Philosophy of Science Between the Natural Sciences, the Social Sciences, and the Humanities. Introduction

The German Society for Philosophy of Science (*GWP*) organises a conference every third year with the intention of representing all fields of philosophy of science. These triannual conferences aim to strengthen the connection between philosophers of science within German speaking countries, but also to foster relations to the international philosophy of science community.

In March 2016 the second conference of the *GWP* took place at the University of Duesseldorf, organised by the Duesseldorf Center for Logic and Philosophy of Science (*DCLPS*). Topics of many subfields of the discipline were addressed, especially those of the philosophy of the natural sciences, the life sciences, and the social sciences, but also from areas of philosophy of science concerned with the humanities. Details about the conference are available in Christian, Feldbacher-Escamilla, & Gebharter (2017). For general details about the standing of and foci within philosophy of science in Germany the reader is referred to Unterhuber, Gebharter, & Schurz (2014).

This collection of nine papers tries to reflect on discussions and results of the last GWP conference. It contains general approaches that try to tackle traditional problems and questions from a new angle, such as, e.g., the problem of induction, the question of reducability of disciplines, and the question of how to adequately describe and understand scientific change. Besides these more or less general approaches it also contains very detailed investigations showing the high degree of specialization in terms of methods and topics within the philosophy of science. In what follows we provide a summary of the contributions to this special issue. The first three articles were written by plenary speakers.

Stathis Psillos' paper "*Induction and Natural Necessities*" discusses the thesis of the so-called "necessitarians" who try to argue that if natural necessities are posited, the problem of induction vanishes. The underlying idea is that necessity justifies restricting the number of possible future situations to those which are similar to situations of the past. Psillos investigates two necessitarian approaches: On the one hand there is Armstrong (1983) who claims that necessity is an extrinsic relation. As a matter of contingent fact, there are relations of necessitation among properties in the actual world. For this reason, given that there is a necessitating relation between property F and property G, the claim is that unobserved instances of F have to be also instances of G. On the other hand there is Ellis (2001) who claims that necessity is an intrinsic relation: It is necessary that there are relations of necessitation among properties. This thesis is argued for by dispositional essentialism with two further theses: Properties have essences, and properties are powers, i.e. they are individuated by their causal profile. According to this approach, since it is part of the essence of F to bring about G, it holds: Whenever F is instantiated, so will be G; this holds not only for observed and unobserved Fs, but also for all possible Fs. Both approaches aim at showing that accepting specific metaphysics allows one to solve the epistemic problem of induction. However, there is a problem with both approaches: Armstrong claims, given the up to now observed data and regularities between properties F and G, by inference to the best explanation (*IBE*) one is licensed to postulate a necessity relation between F and G in order to explain the regularity within the observed data. And the presence of the necessity relation allows for justifying inductive inferences from the observed to the unobserved data set. It is clear that in order to avoid a circle, *IBE* has to be understood in terms other than "inductive inference". Psillos discusses and criticises for this purpose the approach of Foster (1983) to *IBE*. What is more, one should note that since the data is from the actual world, the "necessity" relation can be stated also for instantiations of the actual world only (the phrasing is "it is a matter of contingent fact that there is a necessary relation ..."). But then the question arises, what necessity relativised to instantiations of the actual world means other than plain generality: All Fs (observed as well as unobserved) are Gs. The latter claim is just the inductive generalization, so *IBE* would not lead via the detour of a necessity relation to an inductive generalization, but directly. It seems that for this reason Armstrong's approach fails to solve the problem of induction. Regarding Ellis' position the criticism is among the following line: As noted before, dispositional essentialism argues that properties have essences which are manifested (i.e. dispositionally accessible) via their causal relations. This metaphysics of properties is expected to have the following impact on the problem of induction: Essential modal properties restrict the number of a possible future (i.e. unobserved) data sets to just one, namely that which is exactly like the observed part of the data set. However, as Psillos points out, it is highly questionable whether one can recognise kind essences without "generalizing on what has been observed already in the past". In order to overcome this problem, one might refer to the approach of Sankey (1997), according to which such kind essences can be identified via knowledge about the success of science: The success of our scientific theories can be explained best by assuming that those natural kinds exist that are presupposed by our theories. But then one ends up, again, with an application of *IBE*. And this is, as stated above, in need of non-inductive justification. For these reasons Psillos thinks that the challenge of justifying induction cannot be met by inflating metaphysics. What is needed is an epistemological answer to an epistemological problem.

Alexander Rosenberg's "*Why Social Science is Biological Science*" might sound more provocative than its main thesis is, namely the thesis that Darwinian processes play a central role in human behaviour. By this claim Rosenberg does not propose a reductionism in the explanatory or ontological sense: Neither does he claim that human behaviour is only or mainly genetically driven, nor does he claim that sociological explanations have to be in biological terms. What is claimed, however, is that social science needs to employ experimental methods from biology. In this (and only in this) sense social sciences are presented as divisions of biology, as "studies of one particular biological species". The main argument provided for a Darwinian social science stems from the premise that social sciences individuate their descriptive taxonomies functionally and seek regularities that obtain between instances of their taxonomic categories; since human behaviour serves functions and since all functions are adaptions whose only natural source are Darwinian processes of blind variation and selection (environmental filtration) resulting in local equilibria, also the study of human behaviour has to be a Darwinian investigation of such equilibria. As Rosenberg makes clear, such an approach has the advantage that it avoids problems stemming from an overestimation of the applicability of models from rational choice in the social sciences. However, for such an approach also a serious problem of identifying replicators underlying such processes in the cultural realm arises: Darwinian processes require replicators, but there seem to be no replicators in human affairs. He argues that replicators similar to genes are not necessary for Darwinian processes: Only evolution in environments with slow changes brings about replicators like genes; adaptive evolution, as might be supposed to be the default case for cultural evolution, may employ replicators and processes of replication quite unlike genes. Due to the complexity of this kind of replicators, it is a future task for cognitive neuroscience to identify the "memes" that transmit and encode matters of interest to the social sciences, so Rosenberg. These claims about an adequate methodology of social sciences go together with a claim about restructuring such a Darwinian social science. For the biological realm, Tinbergen (1963) suggested a quadripartite division of biological inquiry around the questions of a trait's function, its emergence, its development in the individual, and its underlying mechanism. Similar to this it is thought that a subdivision of a Darwinian science of human affairs will look quite different from the current set of social sciences which is up to now, so Rosenberg, a quite "implausibly dividing [...] into psychological, sociological, political, economic, or some such a taxonomy of kinds".

Gila Sher's contribution on "*Truth and Scientific Change*" investigates the relation between truth on the one side and scientific change on the other. Regarding scientific change she starts with the now more or less traditional distinction between optimistic and pessimistic meta-induction. Optimistically, one might conclude that our best scientific theories approach a correct description of the world, because otherwise it would seem like a miracle that predictive success concentrates around them. Pessimistically, one might conclude that they will still be found to be incorrect, since all theories that have been considered to be among the best ones in the past have turned out to be incorrect later. Sher argues that both views are correct given a specific interpretation, although on surface level they seem to be incompatible. Her resolution of this seeming incompatibility is based on her understanding of "truth" which is "inspired by Kant's philosophy". The rough idea is that "we, humans, live in a world of which we are a part. We aspire to obtain objective knowledge of that world, but our cognitive resources in doing so are limited." For this reason, Sher thinks, knowledge and truth are based on three cornerstones: (i) on an "orientation towards the world" (ii) which is influenced by our "cognitive make-up" (iii) in a "dynamic" way. Inspired by Kant, she tries to explicate (i) by help of investigating the "semi-Kantian" question "under what cognitive conditions does the question of truth emerge?" (in Kantian terms one would ask for the conditions of the possibility of truth). Her analysis brings about three such conditions or modes of thinking: the immanent mode which covers the "need to be able to think about something external to our thinking"; the transcendent mode which covers the need to adopt this external point of view; and the normative mode which allows us to evaluate immanent thoughts. These are, according to Sher, the "fundamental principles of truth". Very simplified put, the truth of a theory T consists in its correspondence with our access to the world which is nothing else than another theory $\boldsymbol{T}$. By this one also refers to the second cornerstone inasmuch as $\boldsymbol{T}$ represents the result of our "cognitive make-up" influencing our orientation towards the world we live in. In doing so, Sher tries to connect her approach to the so-called "realistic-aspirations argument" of van Fraassen (1980), namely that by the assumption that scientific theories aim at giving a true story of what the world is like, acceptance of a scientific theory involves the belief that it is true. If one assumes furthermore and by this exceeding van Fraassen that "to believe in $\boldsymbol{T}$, i.e. the truth" is epistemically indistinguishable from "$\boldsymbol{T}$ being true", then it follows that acceptance implies truth. Such a view might be connected to Sher's argumentation: Since we aspire to obtain knowledge of the world we live in, and our access to it is influenced by our cognitive abilities, it might be plausible to assume that aspiration of a theory is an indicator for the truth of the theory. However, "in order to avoid that one merely by directing our mental gaze at anything in the world we would automatically have correct cognition of everything about it, we need the normative mode of thinking." Now, the crux of her theory of truth is that the normative principles underlying truth are not static, but dynamic—cf. (iii). By this, the truth of a theory T consists in its "dynamic correspondence" with $\boldsymbol{T}$, or better: its correspondence with dynamic $\boldsymbol{T}$. So, the idea is that based on different epistemic norms we end up with different "working truths" $\boldsymbol{T_1}$, $\boldsymbol{T_2}$,... and "T is true." means that there is a correspondence between T and that $\boldsymbol{T_i}$ which results from the epistemic norms valid at the time of investigation of T. The formulation of epistemic norms is not arbitrary, but regulated by preceding norms and theories evoking changes of norms—this is, in a nutshell, Sher's so-called "foundational holism". The dynamics regarding theory change and change of our working truth-conditions can be very simplified described by the following schema: One starts with truth-conditions resulting in $\boldsymbol{T_1}$; after investigating a set of rival theories one ends up with a theory T_1 corresponding to some degree $\boldsymbol{T_1}$. Some deviations lead to new truth-conditions resulting in $\boldsymbol{T_2}$; after investigating a set of rival theories one ends up with a theory T_2 corresponding to some degree $\boldsymbol{T_2}$. And so forth. Sher describes the dynamis as follows: "The discovery of problems will provide us with resources for developing better theories, better truth conditions, and still better theories that satisfy these better truth conditions." Now, this reciprocity in changing our working truth-conditions and theories allows for an explanation of the different inferences made by optimistic and pessimistic meta-induction: Optimistic meta-induction correctly highlights the fact that up to now we always ended up with a theory T_i approximating (in the sense of partly corresponding to) our working truth-conditions manifested in $\boldsymbol{T_i}$. And pessimistic meta-induction correctly highlights the fact that up to now such a theory T_i always turned out to be false regarding the subsequent working truth-conditions manifested in $\boldsymbol{T_{i+1}}$. However, as Sher stresses correctly, to infer from this that all theories are false makes no sense since according to this theory of truth there is no one and only set of truth-conditions overloading all working truth-conditions. Hence, "what follows from the history of science is not that human theories are bound to be false, but that theoretical change is likely to continue. There is nothing hopeless about this conclusion."

Alexander Christian's contribution "*On the Suppression of Medical Evidence*" is concerned with the suppression of evidence in medical research and its philosophical implications for the concepts of scientific objectivity and bias. Centered around a case study on Tamiflu, a drug used for the prophylaxis and treatment of influenza type A and B, Christian's paper aims to show that suppression of evidence must not be construed exclusively as unjustified restriction of access to research findings. He demonstrates that for an adequate understanding of the problem it must be recognised as the active promotion of ignorance through a variety of concrete questionable research practices that are commercially motivated and affect research and publication processes. These practices bring about belief in falsehoods, a lack of favourable knowledge about pressing epidemiological issues and an enduring inability to identify and correct biases. Pharmaceutical companies apply these practices to protect their commercial interests. Having reconstructed major mechanisms involved in the active production of ignorance, Christian proposes to conceptualise scientific objectivity as the absence of bias from scientific reasoning. This account of objectivity, which focusses on real world research processes rather than research findings and their relation to reality, in turn requires an explication of bias in research. Christian explicates the concept of bias in a way that ensures the identification of responsible agents and the moral accountability of ill-intended or careless professional agents. On his account, an agent is biased, if and only if the agent has a tendency in research and publication processes to act in a way that results in a form of misrepresentation. This agent-based definition helps to unify formally disparate notions of bias and makes their connections transparent. These advantages are illustrated using the example of publication bias, which can be defined with agents (authors, editors, readers etc.) steps in research (scientific writing, submission of manuscripts etc.) or error types (misrepresentation of specific findings) in mind. Against this backdrop, the author characterises suppression of evidence as intentional or negligent conduct in research processes in situations in which there is a legitimate expectation of informed acceptance by all involved parties of specific research goals, full disclosure of relevant research findings in theory confirmation and public scrutiny. Regarding the moral status of ignorance-inducing practices, Christian argues for broadening the focus beyond the principle of openness in science to address the diverse conflicts with codified principles of responsible conduct of research. Recognizing the corrupting influence of commercial conflicts of interest in pharmaceutical research gives rise to two recommendations: Based on the precautionary principle, Christian proposes a turnover tax for pharmaceutical companies in order to finance independent research institutions that help ensure the epistemic integrity of scientific evidence. Concerning individual research, Christian highlights the importance of fostering competence and willingness to identify fraudulent and questionable practices in research and academic discourse.

In "*Optimisation in a Synchronised Prediction Setting*" Christian J. Feldbacher-Escamilla addresses the problem of accurate predictions in a prediction setting. Since in a multitude of prediction tasks like forecasting weather conditions, market trends etc. we generally lack theories about strict and exact regularities, the classical approach is to apply inductive methods on data about the past. However, it is clear that the application of such methods is in need of justification. One approach to this form of the problem of induction is the so-called "best alternatives approach" which aims at justifying inductive methods via their status of being the up to now most successful available methods: It is possible to prove that choosing among available prediction methods by weighting according to the methods' up to now success rates turns out to be long-term optimal in terms of success rates. Since such a method for choosing among prediction methods generates its weights out of the success of other methods, it is called a "meta" method. And since it manages to "transfer" past success to future success, it is called an "inductive" method. This meta-inductive approach to the problem of induction is carried out, e.g., in Schurz (2008). However, the approach has a serious drawback. In order to prove optimality results one needs to assume that the prediction space is continuous (e.g. $[0,1]$). For a discrete prediction space (e.g. $\{0,1\}$) strict optimality cannot be proven. In the literature two approaches for justifying inductive prediction methods in a discrete prediction setting are proposed: The randomisation approach which tries to mimic a continuous prediction by making discrete predictions that are random, but biased towards the optimal continuous prediction. If, e.g., the optimal continuous prediction would be .75, and the discrete prediction space is binary, then in 75% of similar cases the method predicts 1 and in 25% of similar cases it predicts 0. By help of further assumptions one can then prove optimality results regarding expected success. The second approach is a meta-meta-approach which tries to mimic a continuous prediction by introducing further meta-methods that make discrete predictions which on average approximate the optimal continuous one. In the example above with an optimal continuous prediction of .75, adding 100 meta-methods where 75 predict 1 and 25 predict 0 leads on average to a success that equals the success of an optimal prediction in a continuous setting. Both approaches suffer from proving only optimality of expected success or success on average. To overcome this problem, Feldbacher-Escamilla suggests to consider the problem in a synchronised prediction setting, where methods are supposed to deliver continuous as well as discrete predictions. The idea is that since we do not know whether the adequate or correct prediction space is continuous or discrete, we should demand from prediction methods to serve both ends. An epistemic counterpart to such a setting are agents who have degrees of belief as well as plain believes. And, assuming that both forms of belief exist next to each other, it is clear that both should be also somehow in accordance with each other. So, e.g., one should not discretely predict 1/believe a proposition while at the same time one continuously predicts/has degrees of belief $< .5$. In the paper two rationality constraints for synchronising discrete and continuous predictions are proposed: A synchronous principle which avoids a case just as discussed; and a diachronic principle that demands a method to be calibrated, i.e., if the discretely predicted frequency of an event turns out to be r, then at some point in time the continuous predictions should equal r. By proving that scenarios undermining optimality for the discrete prediction setting do not satisfy both synchronisation constraints at once, the author is able to identify them also as conditions for enabling optimality of discrete predictions in a synchronised prediction setting.

Carsten Held presents and investigates in "*Ceteris-Paribus Qualifiers*" the idea that a new approach to the problem of antecedent-strengthening can be also successfully applied to problematic cases of *ceteris-paribus* (CP) qualifications of laws. Antecedent-strengthening comes, e.g., in the form $\forall x(P(x) \rightarrow Q(x)) \vdash \forall x((P(x)\&R(x)) \rightarrow Q(x))$. Now, it is clear that for some instantiations of P (rain), Q (wet), and R (covered) this inference seems unplausible. A standard solution is to introduce CP-qualifiers of the form "If $P(x)$ and everything else relevant remains unchanged, then $Q(x)$." Formally: $CP(\forall x(P(x) \rightarrow Q(x))$, or, allowing also for infinite conjunctions: $\forall x(P(x)\&C_{cp}[x]) \rightarrow Q(x)$, where $C_{cp}[x]$ is a conjunction of all propositions according to which there are no factors present that prevent $Q(x)$ to be true. A problem with this approach is a threat of vacuity: If $P(x)$ allows for the presence of factors that prevent $Q(x)$ to be true, $\neg P(x)$ is in $C_{cp}[x]$ and by this the material implication is vacuously true. If not, then $C_{cp}[x]$ restricts the situations such that $Q(x)$ is always true. Hence, the material conditional is vacuously true. Traditionally $C_{cp}[x]$ is restricted (describing ideal situations), however, Held suggests to let $C_{cp}[x]$ unchanged, but replace $\rightarrow$ by a conditional different from the material conditional. For this purpose he utilises the approach to conditionals of Lycan (2006) which enriches the structure by speaking of P-situations that are Q-situations. Formally: $\forall x(In(x,P) \rightarrow In(x,Q))$. Combined with restricted quantification in the sense that existential import is generally assumed, one can define such a conditional: Let $\forall x(P(x) \Rightarrow Q(x))$ stand for $\exists x In(x,P)\&\forall x(In(x,P) \rightarrow In(x,Q))$. Interpreted in such a way, antecedent-strengthening fails: $\forall x(P(x) \Rightarrow Q(x)) \nvdash \forall x((P(x)\&R(x) \Rightarrow Q(x))$. However, there is still a problem: Given the strong interpretation of C_{cp} as the conjunction of all propositions that rule out $\neg In(x, \lambda y \neg Q(y))$ it holds that $\forall x((P(x)\&C_{cp}[x]) \Rightarrow Q(x))$ is equivalent to $\forall x((P(x)\&Q(x)) \Rightarrow Q(x))$. The latter is not logically valid, but only because $\exists x In(x, \lambda y(P(y)\&Q(y)))$ is not logically valid, whereas the relevant part with the implication is. For this reason Held expands the framework further by principles for conditions (in contrast to conditionals). Very much oversimplifying one might express the result of this investigation by a further context-dependent qualifier which blocks the equivalence of the two claims above. Technically, as proposed by Lycan (2006), this can be implemented by just adding next to the CP-qualifier $C_{cp}[x]$ a(nother) context-dependent qualifier $S(x)$: $\forall x((P(x)\&C_{cp}[x]\&S(x)) \Rightarrow Q(x))$. E.g., in the context of an agent promising $Q(x)$ in case of $P(x)$, $S(x)$ might filter out those P-cases that are controllable for the agent. If the context changes, as might happen, e.g., via strengthening the antecedent, then also the context-dependent qualifier might change: $S'(x)$. So, strengthening the antecedent happens according to the following schema: From $\forall x((P(x)\&C_{cp}[x]\&S(x)) \Rightarrow Q(x))$ we end up with $\forall x((P(x)\&R(x)\&C_{cp}[x]\&S'(x)) \Rightarrow Q(x))$. Again, the latter does not follow from the former. However, the main burden of invalidation is not only due to restricted quantification, but also due to counterinstances to the implicational part. Regarding CP-laws one is now able to distinguish three cases of possible counterinstances: exceptions (via $C_{cp}(x)$), entities that are not covered (via $S(x)$), and defeaters (via $P(x), C_{cp}(x), S(x)$, and $Q(x)$). Take, e.g., "All ravens are black." to be a CP-law. Then a raven that turns spontaneously albino might count as an exception, a classical albino raven (similar to a white shoe) as being not covered, and a non-albino white raven as a defeater of the CP-law.

In "*Making Sense of Interlevel Causation in Mechanisms from a Metaphysical Perspective*", Beate Krickel provides a new argument in favour of so-called "inter-level causation". The background theory she starts with is a mechanistic approach of scientific explanation which states that the metaphysics of causation should be understood in terms of mechanisms and that the latter come in hierarchies of levels of mechanisms. Since causal explanations seem to refer to different levels, one central question of this approach is whether there can be causal relations between entities at different levels, *inter*level causation in mechanisms. Craver & Bechtel (2007) argue that causes and effects are wholly distinct since their occurrence is temporally asymmetric, and effects depend on causes, but not vice versa. On the other hand, wholes and their parts are spatiotemporally coexisting or at least overlapping, changes in them occur simultaneously, and they are mutually dependent. For this reason causal relations hold between causes and effects which have to be wholly distinct. However, entities at different levels of mechanisms are related via a part-whole relation. Hence, they cannot be causes or effects of one another. From this it follows that there is no interlevel causation. However, there are lots of examples where we seem to speak of interlevel causation. Consider, e.g., someone's heart which is a component of bodily mechanisms. The failure of blood supply to the heart muscle (part of the whole mechanism) seems to be correctly considered as the cause of something on the level of the whole mechanism, namely death. But speaking of "cause" and "effect" implies the presence of interlevel causation. Contrary to this, Craver & Bechtel (2007) argue that causal relations are only *intra*level relations; interlevel relations like the heart attack and the death of someone are considered by them to be mechanistically mediated non-causal constitutive relations. But, as Krickel points out, such an approach is in need of rephrasing lots of our causal speaking. In order to approach the problem of *inter*level causation, she therefore suggests to describe the relata of the part-whole relation as entity-involving occurrents. This has it as consequence that causal relations between wholes and parts are not precluded for trivial reasons. Speaking of "occurrents" in contradistinction to "continuants" is common in metaphysics, where continuants are said to exist *in* time, whereas occurrents are taken to exist *through* time. Such occurrences can be regarded as space-time worms and they have parts in two different senses, namely spatial parts and temporal parts; Krickel argues that explanations as provided in the example above allow for at least two interpretations. According to one interpretation, the part-whole relation involved in this example is spatial and holds between the occurrence of the heart's failure and the occurrence of that person's dying. In this case both occurrences are in the same space-time region and by this a causal relation is excluded. However, according to a second interpretation the death of the person is taken to be a temporal part of the occurrence of her dying. This makes interlevel causation possible, because some spatial parts of occurrences (components of the mechanism) of the phenomenon and some of its proper temporal parts are not themselves related as parts and wholes, so they are at different levels. For the general case, Krickel suggests to take mechanistic interlevel causation to consist in causal interactions between the components of the mechanism and temporal parts of the occurrences of the phenomenon.

Markus Schrenk investigates in "*The Emergence of Better Best System Laws*" the problem of how laws of the special sciences might be considered to emerge from more fundamental laws. He does so in terms of the so-called "*better best system account*" Cohen & Callender (2009, cf.) which allows, in contrast to David Lewis' *best system analysis*, also for the possibility to launch system analyses separately for the set of properties of some special science yielding to laws of that special science. Lewis suggested to consider a true contingent generalisation as a law of nature if and only if it is an axiom or a consequence of the one deductive system with natural properties that, amongst all the possible systems, achieves by far the best balance of simplicity, strength, and fit. A problem with this approach is that also the laws of the special sciences have to be theorems from the fundamental axioms, so special science vocabulary has to be introduced via bridge principles by help of terms of the fundamental vocabulary that refers to perfectly natural properties only. But if so, then the overall theory is a reductive one. To solve this problem the *better best systems account* suggests to perform a best system analysis for each special science separately. In such a way one is not forced to reductionism, but one can combine such an approach with an emergentist analysis of laws, where the emergence relation is supposed to allow for considering the higher-order laws to be autonomous in some sense, but also somehow dependent from the more fundamental ones. One problem with such an emergentist reconstruction is that autonomy and dependence pull in different directions. Schrenk suggests to resolve this "schizophrenic friction" between autonomy and dependence in the following way: Autonomy of the higher-order laws from more fundamental ones results from the fact that the choice of attributes for the special sciences happens independently of the latter ones; this is due to running several competitions for different predicate sets separately. Dependence is given by supervenience: A higher-order E-law depends on more fundamental G-laws if and only if the E-law about attributes E_i emerges from laws about attributes G_i by supervenience. I.e.: (i) For all x_1 consisting of $y_1, \ldots, y_n$ and x_2 consisting of $z_1, \ldots, z_m$ it holds necessarily that if $G_i[y_1, \ldots, y_n]$ if and only if $G_i[z_1, \ldots, z_m]$, then also $E_i[x_1]$ if and only if $E_i[x_2]$. (ii) If the E-law is a strict law, then it is true by virtue of G-laws and (i). The laws and properties on the lower level might be a supervenience base for the properties and regularities above (i.e. dependence), but they are "blind" as to which of these regularities come out to be nomological (i.e. autonomy). (ii) refers to strict regularities. But what if the regularities in question are not strict ones: So, e.g., instead of a law of the form: All E_1s are E_2s, one might have to analyse a ceteris paribus (CP) law of the form: *Ceteris paribus*: All E_1s are E_2s. Schrenk argues that CP-laws are to be explained by multiple realisability and incomplete coverage of regularities among the parts on the lower level: So, regarding the supervenience relation as stated in (i) above, it might hold that x_1 and x_2 are indistinguishable regarding the E_is (i.e.: $E_i[x_1]$ if and only if $E_i[x_2]$), although they are differently realised (their constituents can be distinguished by their attributes G_i: $G_i[y_1, \ldots, y_n]$ not exactly when $G_i[z_1, \ldots, z_m]$). In such a case G-laws determining an E-regularity of properties of x_1 might not determine an E-regularity of properties of x_2. So, for CP-laws one can add the condition (iii): If the E-law is a CP-law, then it still is in agreement with (i), but it is not true by virtue of G-laws. So, CP-laws not only allow for autonomy too, they even seem to increase it without fully giving up dependence.

Christian Wallmann's "*A Bayesian Solution to the Conflict of Narrowness and Precision in Direct Inference*" tackles the problem of how to justify direct inferences. In the simplest case, a direct inference is the inference to the probability that a certain individual (c) belongs to a target class ($P(x)$) by equating it with the relative frequency of the target class ($P(x)$) in a suitable reference class ($Q(x)$). So, the principle is to infer from $Pr(P(x)|Q(x)) = r$ and $Q(c)$ that $Pr(P(c)) = r$. However, very often there are more complex situations with knowledge of frequencies of several reference classes. A *prima facie* reasonable approach is to use for a direct inference the narrowest reference class (e.g. $Q(x)\&R(x)$) to which the individual (c) belongs to. So, knowledge of the narrowness of a reference class might defeat the above direct inference: Given $Pr(P(x)|Q(x)\&R(x)) = s$ and $Q(c)\&R(c)$ one might overwrite the result above by inferring $Pr(P(c)) = s$. However, narrower reference classes have less members and by this it is harder to take a sufficiently large sample of individuals that belong to it, for which reason one's knowledge about frequencies might be less precise. This means, e.g., that statistical methods allow only for interval-information of probabilities. So, aiming at narrow reference classes and aiming at precise estimates of frequencies are competing aims. For this reason also the problem of how to deal with cases of the form $Pr(P(x)|Q(x)) = r$ (not narrow, but precise) and $Pr(P(x)|Q(x)\&R(x)) \in [s_1, s_2]$ (narrow, but not precise) shows up. A classical approach to this question is provided by Kyburg & Teng (2001), who propose three criteria: The criterion of precision states that in case of intervals where one is a subset of the other, the more precise interval should be chosen. The criterion of specificity states that if no interval is a subset of the other, then the interval with the narrower reference class should be preferred. And the combination criterion demands that again, if no interval is a subset of the other, then the chosen interval has to be within the range of the intervals. For a body of evidence on many reference classes these criteria are applied iteratively. As Wallmann stresses this approach has a couple of problems: First, minimal changes in reference class probabilities may lead to huge changes in the probability one gets in a direct inference. So, such an inference is not very "stable". And second, the center values of those intervals do not matter at all in this approach, although they maximise the likelihood of the observed sample and for which reason they should be taken into account. An alternative approach is provided by Thorn (2017), who argues that in an inference of the probability of c being a P, where one knows that exactly the possibly molecular property Q holds of c, the ("expected") frequency in the narrowest reference class maximises the (expected) epistemic accuracy. So, given such a Q, the direct inference rule presented above is justified via an accuracy constraint: $Pr(P(c)) = Pr(P(x)|Q(x))$. Thorn himself identifies and discusses several assumptions as, e.g., that the measure of accuracy has to be a proper scoring rule or that a generalised form of the argument about "expected" frequencies and expected accuracy is sound only, if the distribution employed to calculate the expected accuracy is also empirically accurate. Wallmann argues the latter assumption is not satisfied in Thorn's approach, because the method employed there to calculate expected accuracy leads to unreasonable values for probabilities via direct inference: The method relies on weights constructed out of the relative frequencies in arbitrary subsets of the broader reference class. However, as Wallmann shows, in general there is only very small variation of these relative frequencies. For this reason the constructed weights lead to probabilities that turn out to be extreme. This, according to Wallmann, makes the so-called "subset approach" to direct inference as pursued by Thorn, but also by John Pollock, implausible. For his Bayesian solution he suggests to not consider any arbitrary subclasses, but those that are supposed to be causally relevantly related to the target class. A distribution of such sub-reference classes is also called a "natural distribution". Since the variation rate is supposed to be much higher regarding such subclasses, the former mentioned problem vanishes.

References

Armstrong, D. M. (1983). *What is a Law of Nature?* Cambridge: Cambridge University Press.

Christian, A., Feldbacher-Escamilla, C. J., & Gebharter, A. (2017). "The Second International Conference of the German Society for Philosophy of Science (GWP.2016), 8–11 March 2016". In: *Journal for General Philosophy of Science* 48.2, pp. 289–291. URL: http://dx.doi.org/10.1007/s10838-016-9358-4.

Cohen, J. & Callender, C. (2009). "A better best system account of lawhood". In: *Philosophical Studies* 145.1, pp. 1–34. URL: http://dx.doi.org/10.1007/s11098-009-9389-3.

Craver, C. F. & Bechtel, W. (2007). "Top-down Causation Without Top-down Causes". In: *Biology & Philosophy* 22.4, pp. 547–563. URL: http://dx.doi.org/10.1007/s10539-006-9028-8.

Ellis, B. (2001). *Scientific Essentialism*. Cambridge: Cambridge University Press.

Foster, J. (1983). "Induction, Explanation and Natural Necessity". In: *Proceedings of the Aristotelian Society* 83.1, pp. 87–101. URL: http://dx.doi.org/10.1093/aristotelian/83.1.87.

Kyburg, H. & Teng, C. M. (2001). *The Theory of Prbability*. Cambridge: Cambridge University Press.

Lycan, W. G. (2006). *Real Conditionals*. Oxford: Oxford University Press.

Sankey, H. (1997). "Induction and Natural Kinds". In: *Principia* 1.2, pp. 239–254.

Schurz, G. (2008). "The Meta-Inductivist's Winning Strategy in the Prediction Game: A New Approach to Hume's Problem". In: *Philosophy of Science* 75.3, pp. 278–305.

Thorn, P. D. (2017). "On the Preference for More Specific Reference Classes". In: *Synthese* 194.6, pp. 2025–2051. URL: http://dx.doi.org/10.1007/s11229-016-1035-y.

Tinbergen, N. (1963). "On aims and methods of Ethology". In: *Zeitschrift für Tierpsychologie* 20.4, pp. 410–433. URL: http://dx.doi.org/10.1111/j.1439-0310.1963.tb01161.x.

Unterhuber, M., Gebharter, A., & Schurz, G. (2014). "Philosophy of Science in Germany, 1992–2012: Survey-Based Overview and Quantitative Analysis". English. In: *Journal for General Philosophy of Science* 45.1, pp. 71–160.

van Fraassen, B. C. (1980). *The Scientific Image*. Oxford: Clarendon Press.