framework ("a set of variables and the relations among them", according to the glossary), from which one could have built his theory that light exists in the form of discrete units, quanta (today known as photons). Consequently the theory was immediately rejected; it was only reluctantly accepted as it solved the problem of explaining the photoelectric effect.¹

Surely, this is a much more typical situation, in which a new theory is likely to emerge. The sceptical response from large parts of the scientific community is not unusual either. A new theory is more similar to a sudden leap out of a persistent and controversial research problem, than to the building of a house brick by brick. More like a frog leaping out of the pond.

Applying this to population health we should expect that theoretical development is more likely to come from research areas where we have controversy about specific issues of substance, rather than from controversy between theoretical schools of thought in general. For instance, does sharp income inequality in a country contribute to increased levels of circulatory mortality in that country? Around this issue, economists, epidemiologists, historians, psychologists, and sociologists continue to argue. By throwing oneself into the argument with empirical data, or with wild ideas, or with systematic reviews, one is likely to promote theory about social inequality and health. It seems less likely that all good researchers will first unite around a framework and then build a theory together. Yet one crucial empirical study (or more likely several

crucial studies) could shift the whole discussion towards consensus. Observation (a term not found in the glossary) and theory develop together.

I agree that the slow process of repeating studies, correcting previous mistakes, designing new studies, collecting evidence, adding detail, changing minor aspects, and refining concepts is a bit more like the building of a house; and certainly those processes are equally important in the long run. Thus there is a place for orderly brick by brick work to advance theoretical problems. But here also, one is most likely to advance if one focuses on anomalies, inconsistencies, and inherent conflicts in what is believed to be the known, and how this is conceptualised.

(2) For many years now, the social sciences have been plagued by a large amount of quasi-philosophical jargon of doubtful value. The most embarrassing example for us who have a social science background was revealed by Sokal² who managed to publish a faked nonsense-theoretical paper. Obscure language from social science sources played a key part in his successful attempt. Sometimes the approach to theory in the social sciences (and not only there) has resembled the building of churches, or empires, focusing on advancing the teaching and language of certain leading lights, rather than on the testing of their ideas against empirical evidence.

The formation of schools of thought is perhaps inevitable; but they often form around a mixture of ideological and scientific arguments. Labelling of one's own school of thought with a fancy name and that of the opponents with a less

fancy name (the most popular one for your opponent is "positivist") is therefore a bad tradition, which we should try to avoid. It does not add to any substantial argument, rather the opposite in my view. It is far from clear from this guide and glossary why we should be interested in "postpositivist theory building" rather than any other view on theory. Terms like observation, evidence, and causality are all missing from the glossary, perhaps this is significant?

My own view is that a theory of population health will depend on researchers giving up their disciplinary narrowness. In the words of Mervyn and Ezra Susser we should try to integrate "causal pathways at the societal level....with pathogenesis and causality at the molecular level."³ Not an easy task, but a very necessary one.

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Postpositivist theory

Theory building on the high seas of population health: Love Boat, Mutiny on the Bounty, or Poseidon Adventure?

Richard M Carpiano, Dorothy M Daley

The complexity of health is a vast, seductive ocean that beckons us—challenging us to explore, navigate, and often battle against waves of ideas—within ourselves and among others.

We thank the three authors for their commentaries on our "Guide and Glossary." We hoped that our article would stimulate discussion about the importance of theory and theory building for population

health, but to be honest, we had no idea how the paper would be received. You can imagine, then, our excitement at being asked by the editors to reply to the three interesting commentaries published herein.

As we point out—and Dunn reiterates in his commentary—theory is a topic that regretfully, receives little attention in population health. We have been pleased to see that others share similarly strong opinions regarding the importance of theory. This is evident in all three commentaries. It was also abundantly evident in the review process for our article. We engaged in a lively debate with reviewers—and those exchanges (which were longer than the original manuscript) reinforced our beliefs that population health research could benefit from a more public discussion regarding the role of theory in research.

In this essay, we respond to points raised by the commentators as they pertain both to our "Guide and Glossary" and, more broadly, to facilitating theoretically driven research in population health. However, before addressing particular issues, we want

to reiterate the specific aims of our “Guide and Glossary,” as each commentary alludes to some misunderstandings about our motivations, arguments, and rationale.

Our article has two basic aims. Firstly, we contend that researchers should use theory explicitly—rather than implicitly. Our focus on the explicit use of theory is guided by our shared belief that clearly communicating one’s theoretical approach will more effectively contribute to and advance our shared understanding of population health and its determinants (regardless of one’s particular theoretical approach).

Secondly, we wanted to provide a set of tools for researchers to use in¹ evaluating existing theories and² constructing new theories. The article is not prescriptive (nor was it intended to be). Rather, it is meant to provide researchers with tools that they can use to evaluate the relative merits of differing theoretical lenses and the models drawn from these theories.

With these points reiterated, we turn to the issues raised by each author (and, in two of three cases, the questions posed in their titles). The ordering is based on the breadth of focus of each commentary. Our reply is simultaneously light hearted and serious. Finally, rather than simply responding to each author’s concerns, we also use this forum to identify fruitful areas for continued debate and dialogue.

Q: WHOSE THEORY IS IT ANYWAY? A: LIKE CAKE AT A PARTY, IT DOESN’T MATTER TO US—WE’RE JUST GLAD IF IT IS THERE

We commend Popay for raising two important issues: (1) the contribution of qualitative research in relation to theory building and (2) the critical need to consider alternative forms of knowledge with a particular emphasis on addressing the conflict between lay and expert theories.

Popay contends that our article dismisses qualitative research as having no relevance or theoretical contribution to population health research. This is inaccurate; nowhere in our article do we state or suggest that quantitative research is the only viable approach for contributing to our knowledge base. Like Popay, we strongly believe that qualitative research methods must be included in the population health “toolkit.” We recognise the egregious fallacy made by many researchers that qualitative methods have no place in studying population health. Therefore, in the glossary section, we explicitly made the argument for qualitative and quantitative methods in our

definition of postpositivism. We state that a postpositivist approach necessitates “the use of quantitative and qualitative methods” for “making claims and then refining or abandoning some of them... for more strongly warranted claims.” Our use of italics in the original article was intentional and based on our strong belief that population health benefits from methodological pluralism. Finally, the list of sources used in the original article indicate that we relied upon a diversity of material—some of it directly addressing qualitative research (for example, King *et al*’s¹ text on scientific inference in qualitative research).

Theory, and being explicit about theoretical approaches, is crucial regardless of the methodological tools one applies. As Popay notes, “theory is the core element of qualitative research, it derives from it, is tested in it, and... is the mechanism through which qualitative research can claim more general relevance.” Nevertheless, Popay’s discussion on qualitative methods conflates qualitative research with strongly theoretical research. Ideally, qualitative research should have a strong theoretical basis; however, in practice this is not always the case. Like under-theorised quantitative studies, even qualitative research can produce findings that are extremely difficult to interpret and generalise—particularly in terms of how they inform current or new theoretical ideas to be tested. Qualitative research, like quantitative research, should be critically examined for its theoretical rigour.

In her second focus, Popay asserts that we implied legitimate or valid knowledge is limited to scientific “experts.” Certainly, our discussion focused on theory construction by population health researchers, but lay knowledge is not trivial, as any researcher who has read a single illness narrative can attest. At base, our paper contends that researchers should be explicit about their theoretical lens. We make this argument in light of the fact that population health is an interdisciplinary field—and being explicit will serve to increase meaningful communication between disciplines. While our discussion does not address the lay public as part of this communication process—the logic of our argument holds true. If researchers are explicit in the theories that they are relying upon to guide their work, we expect that some conflict between lay and expert knowledge could be reduced.

We acknowledge that performing theoretically explicit research will not be a panacea for all of the issues faced by population health researchers.

However, in the long run, we contend that doing this type of work will move us toward a greater understanding of population health.

Of course, many factors will be important in addressing the lay/expert conflict that Popay discusses, such as community based participatory approaches, ethnographic methods, and meaningful knowledge exchange. However, a detailed discussion of this is beyond the scope of our “Guide and Glossary.”

Q: WHERE DOES NEW THEORY COME FROM? A: TO US, WHERE IT’S FROM DOES NOT MATTER. WE ONLY CARE IF THE THEORY CAN TELL US WHERE IT IS GOING

While Popay challenges the role of qualitative research and alternative forms of knowledge, Vågerö asks more fundamental questions about the aetiology of new theory and frameworks—and in doing so, raises a variety of interesting issues. Vågerö’s title question is an important one. However, our “Guide and Glossary” intentionally does not attempt to provide an answer. (Incidentally, it strikes us as interesting that Popay’s commentary speaks indirectly to Vågerö’s question, as she suggests that qualitative research often provides fertile ground for understanding complex relations.)

Vågerö contends that theory building is more likely to be the result of a chaotic process rather than incremental additions to our existing knowledge. We disagree. Certainly, new theoretical developments emerge that create significant change—what Kuhn² would label as a “paradigm shift”. However, realistically, such important theoretical developments are not everyday occurrences, particularly in population health. Most theoretical developments and refutations, such as the kind that you would see reading several years of *JECH* issues, are far more likely to represent incremental, but *vitaly important*, advancements for a particular subject or the study of health overall. Essentially, to borrow from astronaut Neil Armstrong, we think “small steps” as compared with “giant leaps” of paradigm shifts are more likely to be the norm.

Whether theory building entails an orderly or chaotic process is tangential to the main points of our article. As we mentioned earlier, we had no intentions to be prescriptive about how theory should be built. There is no one source for theories or frameworks, nor, in our opinion, should there be. Some theories may come from orderly processes, while others may represent paradigm shifts. Our aim was not to discuss their origins

or genesis, but rather to emphasise the need for theory in research, discuss how to evaluate the utility of a theory, and, finally, provide some common conventions for communicating theoretical approaches (for example, our model building suggestions). The qualities that one can use to evaluate a theory hold regardless of the theory's aetiology; this includes examining a theory's logic, causal relation, ability to be falsified, scope, and productivity.

Vågerö also assumes that our "Guide and Glossary" is a call for population health researchers to unite around one theory. At no point in the article do we suggest that researchers must or should use the *same* theory (or even the same theory building approach). Such an approach arguably constitutes ideology as compared with scientific inquiry. Rather, our point is that population health research will benefit from *explicit* use of theory. Being explicit and clear about the relations one expects to see and why one expects to see them can only help promote systematisation of knowledge and cross-disciplinary learning. In fact, the idea of a population health grand theory seems to us absurd and restrictive. In the article, we suggest that it is ideal for a researcher to use competing theories (hence, our inclusion of the "method of multiple hypotheses" in the glossary). Whether the topic is circulatory mortality and income inequality or neighbourhood effects on health disparities, we contend that explicitly relying upon theory (whether developed orderly or chaotically) can facilitate effective communication between economists, epidemiologists, historians, sociologists, and others.

In the second part of his commentary, Vågerö focuses on jargon and schools of thought. Regarding terminology, Vågerö laments that social science theory is plagued by jargon (as does Popay to a certain extent). We partially agree, but also recognise that every discipline/field has jargon and no one to our knowledge criticises modern medicine for terms like "CABG" or "MRI" or even physics for terms like "string theory"—why point the finger at just the social sciences? But, again, we re-emphasise this is why it is particularly important to be explicit about one's theoretical lens or rationale. It provides others with an additional means to understand and evaluate your research.

Vågerö expresses concern that "schools of thought" or even theoretical camps are not useful in advancing knowledge, but instead create undesirable divisions. Again, we disagree. If theory building could become as emphasised in population health as

Dunn suggests it should be, then it is possible that theoretical schools of thought could begin to emerge. However, at this stage of infancy for population health anticipating such a development is premature.

Vågerö notes that certain terms, such as observation, evidence, and causality are not in the glossary. These are purposeful omissions on our part. Simply put, there is an editorial limit to how many terms we could include in the glossary and, thus, we decided to exclude terms that are covered in other glossaries (for example, Susser's causality glossary,³ and Rychetnik *et al*'s glossary for evidence based public health⁴).

Finally, and perhaps most importantly, Vågerö challenges us by asking "why [he and other readers] should be interested in "postpositivist theory-building" rather than any other view on theory?" This question is part of a larger issue: What role does the philosophy of science play in scientific research? As researchers, we should be not only interested in improving the application of theory in our research activities, but also in understanding the core philosophical assumptions that underpin certain practices we take for granted in conducting research. As detailed in our glossary, postpositivism is an approach that substantially differs from both positivism and constructivism—in terms of ontology, epistemology, and methodology. We do not argue that postpositivism is any better or worse than other approaches; it is one approach that has been accepted and widely used for several decades by a significant portion of the scientific community. Our article outlines key elements of this approach for scientific inquiry. In keeping with other glossary topics, like causality, we believe that the subject of theory and theory building (or postpositivism for that matter) is not one that we have to "sell" to the readers. Rather we highlight theory and theory building as an issue and offer suggestions based on an approach that underpins our training and research agendas (that is, postpositivism).

The important question we struggle with is not "Why should we be interested in postpositivist theory building?" but rather "Why are so many researchers *uninterested* in theory building?" This question brought the two of us together to write our article and, essentially, gave rise to this very discussion. Vågerö's commentary shows that he certainly finds theory interesting—or at least interesting enough to write a commentary. That's good enough for us.

SPEAKING THEORETICALLY ABOUT POPULATION HEALTH: OF APPLES, ORANGES, AND WHY CHEWING ON FRUIT IS HEALTHY

The final commentary by Dunn uses our initial effort as a "springboard" to discuss broader definitions of theory and explanation. We applaud his extension of this discussion because throughout our "Guide and Glossary" we acknowledge that we are presenting only one way to approach theory and theory building and, thus, genuinely welcome a discussion on alternative approaches to this topic. However, we take issue with some of the points he raises about our article and, more broadly, with some points on population health and theory overall.

We agree with Dunn's threefold "explanatory project" of population health that includes linking cells to society, neurons to neighbourhoods to nation states, and the cradle to the grave. However, we disagree with his definition of population health ("a broad and complex field that must explain the systematic, differential distribution of health status by socioeconomic position"). This definition outlines how social epidemiology fits into the population health enterprise, but discounts the necessity of numerous population health researchers from other fields and disciplines that contribute to the study of both the distribution and determinants of the health in populations.

Dunn's definition however, raises a critical issue: the need to understand the ways in which different disciplines shape the field of population health. Years ago, a former colleague of ours lamented the dominance of social epidemiology in population health because, although an important group to include, in his opinion, their high level of representation tended to define legitimate and illegitimate population health research. While we view population health far more broadly than Dunn, we nevertheless believe that continued debate focused on the content and boundaries of population health is quite healthy and absolutely necessary for an emerging interdisciplinary endeavour.

Dunn advocates using alternative approaches to theory and explanation, with an emphasis on "examined conceptualisation." This is exactly the type of dialogue and discussion we were hoping to provoke with the original article. However, the ways in which he uses this term to show its utility are puzzling. Consequently, we are left with several questions about examined conceptualisation and its contributions to population health.

Using our example of neighbourhood food stores, provision of products, and obesity, Dunn tries to illustrate ways in which this work might move toward examined conceptualisation. However, it is unclear how incorporating urban planning and geography raises this example to the definition of “examined conceptualisation” or, conversely, relegates our example to simply (as Dunn describes it) “theory as a hypothesis or a functional explanation of why variable A is associated with variable B.”

Instead, Dunn’s example neatly shows the power of what Vågerö would call the “brick by brick” approach to theory and research. In essence, this example seems to show how research can (and should) be extended to incorporate other (in this case, more macro-level) hypothesised determinants of obesity patterns that might be found by neighbourhood areas. We remain unconvinced that exploring these macro-level issues constitutes something more than, to use Dunn’s phrasing, “variable A is associated with variable B.”

Turning to the influential work of Evans and Stoddard,⁵ Dunn perceives our article as critical of their framework and its contribution to population health. By contrast, we share Dunn’s appreciation for Evans and Stoddard’s framework. However, we fail to see how this framework constitutes “examined conceptualisation.” To us, the “furnace and thermostat” metaphor is another way of communicating “variable A is associated with variable B”—or perhaps more appropriately, “the degree of association between variable A and variable B is moderated by variable C.” To be clear, we are not saying that it could not fit an “examined conceptualisation” typology—but, as it stands now, Dunn’s description is not persuasive.

Finally, Dunn argues that the Evans and Stoddard framework “quite rightly defies straightforward empirical testing, and [is] better judged for its internal consistency, cogency, and plausibility for explaining real-world phenomenon, and revealing previously unseen and unarticulated aspects of that phenomenon.” If this framework cannot be empirically tested, then how can one

judge whether or not it is accurate in explaining real world phenomena? A framework or theory that cannot be tested, either in total or in terms of its substantive parts yields scant real world information. The importance or relevance of a theory is very much dependent upon both its level of abstraction and its ability to be tested. A theory that is too abstract can encompass so much of anything that the theory itself sheds no light on critical relations. Theories that cannot be tested and, as we would contend, falsified, may contribute more to ideology than to scientific inquiry. The most prominent example that comes to mind is the recent conflict in the USA concerning whether or not to include intelligent design in science curriculum as an alternative to evolutionary theory in teaching about the origins of human life.

Overall, we agree with Dunn (and, incidentally, Popay) regarding the need for a “pluralistic vision of theory.” Theory as “examined conceptualisation” is certainly valuable, at least in terms of how Dunn defines it compared with how he describes it in his examples. However, regardless of the type of theory, that is, whether it is a, b, c, or perhaps even an unspecified “d”, the same issues hold—it must be testable, regardless of the method used.

A CALL FOR FURTHER DISCUSSION: AFTER ALL, WE ARE IN THE SAME BOAT TOGETHER (AT LEAST THEORETICALLY)

In closing, we wish to reiterate that theory is the cornerstone of our population health enterprise. In light of the arguments presented in this public dialogue, we wish to end with a quote by Leonardo da Vinci: “He who loves practice without theory is like the sailor who boards ship without a rudder and compass and never knows where he may cast.” The complexity of health is a vast, seductive ocean that beckons us—challenging us to explore, navigate, and often battle against waves of ideas—within ourselves and among others. While the theoretical “rudders and compasses” used in population health will (and should) vary in their construction, application,

and utility, one thing is certain: without careful consideration and continuous promotion of theory (broadly writ), the field of population health runs the risk of not only sailing aimlessly, but even dying at sea. Essentially, the tools to construct theory are too important for any population health expedition to leave behind on shore. A long voyage lies ahead—let us set sail prepared. All aboard, anchors aweigh, and “damn the torpedoes...”

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