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What matters, when, for whom? three questions to guide population health scholarship

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We are at an inflection point in the history of public health scholarship. The past decade has seen a steady increase in the adoption of population health as a conceptual lens, emerging as the science that underlies the practice of public health.¹ Attending this increase in interest have been books² and papers³ that aspire to create frameworks that guide the study of population health science. We consider this all positive, seeing the rise of population health science as freeing, allowing the evolution of a scholarship of population health that can engage with ideas, unencumbered by the pragmatic needs of public health practice.

As might be expected, however, this evolution also occasions challenges. The growth of scholarship in population health may diverge too widely from the practice of public health. Freedom from pragmatic exigency runs the risk of leading to scholarship that is unmoored from the needs of the public health enterprise. In this essay, we aim to distil some of our own scholarship around population health science,^{4–6} informed by others who have come before us⁷ or are currently writing in the field,⁸ to highlight three core questions that we think can help focus contemporary population health science. We argue that the three questions emerge from an understanding of the fundamentals of population health but can, and perhaps should, serve as guideposts for emerging scholarship. We illustrate the import of each of these questions with examples of relevance to injury prevention, aiming both to concretise our thoughts and to have these questions challenge and provoke scholarship and action in this area.

WHAT MATTERS?

We suggest that the first question of fundamental interest to anyone engaged in population health is deceptively simple: what matters? We have previously commented on what might matter most,⁵ but here we take a step even further back, asking foundationally what matters at all. Insofar as population health is concerned with understanding the causes of health within and across populations,⁴ scholarship in the field must use the (mostly) quantitative

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approaches at its disposal to understand the causes of health, providing then a guidepost to suggest where the most effective interventions might lie. Foundational quantitative disciplines— principally modern epidemiology and biostatistics—offer the tools to consider what these causes may be, even as they, appropriately enough, grapple with argument about how we may consider causal architectures⁹ or about which methods are best suited to ask causal questions.¹⁰ In addition, qualitative and systems science methodologies in population health add useful approaches to our armamentarium that can help address causal architectures. The science then provides us with insights about the factors that might create health within populations, across populations and across time. The challenge, however, rests with figuring out how to stratify these observations, how to sift what does matter and what does not. We suggest that there may be three useful signposts that helps us sort what matters.

First, and perhaps most obviously, observations about the health of populations that can lead to clearly defined action that improves health of these same populations matter. By way of example, we know that higher speed limits are associated with more motor vehicle-related death.¹¹ Driving at higher speed is a cause that shapes health across populations. A clear intervention that improves population health is well-enforced and well-posted speed limits on roads, which is a public health response to a population health problem (MVCs). A town where the speed limit is 55 mph throughout will have a higher motor vehicle fatality rate than one where the speed limit is 35 mph. These forces can guide pragmatic intervention. A lower speed limit, readily enacted through municipal legislation, can save lives, but what of observations that do not lend themselves to such clear action? We know, for example, that men are more likely than women to drive faster than the speed limit¹² and that religiosity at the state level is associated with likelihood of motorcycle helmet laws.^{13 14} The former might suggest making more of an effort to reinforce driver's training for men. Yet, we know that formal training makes little difference for quality of driving.¹⁵ It is hard to know what to do with the latter observation. Certainly, tackling religious beliefs is not likely an easy or tractable path. Does an observation then matter if it can easily lead to action? Do observations not matter if they do not?

In answer to the latter question, we suggest that, secondarily, population health science that does not point to immediate action can still matter if it points to foundational truths that helps us understand the causal architecture of population health. Recognising gender differences in driving patterns provides hints of gender-patterned education that shapes behaviour across multiple dimensions. Understanding that religious beliefs are associated with helmet laws provides hints of a complex causal architecture, of shared foundational causes likely embedded in deeply held values. These observations help us paint a picture of the complex causal patterns that ultimately shape health in populations and serve the added function of warding us off from overly simplistic interpretations of what shapes health and what we might do to improve it. Recognising that powerful social forces are associated with state-level decisions around adoption of motorcycle helmet laws may suggest that it would be unwise to tackle helmet laws by presenting only data-based arguments and that those interested in promoting the public's health need to consider the values that inform policy decisions. It may be not at all clear how we would go about tackling those values, but a reminder that they are central pieces of the puzzle offers perspective and a tonic to easy simplification that ill-serves action. Investment in science that does not have a clear

actionable path to intervention is not currently a popular approach, given limited resources for a vast array of priorities. We suggest that disinvestment in foundational, classical ecological and risk factor research distances us from our collective goals towards healthier populations.

Third, observations matter if they lay the groundwork for further work that can then lead to action. We now know that rapid acceleration–deceleration is associated with greater likelihood of traumatic brain injury and concussion.¹⁶ This observation explains the cognitive symptoms that are associated with some sports such as football¹⁷ but does little to point to an obvious solution, given how central to the sport such movements are. It does, however—in isolating the causal mechanism that underlies the injury—suggest areas for further inquiry that *could* lead to action. It calls, for example, for research into whether particular rule or equipment changes could minimise rapid acceleration–deceleration, making the sport healthier without substantially reducing fans’ or players’ enjoyment. Many such observations are ones that point to mechanisms, and it is understanding such mechanisms that can point to the course of action that can achieve the desired improved health outcome.

Therefore, population health science that matters should be able to guide efforts that improve the health of the public, can guide us to a foundational understanding of the causal architecture of population health or can be the building block to one of these two. What matters is, however, modified by two additional questions: what matters when? And, what matters for whom?

WHEN?

Any assessment of what matters must take into account time. Time, in this context, represents two ideas: a position during the lifecourse, when insights might illuminate key inflection points where health might be maximally affected, and the temporal time over which the distributions of risk factors and the causes that interact with them may vary in incidence and prevalence.

First, a consideration of what matters cannot be divorced from the lifecourse. Lifecourse models of population health suggest that health states for any individual at any point in time reflect that person’s historical health states and exposures, including the perinatal period, as well as the lifecourse experience of their predecessors, shaping one’s health cross generation.¹⁸ By way of example, let us consider one of the most consistent causes of injury death over the past century—suicide. There have been substantial increase in suicide rates in the US population in recent years¹⁹; efforts to curb these increases and also understand why these increases are occurring are paramount to public health. Suicide is often considered an injury with causes quite proximal to the event; interviews with near-fatal suicide survivors indicate that approximately 24% of suicides are contemplated just 10 min before the attempt.²⁰ However, suicide is also linked very clearly to life- time history of mood disorder, particularly major depression,²¹ which is most likely to onset in adolescence. Therefore, efforts to prevent major depression in early life is likely to influence an individual’s risk of suicide in the future. Further, parental and grandparental depression

increases the risk of offspring depression,²² suggesting as well that prevention of depression in early life may have downstream consequences to future generations. Appositely, points in the lifecourse, within and across generations, may be important high-yield levers that influence health much more than do others. For example, there is a robust argument to be made that early childhood education around safety matters much more to mitigate risky behaviour later in life than does such training for adolescents.

Second, this model also implies that understanding the population distribution of exposures (such as depression) that may influence population rates of outcomes (such as suicide) must also consider these potentially long latency periods as well as how changes in the distribution of risk factors may influence population rates of outcomes in the future. As such, the lifecourse model of health implies that an understanding of population health and efforts to improve population health in the present need both to take into consideration how that health is a function of history and how that history will modify any efforts to influence population health in the future. The time scale along which time matters remains difficult to predict and bedevils efforts at intervention that aspire to results quickly. By way of example, scientific inquiry into the historical drivers of depression prevalence historically, both within and across generations, becomes imperative to understand the causal architecture of suicide rates in the population in the present and a sine qua non of action that aims to minimise suicide rates in future. Therefore, scholarship that matters can be, perhaps must be, future oriented, alert to and patient about the potential consequences of its findings for the future health of populations.

Third, useful science should transcend the immediate and have implications for what may be important well into the future. The observation that lifecourse models imply that the past is important to the future, with respect to the population, aligns with, but is not necessary for, this third observation. One of the challenges embedded in a quest for useful science is a reflexive focus on what may be useful today, sensitive to contemporary concerns. This orientation is eminently understandable. After all, we are motivated by historical patterns of health in populations and by present concerns, both being known and knowable. Future health concerns are much more abstract and marked by uncertainty. Overconcern with the future can be both fanciful and wrong. However, there is good reason to agitate for a future-oriented science and practice. In the context of injury prevention, we know perhaps more than we typically think we do about future patterns. By way of illustration, we know that large-scale disasters will happen in future and that they will result in large numbers of people being affected by trauma and be vulnerable to its psychological consequences. What we do *not* know is specifically when and where these disasters will happen.²³ That should not, however, minimise the contemporary importance of science that can guide the mitigation of these traumatic events, and our certainty, based on ample historical patterns, that mass traumatic events will happen again, and soon, should perhaps raise this science to the forefront, elevating its utility. We also can anticipate some future developments that will have implications for population health. There seems little doubt that autonomous vehicles will supplant some human-driven cars in the near future.²⁴ These vehicles will have implications for motor vehicle injuries for both occupants and pedestrians, and scholarship that can guide their development can have an important role to play in the evolution of what seems certain to be a signal change in transportation over the coming decades.

In sum, time adds a dimension to scholarship and potential action that might matter. Time matters both in the timing of risk factors for injury outcomes within and across generations and also in the shifting prevalence of those risk factors across time and the implications for explaining present distributions of population health outcomes and predicting future distributions of population health outcomes. A future-oriented population health grapples with time, both historical and future, as an added dimension guiding the science we choose to engage in and how we see the impact of that science on the health of populations.

FOR WHOM?

Although the very term population health suggests a concern with the health of whole populations, this elides both the complexity of population health generation and the heterogeneity that underlies the aggregate health of populations. Three core concerns about differences *within* population health inform any assessment of population health scholarship that matters.

First, the health of populations is a product of a set of causes that interact, characterised by reciprocal relationships, feedback loops and phase discontinuities.²⁵ Population health, therefore, occurs within a complex system, and there is utility in considering population health through a complex system lens.²⁶ This also suggests that population health is an emergent property of exposures occurring to individuals within this complex system. By definition, emergent properties are properties that can be seen when one observes a system but that cannot be seen when observing individual components of the system by themselves. A classic example of an emergent system is the flight pattern of a flock of birds, unobservable when one studies individual birds, but readily apparent when we consider the whole population. Traffic patterns represent a comparable example in human populations, as do collective behaviours that lead to group violence. This suggests that scholarship that aims to understand population health needs to rise above the study of individual people within populations, to consider how populations may behave as a whole. This requires a population-based lens and also the adoption of methods that can yield population-level insights. For example, recent work has tackled the question of how spatial racial segregation might influence efforts to mitigate the consequences of violence,²⁷ considering both a cause—racial segregation—that exists only at the population level (ie, any individual does not have a level of segregation; spatial segregation exists only as a group property) and consequences of that cause that can be mitigated by considering populations as a whole. We are far from clarity in population health science about what methods may be best suited to grapple with these questions, but the emergence of books and papers that challenge our established methodological armamentarium hold promise.²⁸

Second, orthogonal, but complementary to the first, population health scholarship must consider the implications of particular observations and actions for particular subgroups within populations and the extent to which subgroup heterogeneity should inform the utility of particular population health observations. Of core concern is the existence of intergroup differences in health. The existence of disparities by race/ethnicity or socioeconomic position has long been documented across a broad range of health indicators.^{29–31} Health inequality is the result of social structured inequity, reflecting historically embedded

structural differences that produce health divides. For example, in the USA, unintentional drug overdose death rates are substantially higher among whites with low levels of completed education and have a much higher burden in certain areas of the USA (eg, West Virginia), driven mainly by wide- spread non-medical use of prescription opioids, the latter shaped by prescription patterns across the country.^{32 33} This overdose death rate is having an enormous impact on the health of affected populations, militating for population health scholarship that prioritises overall opioid use even as it focuses on the highest-risk groups.

Third, and relatedly, population health scholarship must tangle with the differential response to intervention that in and of itself may drive differences in population health. The foundational factors that shape the health of populations can also be responsible for the uptake of particular interventions and their influence on the health of populations. Take, by way of example, efforts to distribute naloxone, an opioid antagonist that is intended to reverse accidental opioid overdose and reduce the risk of death. Such efforts are almost certainly to be more successful within well-resourced institutions, where security personnel can be trained in the use of naloxone, achieving success and rapid reduction in mortality rates quickly.³⁴ Conversely, rural populations are likely to be some of the hardest groups to reach, to effectively deliver naloxone, even as this is the group that is likeliest to benefit from the intervention. This is in some ways a corollary of the inverse care law, first articulated by Julian Tudor Hart, whereby those who need an intervention most are least likely to receive it.³⁵ The net effect on population health is that intervention efforts may well widen intrapopulation health gaps, even as they might improve overall population health. It then becomes a question of values whether we are willing to accept widening intrapopulation differences even while improving population health improvement. There is clearly no right answer to this conundrum, but rather it represents a challenge. Population health scholarship needs to consider carefully who may benefit, and who may not, from particular approaches, and illuminate conversations that can help us engage these questions.

IN CONCLUSION

The rise of population health science represents an opportunity for clarity around how we understand the production of health in populations and how we may use that knowledge to guide action. We suggest here that asking three fairly simple questions: what matters? when? and for whom? may provide a relatively straightforward framework that can help evaluate the utility of our science, towards the generation of ever better and more consequential scholarship. Our thinking is not intended to castigate scholarship that does not neatly fit this bill. Science needs its share of intellectual noodling that leads to dead ends. However, we mean this to help organise our thoughts towards more generative science and action. We are also aware that sometimes science, as it is being conducted, does not have the dispassion to recognise whether it meets any of these three criteria. That is as it should be; science needs unfettered space to grow and evolve. However, we contend that clarity about what matters can both help us sift through what we know and prospectively inform the questions we ask. We illustrated this throughout with examples drawn from injury and injury prevention, aiming to bring these ideas to scholarship around and the practice of injury prevention, although these ideas probably apply equally well across the breadth of population health science. We suggest that practitioners can equally well benefit from thinking about the

principles we highlight here. Asking what matters? when? and for whom? can serve as a litmus test for interventions, a check on internal coherence of the proposed intervention before resources are expended. If a proposed intervention cannot answer these questions, we would argue that it may sit on shaky ground and may benefit from being rethought. Ultimately, we aim these questions both to be as much a useful guide and a challenge to other scientists and practitioners in the field to suggest alternate approaches that can even better inform and shape our thinking going forward.

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