proposal Effect" in Science Funding
Grants tend to go to people who won grants before. This may be (1) because of a Matthew effect in science funding or (2) because some are better scientists than others (or both). In either case, the implication is that if in a funding competition panelists did not actually read the full proposal texts, they would end up selecting a not-too-different set of winners. We report on a field experiment conducted by The Dutch Research Council (NWO) in collaboration with the authors in an early-career competition for awards of 800,000 euros. At the preselection stage of the competition, proposals were evaluated by the regular panel as well as by a shadow panel. A random half of shadow panelists were shown a CV and only a one-paragraph summary of the proposed research, while the other half were shown a CV and a full proposal. We find that two shadow panelists did not agree less on the merit of an application when one did not have access to the full proposal text than when both had access. These results suggests that lengthy proposal texts are mostly redundant and of little consequence in the selection of applications, at least at the preselection stage of the grant review process, where most applications get discarded.

Aydin Mohseni

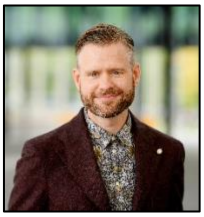
Intervention and Backfire in the Replication Crisis
How should we assess proposals for changes in the norms of scientific practice? I present a modeling framework that addresses essential aspects of this question and I employ the framework to illustrate unobvious implications for standard, proposed methodological interventions in the replication crisis. In particular, I show how interventions intended to lower false discovery rates can exhibit diminished efficacy and even backfire under imminently plausible conditions. While such results should be combined with empirical data to make reliable inferences, I argue that these should already demonstrate the promise of the proposed framework in elucidating unrecognized facets of the logic of intervention.

Roberta Sinatra

Quantifying the biases of scientific impact
Every day our life is made easier by efficient measures and algorithms that search and rank scientific information. Yet, these measures and algorithms have an issue: they are trained on citations, which are ingrained with human biases. Therefore the output is inherently biased too, creating inequalities, raising concerns of discrimination, even harming economic growth. In this talk, I will present research focusing on quantifying bias in publication data. The overarching goal of this research is to uncover the bias mechanisms that, given the same quality, drive different citation trajectories, and use them (1) to create fair measures and algorithms, (2) to improve our understanding of the scientific enterprise.



Mike Thicke
Modes of collective knowledge production in science
How should scientists be organized to best achieve collective epistemic progress? I consider three modes of organization: an entrepreneurial mode, a centralized mode, and a hybrid mode where a centralized body both sets the research agenda and synthesizes research outputs. Entrepreneurial science—where scientists have the freedom to choose their own agenda and where collective knowledge is an emergent property of the publication process—is the paradigmatic mode of research today. I argue that many pathologies of science can be attributed to this mode, which suggests that we ought to look for other ways of doing science collectively.



Daniël Lakens

Which research is valuable enough to do well?
In recent years scientists in many disciplines have had to raise the bar to improve the reliability of their findings. The increasing realization that fields should perform more replication studies, as well as design studies with larger samples, reduces the number of research questions we can reliably study, compared to our beliefs about this a decade ago. This raises questions concerning research prioritization. Which studies should fields perform, and how should they decide upon these studies? Are individual scientists able to make these choices, or does research prioritization require collective decision making, for example at consensus conferences? Would increasing coordination in the selection of research questions facilitate scientific progress, or hinder it? And how should we shape reward structures in academia to move towards collective agreements about research prioritization, in fields where it is deemed to be beneficial?

Leo Tiokhin

Science as an evolutionary process
Science involves generation of variation (ideas, methods), transmission of information within and between generations, and mechanisms for selection that favor some variants over others (journals, funding agencies, rules that scientists use to evaluate ideas). Science thus satisfies the 3 conditions necessary for the operation of evolution by means of natural selection: variation, heritability, differential reproductive success. In this talk, I will illustrate how an evolutionary perspective can shed light on key issues relevant to the question of how to reform science to improve collective scientific progress.

Duygu Uygun-Tunç

Collective vice in science: Lessons from the credibility crisis
In this talk I examine the role of epistemically virtuous scientists in the success (and failure) of science as a social institution characterized by predominantly epistemic ends; that is, in collective epistemic virtue (and vice). I analyze several structural explanations of the epistemic success of science which rule out virtue attributions to scientists vis-a-vis a case of collective epistemic vice; i.e., the credibility crisis in the social and behavioral sciences. The question as to the role of epistemic virtue in science is embedded in a broader debate on how the goals of epistemically virtuous communities and epistemically virtuous individuals are related.
On one extreme end of the individual-collective virtue debate there is the conservative or traditional position that individual epistemic virtue is necessary and sufficient for collective epistemic virtue. Let us call this position virtue absolutism. On the other extreme end there is the recently much more popular position that individual virtue is neither necessary nor sufficient for collective virtue, and even individual vice can possibly contribute to the success of science. Let us call this virtue reductionism.

This latter position has been defended by many in the philosophy of science with different arguments that aim to explain the success of science in terms of its social structure, most notably the arguments from the invisible hand and division of cognitive labor by Hull and Kitcher respectively. These arguments maintain that the success of science is primarily due to the incentive structures of its credit economy, thanks to which scientists can (and sometimes better) serve collective epistemic good by pursuing credit instead of genuinely epistemic aims such as truth or knowledge. The same conclusion is defended in the sociology of science in reference to scientific norms, most notably by Merton. The sociological argument maintains that scientists behave in a way that serves the institutional goals of science not because they are epistemically virtuous people but because they comply with the rules and norms enjoined by the scientific institution, which are thus the real cause behind its success.
While acknowledging (contra virtue-absolutism) that divergent motivations and behaviors might also serve the collective goals of science, I argue (contra virtue-reductionism) that the presence of a significant proportion of epistemically virtuous scientists in a scientific community is a necessary condition for it to manifest collective epistemic virtue. I analyze a set of major arguments for virtue-reductionism, and offer objections in reference to meta-scientific research and theorizing on the credibility crisis. Firstly, the same incentive structures correlate with collective epistemic vice as well as virtue, which begs for additional explanatory factors.
Secondly, scientists respond to incentives or sanctions differently depending on both individual and contextual factors. In some cases the most salient explanation of the heterogeneous behavior of scientists in relation to situational factors are features pertaining to their epistemic characters.
Thirdly, many equally important aspects of the success of science cannot be explained without reference to individual epistemic virtue. Scientific progress is multifaceted: besides productivity and impact we also value reliability and rigor, without which the former can be meaningless. However, there is no single social mechanism that can achieve these all at once, because there are trade-offs between values pertaining to the context of discovery and those pertaining to the context of justification. While successful individual contributions to discovery-oriented collective goals need not manifest epistemic virtue, successful contributions to justification-oriented goals do.
Lastly and most importantly, neither incentive structures nor institutional norms can be established, maintained, or reformed in a way that reliably serves collective epistemic ends of science without there being a significant proportion of epistemically virtuous scientists.

Scientific Progress and Robust Bayesian Confirmation

Joshua Luczak

This poster intends to argue that scientific progress occurs within a field whenever there is an increase in the average degrees of confidence (credence) among diverse members of the relevant scientific community. If we think that our scientific theories are confirmed whenever there is a boost in the credence of engaged scientists who begin with reasonable priors and who update their beliefs in light of evidence in accordance with Bayes theorem, and we think that confirmation is closely tied to a theory's truth, then it seems reasonable to think that a scientific field makes progress towards truth whenever there is an increase in the average degrees of confidence among diverse members of the scientific community. By tying progress to increases in the average degrees of confidence among diverse members of the scientific community, we wash out the effects of biases and idiosyncrasies of individual scientists, and so, in this way, obtain a robust result.

Cohen's error-rates and the body of knowledge in behavioral science

Aran Arslan and Frank Zenker

In 1965, Jacob Cohen had recommended that researchers in behavioral science adopt error-rates for false positive (α) and false negative errors that mirror what he called the 'general relative seriousness' of these errors. Cohen also argued that the consequences of an α-error are about four times as serious as those of a β-error. This ratio of $\alpha/\beta=1/4$ became a widely accepted convention for null hypothesis testing research in behavioral science. The reasonableness of $\alpha/\beta=1/4$ depends on seeking to preferentially minimize the negative consequences incurred if an individual researcher were to contribute a mistake to the body of scientific knowledge, and so depends on epistemic considerations. If, by contrast, one also considers the negative practical consequences of an action that is based on an erroneous hypothesis test-result, then the reasonableness of $\alpha/\beta=1/4$ becomes questionable. Already the typically expectable action in response to a negative test-result for a contagious disease shows decisively that failing to accept a true H1 hypothesis (β-error) can have practical consequences that are more serious than those of erroneously rejecting it. This holds for both individuals and social groups. Epistemic grounds can thus suffice to favor low α-error-rates over low β-error-rates if adding a falsehood to the body of scientific knowledge is less desirable than missing out on a truth, i.e., if a "mistaken discovery" is a less preferred scientific contribution than a "missed discovery." But Cohen's convention of $\alpha/\beta=1/4$ not only ignores that a β-error can have more serious practical consequences than an α-error. The replication crisis in behavioral science also shows that conventionally accepting $\alpha/\beta=1/4$ at the level of an individual researcher's contribution does—already before practical considerations are even considered—incur long run negative epistemic consequences at the collective level. This not only speaks strongly against Cohen's error-rates. It also makes their conventional acceptance crucial in explaining the current state of scientific knowledge in behavioral science.

Scientific Collaboration under the Shadow of a War: The Nuclear Fusion Community after the Russian Invasion of Ukraine

Dominika Czerniawska, Richelle Boone and Simcha Jong

This paper investigates potential changes in the global fusion community collaboration network, caused by the political reactions to the Russian invasion of Ukraine. The ties in the fusion community are old and robust. Built up in the 70s and strengthened during the collapse of the Soviet Union as well as many forthcoming political turbulences, fusion collaboration ties have been understood and used as a way to maintain and improve international relations. The current crisis raises the question of whether these ties are robust enough to survive the current political crisis between Russia and the West. Will the conflict change the fusion community and if so, will change be permanent? While all collaboration with Russia funded by the European Commission through EUROfusion has been suspended, up to now the worldwide fusion project ITER did not take similar steps. Russia was among the founding members of ITER, and the Russian fusion community is seen by other ITER partners as contributing valuable expertise. The policy response of the fusion community regarding the collaboration with Russian scientists and institutions has thus not been cohesive. Particularly researchers ‘on the ground’ wonder if and how to continue their relations and collaborations with their Russian colleagues. Using bibliometric data about fusion research and qualitative insights from documents and interviews with members of the global fusion community, we try to assess how the collaboration network and community itself might change under the current political realities.

Thomas Kuhn and his cumulative progress through scientific revolutions
Serdal Tümkaya
It has been frequently claimed that Kuhnian picture of science is the following: (i) According to Kuhn, science is not progressive, (ii) is progressive but not cumulative, (iii) is progressive but not-linear (iv) normal science (cumulatively) progresses but paradigm change is not (or not cumulatively) progressive. All these four mistakes, I try to show, are the results of some other mischaracterizations about some basic elements of the Kuhnian picture of science. For Kuhn, normal science and the notion of paradigm are so closely associated. Therefore, any misperception about one of them would have repercussions on the other. However, because the shattering of normal science through emergence of a new paradigm results in the situation in which scientists have to make paradigm choices, the following problem immediately emerges: If a philosopher mistakes Kuhnian ideas regarding theory choice, then she would easily mistake Kuhnian ideas related to scientific progress, whether it be normal or extraordinary science. Here I argue that Kuhn has defended the existence, importance, and (in a certain sense) the cumulative character of scientific progress, both within normal science and also through scientific revolution.

The neglected risk of working in a big scientific group
Yanmeng Xing, Ying Fan, Roberta Sinatra and An Zeng
In science, mentorship is a determinant of the achievements of both mentees and mentors. Various existing works have revealed the positive association between the success of the mentors and mentees, ranging from academic proliferation and prizes. Nevertheless, academia also experiences a high dropout, especially among researchers at early career stages, even in successful groups. Specifically, we collect genealogical data on nearly 350,733 mentor-mentee pairs and 309,654 scientists who published 9,248,726 papers in chemistry, physics, or neuroscience from 1900 to 2021 and investigate the relationship between mentor and mentee achievements. We find that the mentees trained in large groups perform better than those from small groups if they work in academia after training. The degree of collaboration with a mentor during the training period is positively associated with the mentee's long-term academic performance. However, we also find two surprising results : (1) Mentees from large groups have a lower academic survival rate than those from small groups. (2) The more productive a mentor is, the smaller the probability a mentee survives in academia. Furthermore, the evolution analysis over 60 years shows that the large laboratories are losing advantages in transporting talents into academia. Taken together, we surprisingly find that the success of the mentors could act as a negative factor associated with the survival rate of the mentees in academia. Our findings have the potential to give practical suggestions to individuals and education managers concerning career development and researcher cultivation.

Alexandria: a Proof-of-Concept Publication Platform that Treats Academic Outputs like Software Artifacts
Andrew M. Demetriou, Amy van der Meijden, Jos Sloof, Mattheo de Wit, Emiel Witting, Andreea Zlei and Cynthia C. S. Liem
What should scientific publication look like in the 21st century? Discussions abound with regards to the lack of wide-spread adoption of contemporary standards, such as open peer review, materials and data, adherence to pre-registration, interoperability of materials, the length of time from submission to publication, etc. We take a ‘fail-loudly’ approach to crowdsource a solution that will meet contemporary meta-science standards: specifically, we are building an online

platform as a proof-of-concept, designed to inspire a response either in the form of collaborative efforts to design and build it, or alternative solutions. Our proposed solution is inspired by best practices in software engineering, more specifically by the git versioning system,in two ways. Firstly, by open-sourcing and documenting every aspect of our process, we allow for easy cloning, forking and adjustment. The aim is to make it as simple as possible for others to either build on, or completely re-think our work in the case that our solution fails. The more individuals working on a solution, the more potential solutions offered, the greater the chances of finding a sustainable solution. As part of this effort, we aim to inform as many potentially interested parties as possible so that, should we fail, we will fail 'loudly'. Secondly, our git-inspired suggestion is an alternative form of scientific publication: we treat academic outputs similarly to software artifacts, as collective contributions to a 'living article'. An entire software team collaborates by working on components of a single piece of software, making continuous improvements, trying out new functionalities in separate branches, that will be merged upon approval. Furthermore, at certain points in time, the software artifact may be deemed mature enough for an explicit release with a version number, while development can still continue thereafter. Similarly, we treat publications like software artifacts by providing a platform that allows for continuous collaboration on the components of scientific products. Using the functionality of git we allow for complete transparency of all reviews and contributions, massive online collaboration and replication, deconstruction of the whole into parts so that specific expertise can be applied to specific components, all in a single reproducible digital notebook submission format (quarto). Thus we present Alexandria: an open-source proof-of-concept online platform being developed by second-year bachelor students at Delft University of Technology.

Is science updating the definition of human diversity?
Sakshi Ghai
Great strides have been made to increase awareness about diversity, equity, and inclusion (DEI) in science. However, progress in this space is slow and the conversation on achieving DEI in science has largely privileged a Western perspective. For example, the critical issue of racial discrimination is relatively more salient in the diversity discourse within many high-income countries, which remain a global minority. Yet, in many emerging and low-income countries, a global majority, other marginalized identities are often overlooked yet significantly more prominent than race. For example, in the context of India, many groups are stigmatized and dehumanized based on religion or caste. In this talk, I will first introduce the complexity of capturing diversity in science. Second, I will present tangible ways to rethink diversity and measure both between-country and within-country diversity. Finally, I will propose a global survey to generate a hyper-local understanding of culturally salient diverse identities across the Global South – Asia, Africa, Latin America, and the Middle East. Data from such a survey could surprise us with more similarities rather than differences across marginalized social identities between the Global North and Global South. Equally, we will integrate multiple dimensions of diversity (e.g., religious, tribal, political identities, etc.) that will expand the repertoire of DEI, making science applicable to most of the world's population.