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Environmental determinants of cycling: Not seeing the forest for the trees?

Samuel Nello-Deakin

Department of Geography, Planning and International Development, University of Amsterdam, Postbus 15629, 1001 NC Amsterdam, the Netherlands

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ABSTRACT

In recent years, the volume of studies in the fields of transport and urban planning seeking to identify environmental determinants or correlates of cycling has expanded dramatically. This viewpoint wishes to put forward a provocative argument: namely, that while further research in this area might refine our theoretical understanding of certain issues, it is unlikely to deliver any fundamentally new policy-relevant insights as to what measures need to be taken in order to increase urban cycling rates. At present, the difficulties faced by the vast majority of cities across the world in encouraging cycling are not derived from a lack of theoretical knowledge, but are fundamentally practical and political in nature. From a practical perspective, I argue that we already know *enough* about what needs to be done in order to encourage cycling in the vast majority of urban contexts. The problem with the seemingly endless proliferation of research on the relationship between cycling and environmental characteristics, I suggest, is that it risks giving the impression that there is some fundamental unresolved uncertainty about what is needed to make a city more cycling-friendly, when this is simply not the case. Instead of focusing on cycling itself, I suggest that exploring the phenomenon of traffic evaporation may be a more fruitful way for researchers to advance the cause of urban cycling.

1. How much research is enough?

As a result of the growing interest in promoting cycling in cities across the globe, the volume of studies seeking to identify and assess environmental determinants or correlates of cycling has multiplied dramatically over the past twenty years (Fishman, 2016; Muhs and Clifton, 2016; Pucher and Buehler, 2017). Despite the value of this research, this viewpoint argues that the exponential increase of publications in this area means that we have currently reached a point of saturation in which, at least as far as policy relevance is concerned, we should question the need for further research on this topic.

To begin with a slightly crude comparison, consider the case of academic research on the relationship between tobacco and cancer. It has long been conclusively proven that smoking significantly increases the likelihood of cancer, to the point that no references are needed to back up such a statement. While there may still be some uncertainties regarding the exact odds smokers have of developing throat, lung or pancreatic cancer, the fact is that we already know *enough* about the basic facts to take decisive policy measures in preventing and dissuading the use of tobacco. Further research on the adverse effects of tobacco might be useful in all sorts of ways, but there is no need for more studies seeking to the prove basic conclusion that smoking is associated with cancer: this finding has already been replicated a critical number of times, and there is little value in doing so again and again.

In the case of research on the relationship between cycling and the built environment, however, I want to suggest that this is almost what we seem to be doing; namely, reiterating the same conclusions over and over again. If we look at the aggregate picture, we find that over the course of the twenty years following the publication of the first cross-sectional study establishing a positive correlation between cycling infrastructure and cycling rates (Nelson and Allen, 1997), empirical studies have found time and time again that urban environments with dedicated cycling infrastructure, traffic calming measures and moderate to high urban densities are associated with higher cycling rates (e.g. Cervero et al., 2019; Dill and Carr, 2003; Handy and Xing, 2011; Koohsari et al., 2019; Mertens et al., 2017; Nelson and Allen, 1997; Titze et al., 2008; Zhao, 2013). Although this statement is certainly open to nuances and exceptions, the overall picture which emerges from existing research is a relatively clear one.

Undeniably, many of these studies have been extremely valuable: at their simplest, they have helped build a critical mass of evidence to support cycling policy, and have definitively discredited arguments in favour of vehicular cycling (Reid, 2017). Perhaps even more importantly, they have helped visibilize and legitimize cycling as a serious form of urban transport in the eyes of policy-makers. In addition, their methodological sophistication and geographical scope has clearly increased over time (Muhs and Clifton, 2016). At present (and for some time past), however, I would like to argue that we have reached a point

E-mail address: s.nello@uva.nl.

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of saturation in which further studies in this area are unlikely to deliver any fundamentally new policy-relevant insights. By 2017, conclusions such as those reached by Mertens et al. (2017, p. 35) – "These results suggest that reducing speed limits for motorized vehicles and the provision of more bicycle lanes may be effective interventions to promote cycling in Europe" – are unlikely to have come as a surprise to anyone (pace Mertens et al.). Like in the example of smoking and cancer, the finding that cycling infrastructure and traffic calming measures are conducive to cycling has simply been replicated enough times to definitively prove its validity.

Given the fact that most researchers seem to agree that cycling infrastructure, traffic calming and moderate to high urban densities are conducive to cycling, this begs the question of why we continue to focus so many research efforts on trying to prove this point, with new studies on the topic being published almost every month (e.g. Koohsari et al. (2019) at the time of writing). I suspect that this situation has fairly little to do with a perceived need to resolve a fundamental scientific uncertainty or disagreement, but has been largely driven by the rising popularity of cycling as a research topic in recent years. Following Pedersen and Hendricks (2014), we might conjecture that research focusing on environmental determinants of cycling has become somewhat of a "science bubble", comparable to the speculative bubbles in financial markets: from transport researchers and urban planners to epidemiologists, everyone wanted to join the booming field of cycling research (see Pucher and Buehler, 2017), regardless of there was actually a real "need" for so much research. As a consequence, we have ended up with a glut of similar studies which may be perfectly sound in and of themselves, but which don't really have any new insights to offer to policy-makers and practitioners involved in cycling planning.

2. Research as a distraction from action

Understandably, some might argue – not altogether unjustifiedly – that I am being unfair or excessively simplistic, and that there are many important aspects of the relationship between cycling and the urban environment we do not yet know enough about. Various authors (e.g. Forsyth and Krizek, 2011; Krizek, 2019; Stefansdottir, 2014), for instance, have remarked that we still know little about how to design cycling infrastructure which is best suited to enhance cyclists' aesthetic and affective experiences. Meanwhile, others (e.g. Krizek et al., 2009; Schoner et al., 2015) have noted that although there exists a clear correlation between cycling infrastructure and cycling levels, most studies have not conclusively proved the existence of a causal relationship in which new cycling infrastructure leads to an increase in cycling rates.

From a practical perspective, however, I would like to argue that these and similar uncertainties are largely beside the point. Echoing a line of thought recently advanced by Handy (2017) in relation to the effectiveness of compact development in reducing car usage, the heart of the matter is that a basic level of cycling infrastructure provision constitutes a *necessary* condition for popularising urban cycling (Dill and Carr, 2003; Forsyth and Krizek, 2010; Hull and O'Holleran, 2014; Pucher and Buehler, 2008), regardless of whether it is also a sufficient one. As long as most cities fail to meet this basic requirement, all subtler academic insights will remain, in a certain sense, irrelevant for policy-makers and practitioners. The existing cycling infrastructure in most streets in most cities worldwide is simply so lacking, that improvements in the theoretical state-of-the-art don't really make any difference to them: in a city where cyclists are forced to continuously compete for space with buses and lorries, for instance, finding out whether they prefer blue or red signposts doesn't really matter.

In this sense, the problem with calls for the need of "more cycling research" (Fishman, 2016; Handy et al., 2014; Krizek, 2018) is that they risk giving the impression that we fundamentally do not know what needs to be done to encourage cycling in a real-world context. In the vast majority of cities across the world, this is simply not the case: although we do not know everything, for all practical purposes we know *enough*. As put by Darnton (2016, p.164), "The truth of the matter is not that the 'case for cycling' lacks evidence or needs more discussion or conferences, but it is rather that the problem is political and emotional, not logical and rational". For the most part, I would even argue that Pucher and Buehler's (2008) review of cycling planning lessons from the Netherlands, Denmark and Germany contains essentially everything most cities need to know in order to promote urban cycling – at least in general terms..[1] As these authors put it, successful cycling promotion is essentially the outcome of a combination of "carrot" and stick" policies which encourage cycling and disincentivize car driving; among these policies, the most important is "the provision of separate cycling facilities along heavily travelled roads and at intersections, combined with traffic calming of most residential neighbourhoods" (p. 495).

In brief, my point is that in the vast majority of cases, making cities more cycling-friendly does not require any more theoretical planning knowledge, but merely the political will to do so. The overarching policy implication which emerges from existing research on environmental determinants or correlates of cycling is in fact remarkably straightforward: **provide more road space for cyclists at the cost of motorized traffic**. Of course there are many other considerations we need to take into account, but they are all secondary to this fundamental point. The seemingly endless growth of research on this topic, I argue, risks distracting us from this basic fact, leading us to not see the forest for the trees, and thereby becoming a distraction action. To quote Darnton (2016, p. 174) once again, "the endless demands of decision-makers for yet another study, evidence review, survey or workshop is no more than an intellectually respectable way of deferring a decision, and of doing nothing".

3. Towards a new research agenda

Where does this leave us? If in most cases we essentially already know what planners need to do to promote cycling in the real world, in what way can researchers continue to deliver new meaningful insights for cycling policy?

Admittedly, there are many possible answers to this question. In a short opinion article like the present viewpoint, it makes little sense to attempt to provide a comprehensive proposal for policy-relevant directions for future research – such an exercise would be something for a thorough literature review article. Nevertheless, I would like to conclude by suggesting a direction for future research which I believe holds unique promise in helping inform and support future urban cycling policy and planning: namely, focusing on understanding the phenomenon of **traffic evaporation**.

In simple terms, "traffic evaporation" or "disappearing traffic" is the opposite of induced traffic: an observed reduction in traffic levels following a reduction in the amount of available road space for general traffic (Cairns et al., 2002). Traffic evaporation is frequently reported following road closures or the reallocation of road space to pedestrians, cyclists or public transport: to provide a couple of notable recent examples, such a phenomenon has been noted following the pedestrianisation of the Seine right bank in Paris (Le Monde, 23/9/Le Monde, 2016) and the closure of 14th Street in New York to general traffic (New York Times, 13/10/2019). Up to the present, however, traffic evaporation appears to have been the object of scant academic attention. Indeed, most of the most available references on the topic consist of conference papers and grey literature, many of them more than a decade old (e.g. Cairns et al., 2002; European Commission, 2004; Mayerthaler et al., 2010; Sharples, 2009); only a handful of academic

[1] As a testament to its value, Pucher and Buehler's (2008) review is probably the most cited article ever in cycling research; by November 2019, it had been cited 1376 times according to Google Scholar.

journal articles appear to touch on the issue (e.g. Hunt et al., 2002; Ortigosa and Menendez, 2014; Zhu et al., 2010).

As argued in the previous section, the main problem faced by policy-makers trying to promote cycling is typically not lack of knowledge, but a practical (and political) challenge: providing more road space for cyclists at the cost of motorized traffic. In the light of this, **achieving a better understanding of traffic evaporation could play an instrumental role in assisting future cycling policy**. For one, documenting the process of traffic evaporation could provide a solid evidence base to counteract both popular and technical opposition to the implementation of cycle infrastructure, which is typically premised on the idea that giving space to cyclists at the cost of cars will lead to traffic congestion, when not full-blown chaos. In addition, developing a better understanding of how traffic evaporation works in different scenarios would allow policy-makers to make informed strategic choices as to what interventions to reallocate road space to cyclists are most feasible or desirable, and over what time frames such interventions can or should be carried out.

Critically, future research on traffic evaporation should try to go beyond a purely "technical" focus on numerical traffic counts and traffic models, which have constituted the bulk of research on the topic up to the present. While more research of this sort is certainly important, we also need a much broader understanding of the *social* dimensions of traffic evaporation. This is likely to require not only quantitative, but also qualitative approaches (see Sharples (2014) for an example). In other words, we should see traffic evaporation not only as a topic of study for traffic engineers and modellers, but also one for transport geographers and sociologists. At present, I would venture that the issue of traffic evaporation holds enough policy-relevant questions and methodological challenges to keep existing "cycling researchers" of various persuasions – from epidemiologists to urban geographers – busy for at least another decade. In what ways, for instance, do traffic evaporation rates depend on local context? What kind of time lags should we expect between road closures and traffic evaporation? What is the likely cumulative effect of multiple road closures? But equally importantly, what are the implications of traffic evaporation for social equity and spatial justice?

As I see it, trying to answer these questions is likely to do more for advancing urban cycling – and for urban liveability and sustainability more broadly – than continuing to devote our research resources to trying to find out ever more precisely why and where people do (or do not) cycle. Furthermore, I would like to think that shifting research efforts in this direction may also be desirable even if only on purely academic grounds. If nothing else, by diverting their energies from the topic of environmental determinants of cycling to the issue of traffic evaporation, researchers are likely to be able to develop more novel intellectual contributions. Even if such contributions do not prove any more successful in influencing real-world policy, they will at least offer a welcome distraction from yet another study telling us something we probably more or less already knew to begin with.

Author statement

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