Quo Vadis Selective Scientific Realism?

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CONTENTS

Greetings

We warmly welcome you to “Quo Vadis Selective Scientific Realism” conference at Durham University and wish you a fruitful and enjoyable stay.

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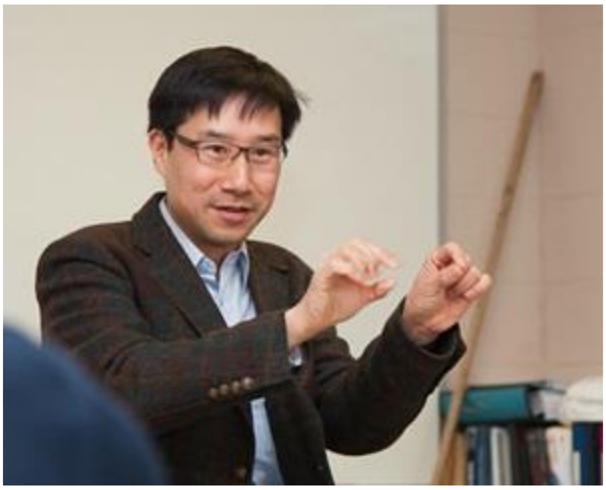

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Abstracts

1. “Scientific Realism and the Justification of Induction”
J. Brian Pitts (University of Cambridge)

Scientific theories depend to varying degrees on a justification of induction vis-à-vis Hume’s induction skepticism. Claims about specific events (perhaps such as historical sciences make), for example, might be more vulnerable than claims about what happens always or for the most part (nearly universal generalizations, perhaps such as experimental sciences make). That is because a few exceptions to the justification of induction might compromise a significant part of the claims of the former kind, but little or nothing of the justified claims of the latter kind. Significant efforts were made to justify induction during the middle decades of the 20th century, especially because of logical empiricism, but are widely believed (such as in Howson’s *Hume’s Problem*) to have failed.

Recently Gerhard Schurz and collaborators have revived and significantly extended perhaps the most promising effort to answer Humean induction skepticism, the Reichenbach-Salmon-Clendinnen pragmatic justification of induction. No predictive method is guaranteed to work, but if anything does, induction does, it is held. This meta-inductive approach employs induction at the meta-level of predictive methods in light of their track records. Meta-induction allows one to entertain *a priori* the possibility of all manner of esoteric prediction methods (such as clairvoyance), and is said to arrive *a posteriori* at the conclusion, based on the actual past, that object-level induction is the best bet. Hence proponents of esoteric prediction methods should be convinced to renounce their esotericism and embrace induction as their full-time predictive method. Such a result would remove the special vulnerability to Hume’s problem of induction faced by historical sciences and claims about specific events more generally.

A difficulty for meta-inductive justification of induction is noted in the appeal to the actual past, a difficulty related to extant short-run worries regarding the problem of induction. Proponents of esoteric prediction methods are likely to think that their predictive methods have actually been more successful than object-level induction. They might well claim testimonial evidence to that effect, possibly not frivolously or tendentiously (*e.g.*, modern medical evidence). If induction is not already known to be justified, then it is unclear how to filter out such reports as unreliable. One can also find examples of meta-inductive reasoning employed to justify esoteric predictive methods, raising the conceptual possibility (however remote) of a meta-inductive *refutation* of induction. Isolated disagreement about the past is a feature characteristic of real debates involving induction in intellectual history and the historical sciences. Hence the meta-inductive justification of induction appears to succeed where no one doubts induction, but might beg the question where a justification of induction would make a difference to real disagreements. The Reichenbach-Salmon-Clendinnen-Schurz meta-inductive justification of induction appears to be incomplete thus far; as a result it does not yet remove the extra vulnerability of scientific claims about specific events, as opposed to (nearly) universal generalizations, to Humean induction skepticism.

2. “Scientific Realism, QM, and History of Science”

Juha Saatsi (University of Leeds)

Selective realists aim to figure out how to separate belief-worthy features of ('mature', 'successful') scientific theories from metaphysical chaff. The latter does not enjoy the full weight of the evidential support there is for a given theory; rather, it is is more likely to play a merely 'auxiliary' (e.g. heuristic) role. The idea of selective realism has been largely shaped by historical case -studies and worries about 'pessimistic induction', but there are also other forces in play that should also be taken on board to inform the discussion.

It is in this spirit that I will I examine here the how the selective realist strategy plays out in the context of quantum mechanics. This is unquestionably part of mature science, and its predictive and explanatory successes are impressive (to say the least!). Yet realists struggle to make sense of their epistemic commitment regarding quantum mechanics, largely due to the existence of radically different variants of QM (e.g. Bohmian mechanics, Everettian QM, and GRW), each of which seems to meet the empirical criteria that elicit the realist commitments, but which are radically at odds with one another metaphysically.

After setting up this problem for scientific realism, I argue that the most popular 'recipes' for qualifying realist commitments – entity realism, structural realism, and semi-realism – fail to meet this challenge as they stand. By further developing some of the ideas behind semi-realism, however, we can provide a defensible minimal realist perspective on quantum mechanics that capitalises on the theory's modal content.

More specifically, regarding entity realism I argue that while one can motivate this realist view by reference to successful manipulations of some distinctly quantum properties as causes of novel phenomena (e.g. spin in connection with 'spinotronics'), the need to give some description of the property being manipulated allows the problem of underdetermination to bite back. (For instance, the character of the entity realist's commitment to spin is radically dependent on whether she adopts (say) a Bohmian variant of quantum mechanics vs. GRW theory.)

Regarding structural realism, on the other hand, I argue that while it can be motivated by being able to identify abstract mathematical commonalities between the different variants of quantum theory, it is difficult to make sense of the realist commitment to 'the quantum structure of the world' without saying anything more specific in non-structural terms that are underdetermined by the evidence.

The issues with semi-realism are arguably less deep. Semi-realism recommends commitment to a 'metaphysically minimal interpretation' of theories' mathematical structures, understood in terms of the modal content represented by those structures. (Chakravartty 1998, 2015; Egg 2012, 2016) The advocates of semi-realism have construed this modal content in causal terms, in ways that I argue are problematic in connection with quantum mechanics. But the broader idea that our realist commitments should primarily concern the modal content of our best theories is defensible in relation to quantum theory, and I argue that here we find fertile middle ground between anti-realism and standard realist proclamations. In support of this claim I make contact with modal theories of explanation, and use an analogy to the underdetermination of metaphysics of causation in scientific explanations.

3. “Local Approaches to the Scientific Realism Debate: Theoretical or Experimental?”
Jonathan Hricko (National Yang-Ming University) and **Ruey-Lin Chen** (National Chung Cheng University)

One way to be a selective realist is to adopt what we call a local approach to the scientific realism debate (Fitzpatrick 2013; Magnus and Callender 2004; Saatsi 2010). According to such an approach, we ought to determine whether to be realists about particular theories by examining the relevant first-order scientific evidence. While these approaches are promising, they tend to be somewhat theory-centric, and our goal is to develop and defend a local approach that is centered on experimental practices. According to our approach, we ought to determine whether to be a realist about a particular kind of entity by examining the experiments that scientists use to investigate that kind of entity.

We begin by arguing that experimental practices can ground claims regarding the reality of a kind of entity even when our best theories in the relevant domain are not even approximately true. For an example, we take Lavoisier’s oxygen theory of acidity, which guided chemists to the discovery of the element boron. According to Lavoisier’s theory, all acids contain oxygen and another substance called the radical. Lavoisier hypothesized that extracting the oxygen from boracic acid would result in the discovery of a new element that he called the boracic radical. And in 1808, Davy, Gay-Lussac and Thénard, decomposed boracic acid and thereby discovered boron. We argue that the moral of this case is not that parts of Lavoisier’s theory are approximately true. Instead, the moral is that false theories can guide experiment in fruitful ways, and that experimental practices, not theories, ground claims regarding the reality of kinds of entities.

However, not all experimental practices ground such claims to the same degree. We go on to argue that experimental practices that individuate entities provide the strongest evidence for the reality of such entities. We take our account of experimental individuation from Chen (2016), and we illustrate this account in terms of the case of boron. In 1825, Berzelius described a method of preparing boron which allowed chemists to experimentally individuate samples of it. We contrast Berzelius’s method with the methods of Davy, Gay-Lussac and Thénard, which did not involve experimental individuation. And we argue that Berzelius’s method constitutes stronger evidence for the reality of boron.

Finally, we contrast our practice-centered approach with Fitzpatrick’s (2013) more theory-centric “local strategy.” Fitzpatrick takes Achinstein’s (2002) so-called “experimental argument for scientific realism” as an example of the local strategy, but argues that Achinstein is wrong to suggest that his argument is experimental. Against Fitzpatrick, we argue that most arguments for realism about a particular theory are experimental, since the evidence for a particular theory almost always results from experimental practices. Since our approach is centered on these experimental practices, we conclude that it is preferable to Fitzpatrick’s even for those cases in which we’re concerned with whether to be realists about a particular theory regarding some theoretical entity.

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4. "Defending Selective Realism via Current Science: The Renormalization Group in High Energy Physics"

James Fraser (University of Leeds)

A selective approach to scientific realism is widely taken to be the most promising route to a defensible formulation of the doctrine. Rather than accepting a theories content in toto, the thought goes, we should reserve our commitment for some subset of its content which is really responsible for its success. Defenders of this conception of realism have tended to focus on historical episodes in which we can account for the success of past theories in terms of claims which are, in some sense, preserved in present theories. In the case of Newtonian gravitation, for instance, we can arguably explain the theories empirical success by pointing to features which are recovered by its modern successor, general relativity. One challenge facing this strategy of drawing on the history of science however is that it seems leave the crucial question of what we should believe about our current best theories unanswered. Stanford (2006) has argued that the selective realist needs a way of telling us which parts of successful theories to believe now and not merely in retrospect. Our current epistemic situation seems to be analogous to that of a Newtonian scientist before the relativistic revolution; we don't know which aspects of general relativity will be preserved in future physics and consequently we cannot really substantiate the idea that we should be committed to a subset of its content.

One option in the face of this problem is to bite the bullet and move to a more minimal conception of scientific realism - this option has recently been explored by Saatsi (2016). In this paper I suggest that we can develop a more direct response to the challenge by turning, not to the history of science, but to the details of our present theoretical frameworks. In particular, I argue that the renormalization group, an important method for studying the properties of quantum field theories at different scales, points to a way of implementing the selective strategy now, in the absence of details about unknown future physics. The renormalization group tells us that some features of present quantum field theories do not make a difference to their predictions at currently observable energy scales, and consequently should not be taken representationally seriously on the selectivist rationale. Furthermore, it allows us to isolate coarse-grained features of these theories which are largely independent of the details of physics at currently accessible energies. Whatever the quantum gravity programme has in store for us then, these aspects of present theories in high energy physics should be robust. The lesson I draw from this case study is that theoretical resources found in current scientific practice can play an important role in developing the selective strategy and the complaint that selective formulations of realism are problematically retrospective reflects a gap in the extant discussion rather than a limitation of the approach itself.

5. "How Philosophy could Save Science"
Ludwig Fahrbach (Heinrich-Heine-Universität Düsseldorf)

Selective realism (SR) implies that currently accepted scientific theories contain false parts which can be expected to be replaced in future theory changes, and which can already be identified today. Depending on the version of SR, the false parts of current theories are the idle parts that don't contribute to the success of the theories, the parts that are not about enti-ties, or the parts that do not concern structure, but concern nature (substance, objects, content, interpretation, etc.). However, so far selective realists have not taken their own claims very seriously. If they did, they would spread out into the scientific world, look at the numerous knowledge claims accepted as scientific fact today, examine them and urge the deletion of the false parts.
A good start would be the examination of scientific textbooks from scientific disciplines such as chemistry, biology, geology, and so on. Selective realists should check all statements in the textbooks, determine the part that are idle, about non-entities, or about nature, inform the textbook authors of their findings, and urge the deletion of the respective parts. It would be interesting to see the proportion of the textbook statements that are found to be wrong, 10%, 40%, 80%, or what.[1]

The project is important, because it has major practical consequences. First, it has conse-quences concerning which research topics to pursue and which to ignore. Simplifying a bit, divide et impera realism implies that research should focus on finding parts of theories that are able to contribute to predictions which can be tested, at the expense of idle parts. Entity realism implies that research should focus on existence claims at the expense of theoretical claims. Structuralism implies that research should focus on claims about structure, and leave aside claims about nature, (substance, interpretation, etc.).

Second, the project has implications for the practical application of currently accepted theories in technology, engineering, medicine, and the like, insofar as the applications depend on the false parts. Philosophers should identify the false parts and alert practitioners of their falsity. This may save humankind from a lot of harm from failing air planes, exploding nucle-ar power plants, and so on. Philosophers will be justly proud to be of use for once.

I'm a making a bit fun of SR here. I do so to highlight the fact that SR in its different forms is highly *revisionary*. It is in deep conflict with the judgments of scientists. Now, selec-tive realists don't choose their position capriciously, of course, but rather react to the PMI. It is unfair to poke fun at them, if one cannot come up with a better response to the PMI.

So, in the second part of my talk I sketch a different counterargument to the PMI. I argue that in the recent history of science there has been an unprecedented

[1] One obstacle that needs to be overcome when pursuing the project is that the central distinctions invoked by the respective forms of SR are quite vague and flexible (Vickers 2016, Eronen 2016).

explosion of scientific evidence, which has translated into an unprecedented increase in the empirical success of the best theories. At the same time the best theories have been entirely stable. This refutes the PMI (Fahrbach 2017). I argue that this is what practicing scientists see every day: They see how rapidly science is developing, and how scientific evidence is improving at ever increasing rates. It is what textbook authors would tell selective realists, if they actually started to suggest deletions to scientific textbooks.

Finally, I discuss two objections to my account. The first objection is as follows: People in the past could have reasoned in exactly the same way as I do today, saying: “There has been a strong increase in scientific evidence recently, making our theories more successful than past theories, and this difference blocks the PMI”, but this reasoning would have proven wrong by the theory refutations that subsequently ensued; hence we should conclude that my reasoning today in response to the PMI also fails (Wray 2013). This objection is intuitively compelling, but surprisingly hard to analyze. I argue that it boils down to a version of the PMI. The second objection claims that we can expect science to keep growing exponentially in the future yielding far better evidence than we have today. We don’t know what that evidence will be, so we cannot preclude that it will refute our current best theories. I show that this objection is misguided.

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6. “Gaining Access to Atomic and Molecular Structure”
Teru Miyake (Nanyang Technological University) and **George Smith** (Tufts University)

Perrin’s work on Brownian motion is one of the most-discussed cases in recent debates about scientific realism (Chalmers 2009, 2011, van Fraassen 2009, Psillos 2011, 2014). Realists such as Chalmers and Psillos have focused on the values Perrin derived for Avogadro’s number, and pointed to the agreement between those values and values derived from phenomena other than Brownian motion as evidence of the reality of molecules. Van Fraassen, on the other hand, has argued that scientists such as Ostwald were worried, not over the reality of the hypothesized molecules, but over the absence of adequate empirical grounding of kinetic theory.

One way to gain a better understanding of the extent to which Perrin’s results can be said to have established the reality of molecules is to contrast them with the subsequent progress in the determination of facts about atomic and molecular structure. Even if we grant that Perrin’s experiments changed the standing of molecular-kinetic theory, somehow or other, in the eyes of scientists such as Ostwald, deep problems for the theory remained unresolved. Such problems, mainly associated with specific heats and their variation with temperature, could potentially have overthrown the molecular hypothesis, depending on how they played out. Progress on these problems was made in the 1910s by the work of Debye and Born and von Karman on the specific heat of solids, Eucken’s work showing that the rotational degrees of freedom of hydrogen molecule freeze out at low temperatures, and Bohr's proposal about the structure of the hydrogen atom. The development of the new quantum mechanics of Heisenberg, Born, Jordan, and Schrodinger allowed David Dennison in 1927 to solve the specific heat problem for hydrogen by showing how the new theory reconciles the shape of the hydrogen molecule and its rotational and vibrational spectra with the variation of the specific heats with temperature. That result opened the way to decades of research devoted to experimentally determining the precise sizes and shapes of diatomic and triatomic molecules. The 1950 edition of the volume of Gerhard Herzberg's *Molecular Spectra and Molecular Structure* on diatomic molecules ends with a table containing scores of results on the distances of the nuclei from one another in the ground state of the dumb-bell shaped molecules, while the 1945 edition of his volume on polyatomic molecules gives precise values for the angle between paired atoms in triatomic molecules.

Through an examination of this history, we argue that the issue of the reality of atoms or molecules is multi-dimensional, and a full analysis of this issue requires a sensitivity to at least the following four factors: (1) the difference between gaining access to microphysical facts versus successful deduction of macrophysical facts from purported microphysical facts; (2) the distinction between establishing microphysical facts versus establishing the truth of a theory; (3) the resolution one can achieve in the determination of microphysical facts; (4) the different kinds of microphysical details one can gain access to.

7. "Causal Knowledge as a Resource for Selective Scientific Realism"
Matthias Egg (University of Bern)

The main challenge for selective scientific realism is to find reliable and prospectively applicable criteria on which parts of scientific theories one should be realist about. Many of the criteria proposed by selective realists have been shown by antirealists not to yield the correct results when applied to cases from the history of science, or at least to depend in their application on the benefit of hindsight. Taking account of these past failures, I propose a new version of selective scientific realism, inspired by Anjan Chakravartty's idea of restricting realism to those posits of which we have detailed causal knowledge.

Since this proposal bears some resemblance to earlier realist ideas, such as Stathis Psillos's attempt to identify the "core causal description" of theoretical entities or Philip Kitcher's distinction between "working posits" and "idle wheels", I will first show that my proposal does not repeat the shortcomings of these earlier strategies. I will then seek to make precise the relevant notion of causal knowledge by elucidating three (individually necessary and jointly sufficient) criteria for those instances of inference to the best explanation generating the kind of knowledge that warrants realism. The virtues of this kind of selective realism will become apparent in my discussion of how the realist should respond to Kyle Stanford's argument from unconceived alternatives.

In the final part of my talk, I will briefly present two case studies that illustrate and support my proposal. The first one concerns Jean Perrin's experimental work on the atomic hypothesis. Drawing on Stanford's criticism of earlier realist treatments of this historical episode, I will show that the criteria previously introduced furnish a better account of the importance of Perrin's work for establishing realism about atoms and molecules. The second case study demonstrates the strength of selective scientific realism in making sense of what particle physicists call the “direct detection” of a theoretically predicted particle. Analyzing the discovery of the neutrino, I show that my version of selective realism precisely captures what differentiates the direct detection of a particle from other (less convincing) means of confirming its existence.

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8. “The Argument from Choice: why theoretical virtues better be truth-conducive”
Samuel Schindler (Aarhus University)

Scientific experiments often produce conflicting data. How do scientists deal with data conflicts when they want to arbitrate between theories on the basis of those data? On the basis of a number of case studies, I argue that theoretical virtues can boost scientists' confidence in viewing data as reliable or unreliable. Such cases, I argue furthermore, are evidence against the empiricist's Negative View, according to which theoretical virtues are pragmatic but not epistemic criteria in theory choice. More specifically, I argue that a rational rendering of scientists' theory-choices requires theoretical virtues to be epistemic criteria of theory choice. I call this the argument from choice for scientific realism.

9. “Scientific Realism and Economics”
Jennifer Jhun (Lake Forest College)

Except in a few circles, questions about realist or anti-realist (in particular, instrumentalist) interpretations of economic theory have often gone unnoticed by the literature of general philosophy of science. In this paper, I suggest economic theory, in particular the equilibrium methodology that undergirds most of modern work in the last century, as an interesting case study in the assessment of selective realism and for the realism debate at large. In particular, I’ll try to argue that the realism/anti-realism debate implicit even in the discussion of selective realism is misguided when it comes to popular diagnoses of economics’ successes – and failures. In turn, this will have further implications about how to think about selective realism with respect to disciplines that partake in what I believe to be a shared methodology relying on equilibrium reasoning as well as more contemporary developments in complexity theory that cross such disciplines.

First, I argue that economics presents a clear case in which philosophers of science often over-emphasize certain theoretical virtues – such as whether the tenets of a theory provide novel *predictions*, or whether they can be interpreted as proclaiming *truths* – virtues that the authors of contemporary economics, such as Alfred Marshall, simply did not think of as primary in economic methodology. This does not mean that economics was not meant to be used in any predictive enterprise. It does, however, suggest that the standard against which we measure a science’s success may have less to do with such notions of accuracy. The central tenets of economics, such as the laws of supply and demand, are typically not just denigrated as being idealizations – they are not even close to being “approximately true.” But economists are well aware of that; this points to some other explanatory role that such tenets play and that what we consider to be successful or unsuccessful in economics has been muddied by these oversights.

One might, however, suggest that the kind of causal knowledge that equilibrium methods can help reveal promises to be a good candidate to be the theoretical posits that are essential to the success of the economic science over time (such as Laudan 1984). I argue that this would also obscure the methodological role of theoretical tenets in economic reasoning such as equilibrium idealizations. Rather, I maintain that such causal analyses actually do not privilege anything determinate as theoretical posits apt for a realist interpretation. If this is the case, then equilibrium methods which have similarly be deployed elsewhere, such as in thermodynamics, are *also* similarly neutral. Yet, this does not mean that we ought to discard realism altogether, even selective realism. Instead it means we ought to rethink the distinction that the realist/anti-realist division cleaves. It is one that doesn't quite fully respect the actual practice and history of economics – and, I shall suggest, quite generally with sciences that deal with heterogeneous, complex, dynamic systems.

10. "Historically Probing the Scope of Structural Realism: The Domain of Special Relativity"
Jan Potters (University of Antwerp)

Structural realism is one of the most well-known contemporary responses to general anti-realist claims based on radical theory-change in the history of science. One example that is often employed as historical evidence for such a structuralist position is the formulation of special relativity. In its ontology, special relativity is quite different from its predecessors: the ether, for example, played a central role in many of its predecessors, but is completely absent from relativity theory. This in itself is often used as an argument against standard scientific realism in terms of theoretical entities. There is, however, a certain kind of continuity at the level of their mathematical structure (see e.g. [Ladyman&Ross, p. 94 – 95]). This is then taken to show that we can and should take a realist stance towards the success of science, but solely in terms of structures, which in turn gives rise to metaphysical claims such as "it's real patterns all the way down".

In this talk, I will argue that the history of special relativity does not necessarily provide a good argument for such general metaphysical formulations of structuralism. For this I will proceed as follows. I will first argue that for Einstein himself [see e.g. his review article from 1907], the theory of relativity was primarily a heuristic instrument, and that it was only through the work of Minkowski and other mathematical physicists that special relativity came to be seen as a theory about reality replacing classical mechanics [Staley 2008]. I will then claim that these developments in the theory of relativity were first and foremost part of a broader shift towards a physical practice that focused primarily on mathematical structures [Walter 1999], and that, as a consequence of this, particular questions about the physical domain of Einstein's [1905] theory of relativity – e.g. concerning the ether or the velocity-dependence of the electron's mass – were no longer considered to be of interest for fundamental physics. As a conclusion I will then suggest that the domain of special relativity indeed concerns mathematical structures in reality, but that the scope of this domain is more contingent and less universal than structural realists often seem to believe.

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11. "Making Content Selectivism the Best Realist Game in Town"

Alberto Cordero (CUNY)

Efforts to improve on Laudan's antirealist list of historical cases have yielded valuable material over the last decade. One initially convincing case is Kirchhoff's theory of diffraction, regarding which direct calculation from Maxwell's equations (made now possible by computers) seems to expose flies in the selectivist ointment (e.g. field values over the slit aperture at variance with a central Kirchhoff's posit) compelling content selectivists to weaken their stance. To Saatsi & Vickers (2011), in particular, the discrepancy found is extreme enough to challenge Kirchhoff's theory "approximately true" status. I use further analysis of the direct calculation in question to contest that pessimistic conclusion and suggest that similar reanalysis applies to other additions to Laudan's list. Next, I feed these results into the content-selectivist strategies led by Juha Saatsi, Peter Vickers, and Ioannis Votsis [SVV]. These approaches, focused on derivations of predictions from theory, clarify and refine previous notions of "theory-part," "success" and "truth-content" in ways that seem promising for content selectivism. But the general strategy at play leans worryingly towards a "bare-bones" version of realism that invites pessimism about the outcome, as recognized by Vickers (2013). I trace this additional line of pessimism to certain specific aspects of the SVV approach, in particular, a tension between the latter's minimalist epistemic emphasis and the ampliative aim of realism, and also an unwarranted level of concern with epistemic guarantees. Interpretive minimalism eases the task of selecting inferential components, but it leads to vague determinations of truthful content and concomitant pessimism. More importantly, minimalism shifts attention away from the realist task of identifying theoretical content realists can judiciously commit to—an augmentative rather than minimalist project. If so, realists should stick to the strategy of content reduction but without an emphasis on finding the absolutely minimum theoretical content needed to yield the impressive predictions of a theory. To improve the task of keeping unnecessary posits out, help would have to be secured elsewhere, most naturally from effective (but not maximally) purgative confirmational resources steadily used in the sciences. Thus, I suggest adjustments that arguably enhance the approach's prospects while keeping the focus on truth-content. The proposed changes both enrich the assessment of theory-parts with resources taken from scientific practice and free the project from anti-naturalist worries about lack of certainty. I argue that realists would get better results by looking instead for the *maximum* content that the most well-established criteria actually used in science *clearly endorses*. The latter seems particularly effective in terms predictive success and freedom from compelling specific doubts attributable to each candidate theory-part. The resulting criterion for identifying parts rich in truthful content arguably yields a version of selective realism that is closer to not only the array of theory-parts and narratives that scientists widely consider successful and beyond reasonable doubt but also closer to the theories and models educated people today actually trust and live by.

12. “Robustness-based Entity Realism”
Markus Eronen (KU Leuven)

The core idea of selective realism is that we are warranted in believing that certain specific aspects of our best scientific theories are true. According to entity realists, these aspects are the ontological commitments of the best theories, the idea being that the entities and properties that scientists manipulate and experiment with are real. However, in contemporary discussions of realism, entity realism (ER) is usually quickly dismissed, probably due to the several serious counterarguments that have been raised against it: For example, it has been argued that ER requires belief in the truth of theories and thus collapses to standard scientific realism, and that it cannot avoid pessimistic induction worries.

Here I formulate a novel version of ER, based on the idea of robustness (also known as triangulation), which has received much attention in other contexts in recent philosophy of science. The idea is that if a scientific entity or property is robust in the sense of being detectable, measurable, derivable, or producible in a variety of independent ways, it is very unlikely that all those independent ways turn out to be mistaken, and consequently we are justified in believing that the entity or property is real. As many entities and properties in science (especially in the life sciences) are extremely robust, we have a very high degree of justification for believing that they are real.

I illustrate this with a case study of the amacrine cell, which is a type of interneuronal cell in the retina. The existence of amacrine cells was still just a hypothesis in the late 19th century, but subsequently the robust evidence for amacrine cells has dramatically increased, to the extent
that their reality is not seriously doubted by any scientists. Based on this example, I also show that robustness-based ER has resources to answer all the main counterarguments raised against original ER. For example, due to its emphasis on independent sources of evidence, robustness-based ER does not require accepting any theory as true, and thus does not lead to standard scientific realism.

Unlike original ER, robustness-based entity realism also provides a strong response to the pessimistic induction argument. Throughout the modern scientific era, new scientific methods and measurement instruments have been developed at an increasing rate. This is also clearly seen in the example of amacrine cells: the number of different ways of detecting and measuring them has not only increased, but is now of a completely different order than 100 or 50 years ago. The evidence that we currently have for scientific entities such as amacrine cells, E. coli bacteria, DNA molecules or sodium ions is vastly more robust than the evidence for any entities in the history of science that were subsequently eliminated (such as the caloric). This defuses antirealist arguments that rely on inductions over cases from the history of science: If there is a jump in the degree of robust evidence for current entities when compared to historical entities that were eliminated, then pessimistic inductions do not get off ground.

13. “An Alternative Definition of Essentiality for Selective Realism”
Mario Alai (University of Urbino)

Psillos' (1999) 'divide et impera' move restricts commitment to components which are *essential* in deriving novel predictions. H is essential when

(1) A novel prediction NP follows from H, together with the rest of the theory RT and supplementary assumptions A, but not from RT+A alone;
(2) no alternative hypotheses H*, non ad-hoc, potentially explanatory, etc., are *available* such that (H*+RT+A)→NP.

Lyons (*BJPS* 2006; *Human Affairs* 2009) argued that this definition —in particular (2)— is too vague to be applicable to any historical case, and proposed to abandon the essentiality requirement altogether; but this would leave realism vulnerable to Laudan's historical counterexamples of successful but false hypotheses.

Instead, I propose an alternative definition. Basically, the essentiality condition stems from Occam's principle that we should assume only what is strictly necessary to explain a phenomenon. Therefore, instead of (2) we may simply require that
(2') H cannot be weakened to H' such that H→H' and (H'+RT+A)→NP
(examples in Alai "Defending deployment realism against alleged counterexamples" §7, in *Defending Realism*, 2014). To avoid disjunction paradoxes, 'H→H' can be read as content inclusion, in the sense of Yablo's *Aboutness* (2014).

We don't need the troublesome (2), because if NP is risky, i.e. a priori improbable, (2') already makes the truth of H extremely likely. In fact, the rate of false hypotheses entailing novel improbable consequences is so small that hitting one of them (without using those consequences) would be a miraculous coincidence.

On the opposite, *all* true hypotheses have true consequences, and those sufficiently fecund have true *novel* consequences. Granted, true (and fecund) hypotheses are much fewer than false ones; however, they are not found by chance, but on purpose and through a reliable method. Therefore, if H fulfills (1) and (2'), most probably H is true, and any alternative H* is false.
However, checking whether H fulfills (2') is not a merely logical task: at any given time some *logically* possible weakenings of H may be overlooked because considered physically impossible in the light of certain (possibly unconscious) background presuppositions. For instance, the presupposition that waves can only propagate in a material medium prevents from realizing that the hypothesis of aether is not essential, for it can be weakened to the idea of field.

In general, therefore, neither what is essential nor what is inessential (Vickers, *Synthese* 2016) can be distinguished prospectively, as hoped by Votsis (*Philosophy of Science* 2011) and Peters *Philosophy of Science* 2014). This explains why we cannot foretell which components of current theories will be preserved in future theories and which won't. Yet, by the above reasoning, the at least partial truth of H can be acknowledged independently of its being preserved today; instead *if* and *when* H is subsequently refuted, it also appears that H was inessential; this is seen retrospectively, but independently of its refutation. Hence, *pace* Stanford (*Exceeding our grasp*, 2006; *Metascience* 2009) the selective realist defense against Laudan's meta-modus tollens is not circular.

14. "Molecular Bonding and the Development of Coordination Theory"

Myron Penner (Trinity Western University) and **Amanda J. Nichols** (Oklahoma Christian University)

According to scientific realists, mature and predictively successful theories provide a strong epistemic basis for thinking that such theories are approximately true. However, proponents of the “pessimistic induction” observe that many theories once regarded as well-confirmed and predictively successful were replaced with successor theories, and claim this undermines the epistemic confidence we should have in the approximate truth of current science. Selective scientific realists in turn argue that if one can show that the predictive success of some rejected theory *T* is a function of theoretical claims consistent with current science, then *T*’s failure doesn’t undermine the claim that current successful theories are approximately true. As such, Selective Scientific Realism (SSR) can be tested through historical examples. Showing that the predictive success of a failed theory is the result of theoretical features later rejected provides a counterexample to SSR. Conversely, SSR is supported if its explanation of the predictive success of failed theories is able to handle a wide array of historical cases.

In what follows, we look at theoretical advances in understanding molecular structures at the turn of the twentieth century which resulted from the Jorgensen-Blomstrand/Werner Debate about cobalt complexes. Jorgensen and Blomstrand proposed structures of cobalt complexes that utilised the more developed bonding principles of organic chemistry and reflected the prevailing model of how many molecules, atoms, or ions can be attached to a metal atom. For example, in organic chemistry, the typical molecular structure is a chain of carbon atoms. Moreover, a cobalt atom would have been assumed to have three attachments based on the best available experimental data.

Both Jorgensen/Blomstrand chain theory and Werner’s coordination theory which ultimately replaced it, make predictions about the number of ions that will be dissociated when the molecule undergoes a precipitation reaction. While chain theory makes correct predictions in many cases, Werner’s coordination theory correctly predicted the number of ions in cases where chain theory failed. If the predictive success of chain theory depended on features that were later rejected in coordination theory, we’d have a case which undermines SSR’s explanation of truth transfer between failed theories and their successors. Conversely, the rise of coordination theory supports SSR if the features of chain theory that resulted in its predictive successes were also a part of coordination theory. It turns out the latter is the case.

Chain theory and coordination theory both follow the bonding rule that the dissociation of chloride ions will occur when they are not directly bonded to the metal atom. When this rule is applied to an assumed chain-like structure, it will make predictions that happen to be correct in some cases but not in others. However, freed from the constraint of thinking about cobalt complexes in terms of chain-like structures, applying the dissociation rule in Werner’s coordination theory resulted in correct predictions in cases where chain theory failed. Thus, the feature that resulted in correct predictions in chain theory--the bonding rule of dissociation of chlorides--is a part of the coordination theory that replaced it.

Programme

Saturday, 5th August 2017		
3.15 pm – 3.50 pm	Registration and Reception	
3.50 pm – 4.50 pm	Welcome and Opening Talk by **Timothy D. Lyons** [G83]	
4.55 pm – 5.45 pm	1. "Scientific Realism and the Justification of Induction" **J. Brian Pitts** (University of Cambridge) [G83]	2. "Scientific Realism, QM, and History of Science" **Juha Saatsi** (University of Leeds) [G85]
5.45 pm – 6.00 pm	Tea Break (Scarborough Cafe)	
6.00 pm – 7.15 pm	Plenary 1. "The Significance of Quotidian Truth" **Hasok Chang** (University of Cambridge) [G83]	
Sunday, 6th August 2017		
9.30 am – 10.20 am	3. "Local Approaches to the Scientific Realism Debate: Theoretical or Experimental?" **Jonathan Hricko** (National Yang-Ming University) and **Ruey-Lin Chen** (National Chung Cheng University) [G83]	4. "Defending Selective Realism via Current Science: The Renormalization Group in High Energy Physics" **James Fraser** (University of Leeds) [G85]
10.25 am – 11.15 am	5. "How Philosophy could Save Science" **Ludwig Fahrbach** (Heinrich-Heine-Universität Düsseldorf) [G83]	6. "Gaining Access to Atomic and Molecular Structure" **Teru Miyake** (Nanyang Technological University) and **George Smith** (Tufts University) [G85]
11.15 am – 11.40 am	Tea Break (Scarborough Cafe)	
11.40 am – 12.55 pm	Plenary 2. "Scientific Realism Again" **James Ladyman** (University of Bristol) [G83]	
12.55 pm – 2.05 pm	Lunch (Scarborough Cafe)	
2.05 pm – 3.20 pm	Plenary 3. "How to be Realist in a Selective (But Not Cherry-picking) Way" **Michela Massimi** (University of Edinburgh)	
3.25 pm – 4.15 pm	7. "Causal Knowledge as a Resource for Selective Scientific Realism" **Matthias Egg** (University of	8. "The Argument from Choice: why theoretical virtues better be truth-conducive" **Samuel Schindler** (Aarhus

	Bern) [G83]	University) [G85]
4.20 pm – 5.10 pm	9. “Scientific Realism and Economics” **Jennifer Jhun** (Lake Forest College) [G83]	10. “Historically Probing the Scope of Structural Realism: The Domain of Special Relativity” **Jan Potters** (University of Antwerp) [G85]
5.10 pm – 5.35 pm	Tea Break (Scarborough Cafe)	
5.35 pm – 6.50 pm	Plenary 4: "Is Structural Realism Really a Version of Selective Realism?" **John Worrall** (London School of Economics) [G83]	
7.30 pm	Conference Dinner	
Monday, 7th August 2017		
9.30 am – 10.20 am	11. “Making Content Selectivism the Best Realist Game in Town” **Alberto Cordero** (CUNY) [G83]	12. “Robustness-based Entity Realism” **Markus Eronen** (KU Leuven) [G85]
10.25 am – 11.15 am	13. “An Alternative Definition of Essentiality for Selective Realism” **Mario Alai** (University of Urbino) [G83]	14. “Molecular Bonding and the Development of Coordination Theory” **Myron Penner** (Trinity Western University) and **Amanda J. Nichols** (Oklahoma Christian University) [G85]
11.15 am – 12:55 pm	Lunch (Scarborough Cafe)	
12.55 pm – 2.10 pm	Plenary 5. “A Difference That Makes a Difference: Stein on Realism, Instrumentalism, and Intellectually Nourishing Snacks” **P. Kyle Stanford** (University of California, Irvine) [G83]	
2.10 pm – 2.40 pm	Tea Break (Scarborough Cafe)	
2.40 pm – 3.10 pm	Concluding Address by **Peter Vickers** [G83]	

How to Get to Durham

By road

Durham city centre is only two miles from the A1(M). Leave the motorway at Junction 62 on the A690 Durham - Sunderland road and follow signs to Durham City Centre. Durham is 264 miles from London, 187 miles from Birmingham, 125 miles from Edinburgh and 67 miles from York.

There are several express coach services daily from most major cities. Durham is well served by both regional express services and the local bus network. From the city bus station - a short walk from the railway station - a bus service runs every 15 minutes past the Colleges on South Road.

By rail

60 InterCity trains from most major centres in the UK call at Durham daily including 14 trains from London. The National Express high-speed service takes under 3 hours from London King's Cross on the main East Coast line. First Transpennine Express offers frequent links to Manchester, Sheffield and Leeds, while Cross Country links Durham directly with Scotland, the Midlands, and the South West. Durham is just over 3 hours from Birmingham, 2½ hours from Manchester, 1½ hours from Edinburgh and 45 minutes from York.

A taxi will take you from the station to any College in about 5 minutes and you can walk to the city centre in 10 minutes.

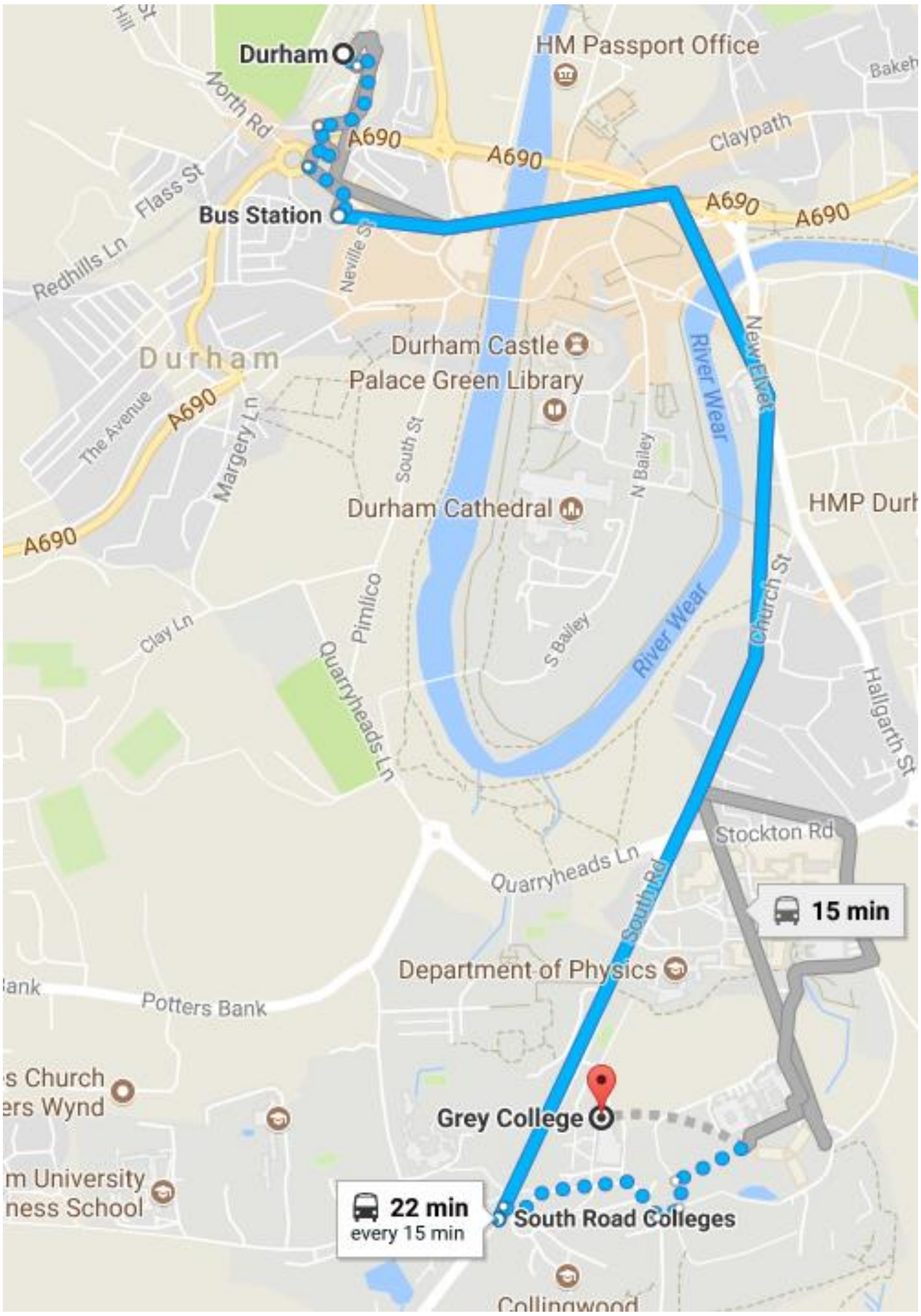
By air

Durham is 30 minutes' drive from Newcastle Airport and about 40 minutes from Durham Tees Valley. Both have regular domestic and international flights. Durham is linked to Newcastle Airport by rail and metro. Travellers into Durham Tees Valley can take advantage of the free Sky Express bus service that links the airport to Darlington railway station, with regular connections to Durham.

College Accommodation

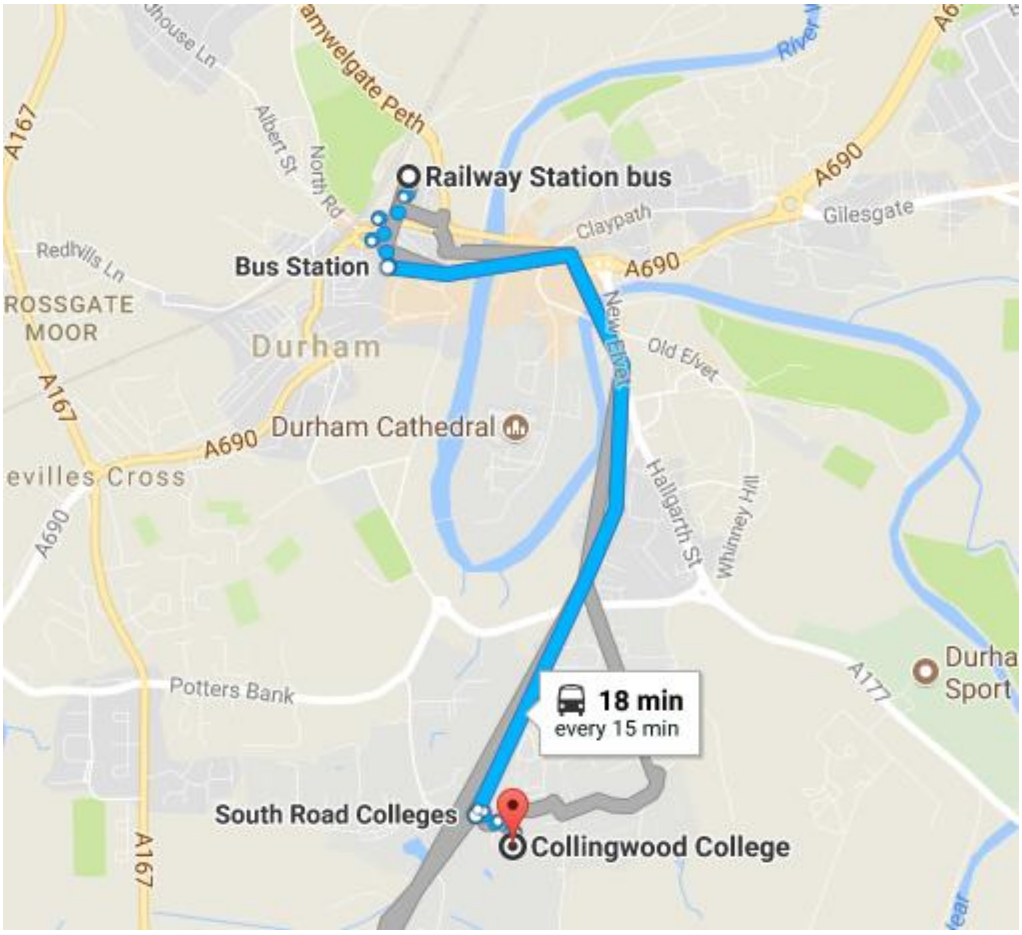
Grey College

Telephone: + 44 191 334 5900
Address: South Road, Durham, DH1 3LG
Directions: Take a No.6 bus from Durham Bus Station and get off at University Science Park. Alternatively, a 30-minute walk from city centre.
NB. The College reception is run from 8am to 5pm by receptionists and at all other hours is manned by the College porters, who can be contacted on number written beside the shutter.



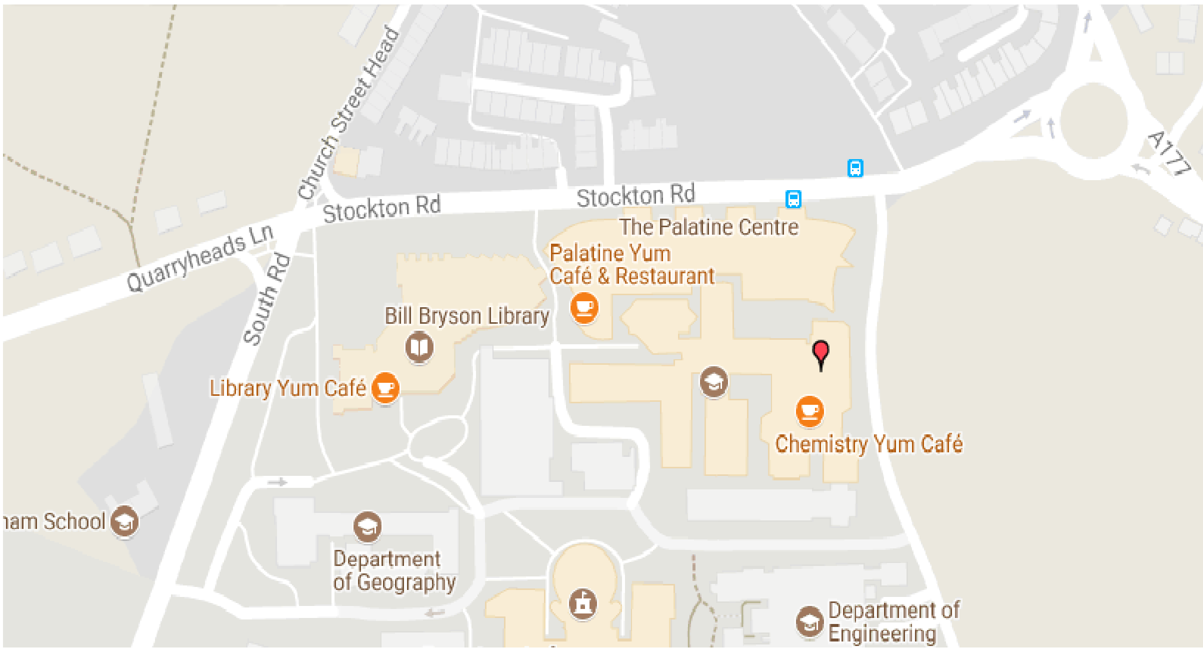
Collingwood College

Telephone: + 44 191 334 5000
Address: South Road, Durham, DH1 3LT
Directions: Take a No.6 bus from Durham Bus Station and get off at University Science Park. Alternatively, a 33-minute walk from city centre.

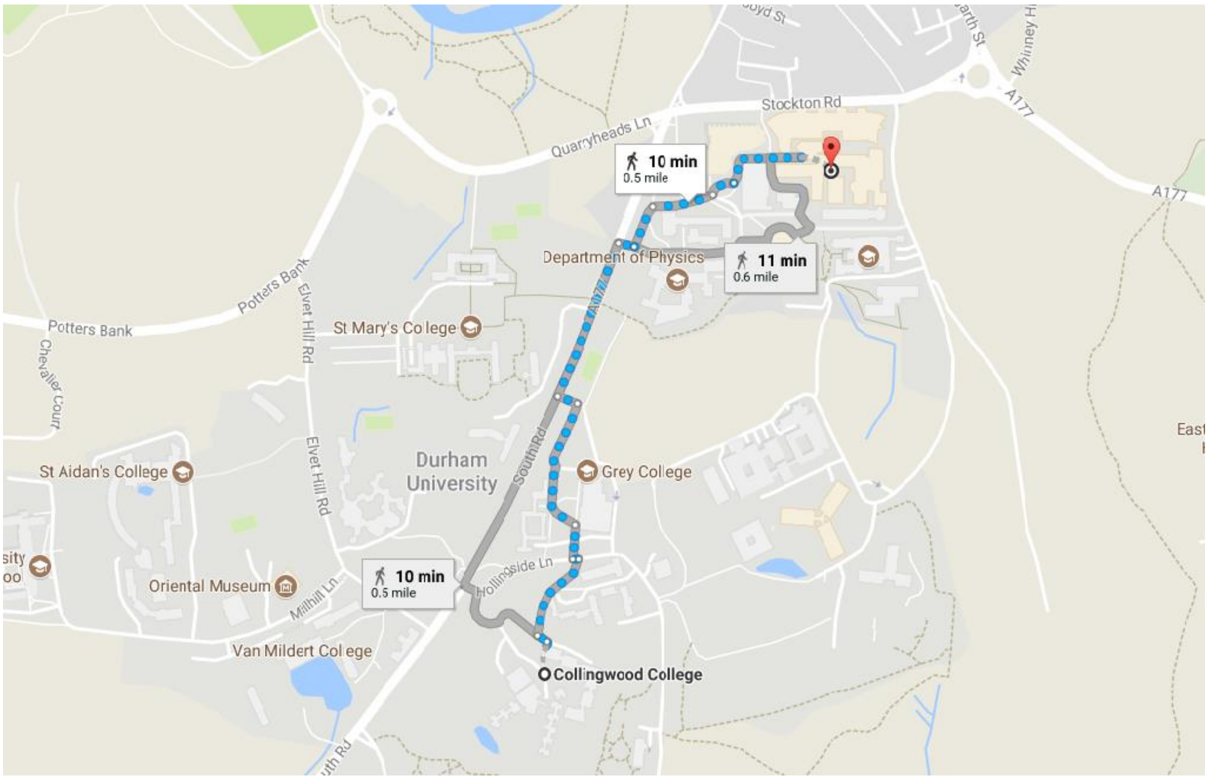


Conference Venue and Maps

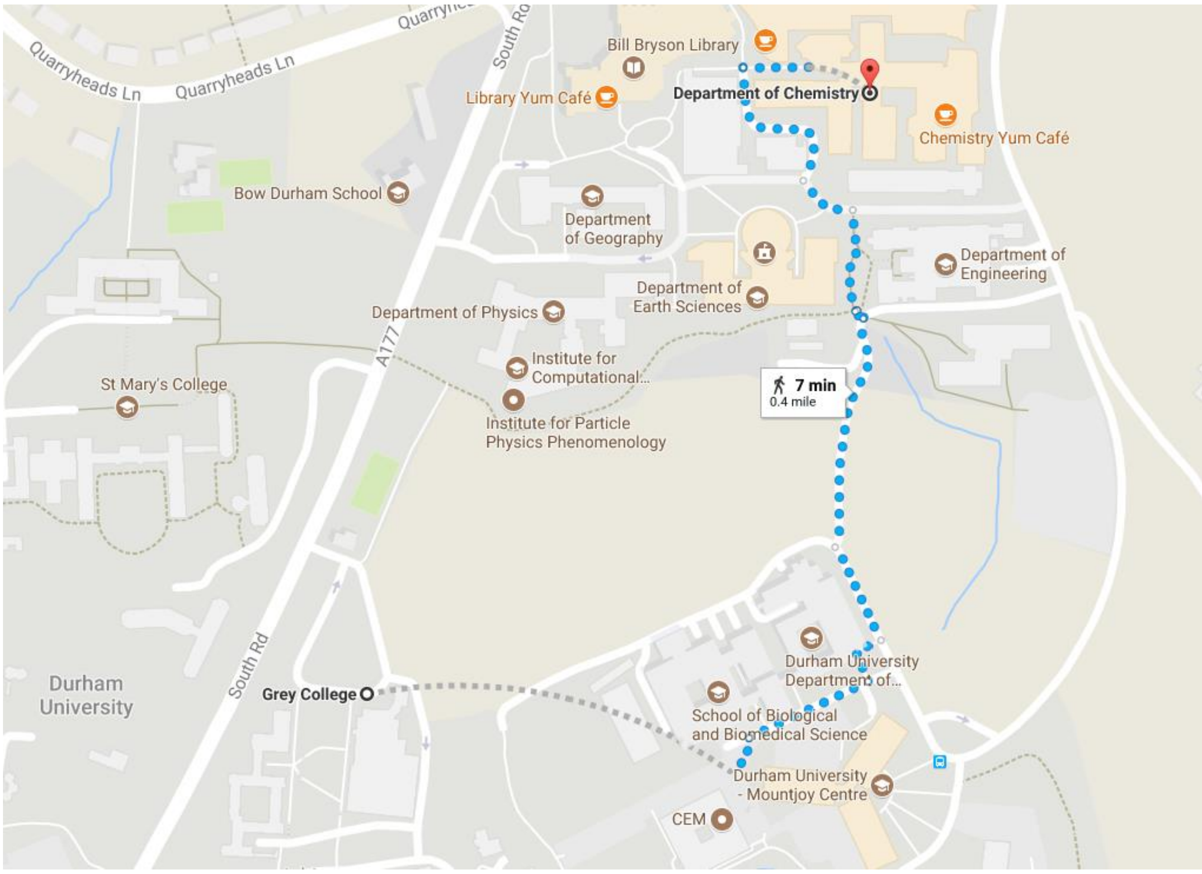
All the talks will take place in the Chemistry building.



Walking directions from Collingwood College to Chemistry building (approximately 10 minutes)



Walking directions from Grey College to Chemistry building (approximately 7 minutes)



Wi-Fi

To connect to wifi in the conference venue, simply follow the steps below:

1. Switch on your smartphone, tablet or other Wi-Fi device and check that Wi-Fi is enabled.

2. Select 'TheCloud@Durham' from the available network list

3. Open your Internet browser - 'TheCloud' landing page below will appear. Click 'Get Online'. If the web page does not appear, refresh the page or type https://service.thecloud.net/service-platform/login/ in the address bar and refresh.

4. You will then see the service selection screen. Select ‘The Cloud Wi-Fi'.

5. Once this is done you can either login with an existing 'TheCloud' account, or click on the 'Create Account' button to register for a free account.

6. Once you have logged in or registered you will be able to access the Internet using 'TheCloud@Durham'.

Conference Dinner

The conference dinner will be held at Bill's Durham, at 7.30 pm on Sunday 6th August. We will meet at the entrance of the Chemistry building at 7.10 pm and walk to the restaurant.