Cease and desist: making false claims about the gateway effect

written by Clive Bates | 13 March 2014



Gateway effect: maybe… but is it an entrance or exit?

I am totally exasperated by spin about the so-called gateway effect.

Unable to take any more, I decided to write to Professor Stanton Glantz and Dr Lauren Dutra, the authors of an analysis of teenage smoking and e-cigarette use based on the US National Youth Tobacco Survey: Dutra LM, Glantz SA. Electronic Cigarettes and Conventional Cigarette Use Among US Adolescents. *JAMA Pediatrics* online 6 March 2014. I hope the letter is self-explanatory. Update: reply from Professor Glantz and my response to his reply.

_____ LETTER TEXT _____

Dear Professor Glantz, Dr Dutra

Misleading use of survey data has no place in professional public health practice

I had intended to blog about your recent paper, but decided instead to write an open letter and to put these concerns to you directly. So, I write to express dismay at the false, misleading and damaging conclusions you have drawn from your recent paper in JAMA Pediatrics and related media communications. The

errors of reasoning are elementary, but have been used to draw conclusions that are relentlessly hostile to e-cigarettes and the important public health concept of 'tobacco harm reduction'.

It should be stressed that misleading information in this arena can cause actual harm to real people if they react to misplaced concerns in ways that cause them to continue to smoke. Further dangers arise as physicians and other trusted professionals give bad advice based on false information. Finally, there is the danger that policy-makers and legislators will be misled into making excessively restrictive regulations that protect cigarette sales from competition from much safer and better alternatives. I believe your study and the subsequent media work owe more to misguided activism than to responsible academic investigation, and as such they have no place in professional public health practice or a reputable university.

I would like to address the claims made in three documents.

Document 1: Published article in JAMA Pediatrics

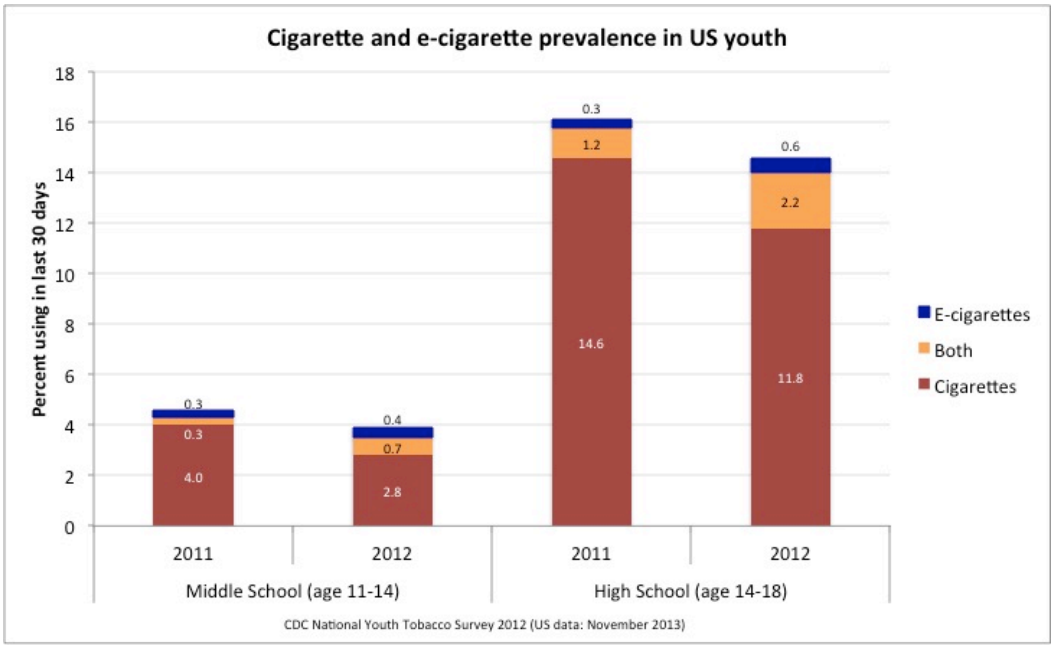
JAMA Pediatrics published article *Electronic Cigarettes and Conventional Cigarette Use Among US Adolescents*. From the abstract:

> ***Conclusions and Relevance*** *Use of e-cigarettes was associated with higher odds of ever or current cigarette smoking, higher odds of established smoking, higher odds of planning to quit smoking among current smokers, and, among experimenters, lower odds of abstinence from conventional cigarettes. Use of e-cigarettes does not discourage, and may encourage, conventional cigarette use among US adolescents*

All of these findings stated in the first sentence can be explained by the idea that e-cigarettes appeal more to smokers – hardly a surprise given they both provide nicotine – and that smokers are trying them more than non-smokers because smokers have an intent to quit, cut down or otherwise protect their health, or that whatever causes someone to smoke also causes them to try e-cigarettes. None of this is surprising, but somehow you have managed to position this study as showing there is a gateway from e-cigarette use to smoking. NOTHING in the study or the underlying data suggests this. You would need information on how smoking, e-cigarette use and abstinence evolved *over time* to test these

hypotheses, but your study does not have that.

There is no basis in this data to claim categorically that '*use of e-cigarettes does not discourage […] conventional cigarette use among US adolescents*'. In fact, despite the moral panic hyped up by CDC and FDA officials over this survey, there was a significant fall in current smoking among school-age adolescents in this survey between 2011 and 2012 (see CDC data: here and here). The relevant data are plotted below and are available to download [XLS] (done in association with Brad Rodu).



The data show a pronounced *decline* in cigarette smoking and in combined e-cigarette and cigarette prevalence between 2011 and 2012, as e-cigarette use increased (roughly as expected and in line with the growth in adult use). Of course, we cannot conclude from this that e-cigarettes are contributing to the reduction in smoking, but you certainly cannot rule it out in the way you have done. It is quite possible, and consistent with the data, that e-cigarettes are being used to quit smoking and nicotine use altogether, to cut down on smoking or to convert smokers to vapers. It is possible also that dual users are on a path to either exclusive use of e-cigarettes or to complete nicotine cessation, and this is a snapshot of the early stages of a migration out of smoking. The available data cannot demonstrate this is happening, but contrary to your unqualified assertion, it certainly does not demonstrate that: "*use of e-cigarettes does not discourage, […] conventional cigarette use among US adolescents*". Nor is there any basis in your analysis to believe e-cigarettes '*may encourage conventional cigarette use*'

any more than they discourage it. So why has this statement been included?

You have included a caveat in the article, but then ignored it…

> *While the cross-sectional nature of our study does not allow us to identify whether most youths are initiating smoking with conventional cigarettes and then moving on to (usually dual use of) e-cigarettes or vice versa, our results suggest that e-cigarettes are not discouraging use of conventional cigarettes. (emphasis added)*

This rather important qualification regarding the limits of the study is included in the full text of the article, not the abstract, but has not stopped you drawing causal inferences that the data and survey simply do not support for these reasons given above. In fact the caveat in the first part of this sentence should stop you making the assertion in the second part. How do you justify both the caveat and the assertion within a single sentence?

Document 2: JAMA Pediatrics press release

E-Cigarette Use by Adolescents Associated With Higher Odds of Smoking. I will focus only on one part of this, as it otherwise mostly follows the abstract.

> ***Background**: E-cigarettes are marketed in much the same way cigarette manufacturers marketed conventional cigarettes in the 1950s and 1960s, including on TV and the radio where cigarette advertising has been banned for more than 40 years. Studies have shown that exposing young people to cigarette advertising can cause them to start smoking. E-cigarettes also are sold in flavors (e.g. strawberry, licorice and chocolate) that are banned in conventional cigarettes because they appeal to young people.*

This is a one-sided and contentious framing of the issue, and none of it relates to the study in question. Its most useful, and unintended, function is to reveal the bias of the authors. You could equally have said: "*E-cigarettes are eroding the market for cigarettes by helping smokers to switch to vaping, cut down smoking or stop using nicotine completely. The edgy and skilful advertising of these products and imaginative range of flavours are essential to encourage as many smokers as possible to switch to low risk products. These products may*

completely change the market for tobacco and become one of the most significant public health innovations of the century. However, policymakers need to be mindful of unintended consequences". But you didn't. You chose to frame the issue in a way that suggests a strong predetermined belief, and not a that of a neutral investigator.

Document 3: UCSF press release

E-Cigarettes: Gateway to Nicotine Addiction for U.S. Teens, Says UCSF Study

Given what the study says and the token attempt at a caveat in the published paper, the UCSF press release headline and most of the text of release is *shockingly* misleading.

> ***E-Cigarettes: Gateway to Nicotine Addiction for U.S. Teens, Says UCSF Study***.

There is no basis for drawing this unequivocal 'gateway' conclusion from the data presented in the study. *None whatsoever* – and the published study even makes this clear. In fact the data are consistent with the opposite hypothesis – that e-cigarettes are primarily used by smokers interested in quitting or cutting down. Indeed the observation that users have a higher intention to quit smoking lends support to that hypothesis more than to your preferred explanation. This headline error is compounded and elaborated thus:

> *But the authors noted that about 20 percent of middle school students and about 7 percent of high school students who had ever used e-cigarettes had never smoked regular cigarettes – meaning that some kids are introduced to the addictive drug nicotine through e-cigarettes, the authors said.*

This is highly misleading. In an alternative situation in which e-cigarettes were not available, then the small proportion of kids that "*are introduced to the addictive drug nicotine through e-cigarettes*" may simply have started to smoke cigarettes and got their introduction via the much more dangerous and addictive route. In that case it may be a *good thing* that their smoking onset has been diverted into e-cigarette use – please see my posting: *We need to talk about the children – the gateway effect examined* for more discussion on the likelihood that

gateway effects will be positive for health, or exits from smoking.

In one situation where low risk nicotine products have radically eroded the market for cigarettes, that being snus use in Sweden and Norway, we see snus acting as alternative to smoking onset, diverting young people away from smoking, with greatly reduced risks that follow from that. If you are unaware of this experience, this paper might help: Ramstrom L., Foulds J. Role of snus in initiation and cessation of tobacco smoking in Sweden. *Tob Control* 2006; **15**:210–4. I can provide further citations if you are interested in gaining a better understanding of the practical experience of market-based tobacco harm reduction and real-world gateway 'exits' from smoking.

Also, to note the insight offered by Carl Phillips, it is not the *order* in which initial use occurs that determines whether one behaviour causes another and there is a 'gateway effect'. We are interested in what determines whether someone becomes a smoker (i.e. adopts an enduring pattern of cigarette use), not what causes someone to use their first cigarette – these effects are obviously different and can have different causes. It is theoretically possible for people to take their first nicotine through smoking, but only to continue to regular smoking because of the availability of e-cigarettes to provide ancillary support their smoking habit. In that (unlikely) event, e-cigarettes would be the cause of the smoking behaviour, but not the cause of the first cigarette. Equally, people who have smoked first may use e-cigarettes to quit. People who first use e-cigarettes, may go on to smoke and then use e-cigarettes to to quit smoking. The point is that your study has no data that can illuminate which, if any, of the these pathways is being followed, and by how many people. However, that has not stopped you choosing the most negative and damaging (and *unlikely*) interpretation over all others for no credible reason, and then making a headline message of it.

I would like to draw out one further statement:

> *"Despite claims that e-cigarettes are helping people quit smoking, we found that e-cigarettes were associated with more, not less, cigarette smoking among adolescents," said lead author Lauren Dutra, a postdoctoral fellow at the UCSF Center for Tobacco Control Research and Education.*

The reasoning for claiming e-cigarettes do not help people quit smoking amounts to a crude *non sequitur: "e-cigarettes were associated with more, not less,*

cigarette smoking among adults". More, not less… *but compared to what*? The study found that more smokers were using e-cigarettes than non-smokers. However, this banal observation does not confirm that e-cigarettes do not help quitting any more than finding that NRT is used more by smokers would suggest NRT is not used for quitting. The real test of the impact of e-cigarettes is hard to gauge because it requires knowledge of what would have happened in the absence of e-cigarettes. If you could show there is "more, not less" smoking than there otherwise would have been had e-cigarettes not become available, then that would definitely be a concern. But of course the study does not and cannot do this, given the limitations of its methods and the available data.That doesn't stop you claiming the following, which as far as I can see, *is based on nothing at all*:

> *"E-cigarettes are likely to be gateway devices for nicotine addiction among youth, opening up a whole new market for tobacco"*

Reaction from tobacco control and public health experts

The tobacco control community is often reluctant to challenge its priesthood and can be supine or complicit in supporting junk science that justifies its preferred policies. However, the distortions in this study and its media communications are so egregious that it has provoked criticism from public health establishment figures. The New York Times report on the study contained the following welcome rejection of the key assertions by Tom Glynn of the American Cancer Society:

> *"The use of e-cigarettes does not discourage, and may encourage, conventional cigarette use among U.S. adolescents," the study concluded. It was published online in the journal JAMA Pediatrics on Thursday. But other experts said the data did not support that interpretation. They said that just because e-cigarettes are being used by youths who smoke more and have a harder time quitting does not mean that the devices themselves are the cause of those problems. It is just as possible, they said, that young people who use the devices were heavier smokers to begin with, or would have become heavy smokers anyway. "The data in this study do not allow many of the broad conclusions that it draws," said Thomas J. Glynn, a researcher at the American Cancer Society.*

… and David Abrams of Legacy Foundation also ridiculed the reasoning:

> *But David Abrams, executive director of the Schroeder Institute for Tobacco Research and Policy Studies at the Legacy Foundation, an antismoking research group, said the study's data do not support that conclusion. "I am quite certain that a survey would find that people who have used nicotine gum are much more likely to be smokers and to have trouble quitting, but that does not mean that gum is a gateway to smoking or makes it harder to quit," he said.*

David Abrams elaborates further in the Huffington Post

> *The problem with Dutra's conclusion, said Abrams, is that causality could run the other way; kids who smoke (because of genetics, or parents who smoke) could simply be more likely to engage in other risky behaviors as well, such as alcohol, marijuana or e-cigarettes.*
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> *"One does not lead to the other [in this study]," said Abrams. "The behaviors just travel together in vulnerable kids." The only way to establish causality is a rigorous longitudinal study that follows a large number of people over a long period of time — something that would take decades. In the mean time, said Abrams, "the science doesn't support panic or fear [of e-cigarettes] — yet."*
>
> *Best known for its "Truth Campaign" ads, Legacy's vision is a society free of the death and disease caused by tobacco. And according to Abrams, e-cigarettes could be the key to achieving that society. "They are the first product in 100 years that might make cigarettes obsolete," said Abrams. "That would literally wipe out the death cause to 5.6 million kids alive today, as well as 480,000 adults every year."*

Would you care to explain if you think Glynn and Abrams are incorrect in their assessment?

Other commentators

There are several robust analyses of this study, all of which I have benefited from. All of them are unflinching in their criticism:

- Michael Siegel: Conclusion of New Glantz Study on Electronic Cigarettes

is Junk Science
- Michael Siegel: Glantz Press Release is Dishonest with Public; Authors Appear to be Intentionally Lying to Mislead the Media and the Public
- Carl Phillips: Stanton Glantz is such a liar that even the ACS balks: his latest ecig gateway "study"
- Brad Rodu: UCSF Study Falsely Links E-Cigarettes to Smoking

Sadly, this is not the first time such irresponsible over-interpretation of data has been used as part of a media propaganda offensive against e-cigarettes. The same model was used in this study of Korean school age adolescents (Lee S, Grana RA, Glantz SA. Electronic Cigarette Use Among Korean Adolescents: A Cross-Sectional Study of Market Penetration, Dual Use, and Relationship to Quit Attempts and Former Smoking. *J Adolesc Health* 22 November 2013).

- The study: Electronic Cigarette Use Among Korean Adolescents: A Cross-Sectional Study of Market Penetration, Dual Use, and Relationship to Quit Attempts and Former Smoking
- UCSF press statement: Electronic Cigarettes: New Route to Smoking Addiction For Adolescents: Kids who use ecigs less likely to have stopped smoking
- Critical response by Michael Siegel: New Study Completely Misrepresents Findings to Mislead the Public About Role of Electronic Cigarettes as a Gateway to Smoking Initiation

The failings in the Korean analysis and media approach were bluntly pointed out at the time, and it is therefore all the more troubling that they have been repeated in the new paper.

An approach that abuses and erodes trust

It appears there is a malign system at work here. The original paper has a modestly phrased, but incorrect and unsupported, statement suggesting that e-cigarettes do not reduce smoking and may increase it. This is surrounded a barrage of statistics and protected by what appears to be a responsible caveat, though that is actually ignored in practice. This gets past the inattentive journal peer reviewers and into a publication with an important sounding name. Caveats are then jettisoned and hype is developed in the JAMA press release and, above all, in the UCSF press release. The media work seems to be free of any academic

integrity and aimed at creating a media storm irrespective of the quite dull and predictable findings of the actual analysis. The media, which is still used to trusting famous universities and well known public health academics, obliges and repackages the hype relentlessly – including, annoyingly, in the UK. The distinction between association and causation is lost, as journalists read the press release and quite reasonably assume that the association must be causal or there would be no point or justification in making the 'gateway' headline with it.

Once in the news media, and widely propagated via wire services, it is uncritically repeated in maybe hundreds of regional and local outlets and goes world wide. Then it enters the political sphere. Here is an example – no doubt one of many – of an elected representative, Philadelphia City Councilman and Majority Deputy Whip, Bill Greenlee being mislead by the study and press release, and so tweeting the bogus story:



You are responsible for this, and it is not something to be proud of. It is not the proper role of a university to introduce this sort of propaganda into democratic debate on public health, nor is it right for you and UCSF to abuse the trust of citizens, journalists and politicians like Mr Greenlee. Will you contact Mr Greenlee and explain that he has been misled?

Please put this right and reconsider your approach

I really hope you will reconsider your approach – it does not serve public health or the public interest. It is wrong simply to assume that e-cigarettes will play no part in reducing the expected one billion 21st Century deaths from smoking. The

evidence that there is, taken as a whole, is actually very encouraging, and certainly not a reason to mount a 'dirty tricks campaign' against these products. In any event, there is *never* a case to mislead the public, policy-makers and legislators even if you are convinced the end justifies the means, which in this case it does not.

I would welcome your responses to the points raised in this letter. If I have misunderstood your work I will of course acknowledge and correct. I trust you would wish to do the same for false or misleading statements in your own work. As you know, UCSF expects *adherence to the highest standards of integrity in proposing, conducting and reporting research*. I do not think the highest standards have been met in this case, do you? Do you agree that you should now withdraw the paper from JAMA Pediatrics and circulate a substantial correction to the UCSF press notice? If not, what is the alternative?

Yours sincerely

Clive Bates
London, UK

_____ END OF LETTER _____

Updated 15 March 2014.

News coverage: see examples displayed on Professor Glantz' blog.

Post script. A number of people have commented along the lines: "*nice work, but nothing much will come of this…*" and I agree a letter from me isn't likely to make much difference in itself. There are however other, more forceful, options, if people feel strongly:

1. The authors' responsibility. First, it is important to let the authors have their say. They may make a good case and I may be doing them a disservice. That is possible, but I have yet to have a criticism of the substance of this case and others have drawn similar conclusions. If these complaints are justified, the preferred resolution is that the authors put it right themselves. It is their paper and press release, and their responsibility.

2. Challenge from within public health. The most significant development on this has been, in my view, the welcome comments made by David Abrams and

Tom Glynn to the NYT – and I gather others who made statements but were not quoted. By speaking up, more of the 'heavyweights' in tobacco control and public health can show they do not wish to see this sort of propaganda in tobacco policy-making or tolerated within the academic discourse in this field. There are may ways to do that: comment in the media; write to rogue authors directly; comment on this letter on my blog or support and circulate the excellent critiques made by others; write to correct misleading media reports that have come from bad research; respond to legislators or officials who cite bad research; tweet, blog or otherwise publish critical views; suggest to tobacco control activists that poor papers should not be used in campaigning…. to name a few.

3. Challenge in the formal literature. Established academics can undertake a peer-reviewed, critical response to the paper submitted to JAMA Pediatrics or published in another journal.

4. Challenge through the JAMA Pediatrics editorial board. Ask the editor for the paper to be withdrawn on the grounds that it contains glaring non-trivial errors or otherwise engage with the editorial governance at JAMA – this could be done by anyone, but could be done effectively following publication of a formal critique, as in 3 above.

5. Challenge through UCSF. Approach the Office of Ethics and Compliance at UCSF, and make a complaint. UCSF requires *adherence to the highest standards of integrity in proposing, conducting and reporting research.* This means compliance with general ethical standard for scholarship set out in the University of California Faculty Code of Conduct at Part II B and the UCSF Campus Administrative Policy 100-29 on Integrity of Research: Research misconduct is defined: *fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. Research misconduct does not include honest error or differences of opinion*.The most obviously egregious violation is the way the survey was spun in the UCSF press release – to me this looks like a case of 'fabrication in the reporting of research results'. The university says it is responsible for investigating, so it is up to them to decide where, if at all, there has been a breach. Such a complaint would probably be better coming from a senior American academic or public health leader than from me.

It all depends on how keenly people feel about this, and for me, how well the

authors respond.

Update: Professor Glantz replies as follows:

> *On 31 Mar 2014, "Glantz, Stanton A" wrote:*
>
> *If you would like to engage in correspondence about our paper, I suggest that you send a letter to the journal. We will respond in that forum should the editors deem your letter worth publishing.*

Update: I reply to his response on 31 March 2014, explaining that I don't think this is appropriate – it is his responsibility to put this right.

> *Dear Professor Glantz*
>
> *I am disappointed but not surprised that you would choose to avoid engaging directly. I can see why you might prefer me write to the journal: this would allow you to respond by dismissing the criticisms without addressing them, leaving limited recourse for your critics to challenge your response. It would impose paywalled restrictive visibility, shutting out the wider world with interests in this work, and it would impose more months of delay to the reckoning this work deserves. I'm not going to be diverted down that course for these reasons and for the reasons below.*
>
> *1. The letter I sent you and Dr Dutra covers ground beyond the JAMA Pediatrics article, in particular the press release and hype from UCSF, where most of the distorting damage was done.*
>
> *2. It is not me who should be writing to JAMA Pediatrics, but you, the authors, to request that the paper, or its false conclusion, is withdrawn. Likewise, the egregiously false and misleading press release should be withdrawn. You did not find a gateway, but you claimed to the world's media that you did. It is your responsibility, not mine, to put that right.*
>
> *3. Having published your paper, the journal itself has a conflict of interest. If I and many others are correct, then its peer review system and editing discipline has failed. It has incentives not to have that failure paraded across its own correspondence pages in the straightforward terms that are justified. So I don't think that is a suitable place to challenge your work.*

4. The criticisms herein are broader than an academic disagreement – they relate to the academic-media-political nexus of this work, or, more plainly, the propaganda offensive you appear to be mounting against e-cigarettes and tobacco harm reduction, and it applies to more than just this paper. I had hoped you would read my letter carefully and recognise that this is an ethics and research conduct issue. It is not merely the failure in a single instance of a journal to moderate properly the excesses of its authors, though it is clearly that as well.

I think there are many on this list, and beyond, who would like to see a candid and direct response from you to the points raised in this open letter. You promoted the paper on your popular blog along with the press coverage and press release, so I see no reason why you should now become more formal and reticent about direct discussion with your critics. Once again, I invite you to make a substantive 'well thought out response', as Dr Dutra suggested you would in her e-mail of 14 March.

Please let me know what you intend to do.

Regards

Clive Bates

London, UK