

-----Original Message-----

From: post@dillon.eu [mailto:post@dillon.eu]  
Sent: dimanche 8 mars 2015 14:43  
To: rebecca.giblin@monash.edu  
Subject: Re: Re: Evaluating Graduated Response

Dear Rebecca,

Thank you for your response. I am sorry that you do not think that rational argument would be well spent in defending your paper against my critique.

The knock-down argument that you deploy is the public fact that I used to work for the MPA (though I have no continuing professional relationship with them). I am afraid that if you are the subject of an MPA campaign to discredit your work, the Association has failed to include me in it. My commentary on your paper was motivated by the fact that you wrote to me saying that I had insulted you.

The reason that you give for not engaging in debate indicates that I have failed to persuade you that there is a difference between the statement: "the available evidence shows that the current global graduated responses do not achieve the aims of copyright" and the statement: "the available evidence does not show that the current global graduated responses achieve the aims of copyright". This is not a mere verbal game.

I shall post this exchange on GraduatedResponse.org. I thank you for your attention to the site.

Best regards,

Thomas

**From:** Rebecca Giblin [<mailto:rebecca.giblin@monash.edu>]  
**Sent:** jeudi 5 mars 2015 00:04  
**To:** Thomas Dillon  
**Subject:** Re: Evaluating Graduated Response

Dear Thomas,

Thanks for taking the time to reply in such detail. I was very interested to discover that someone had taken the time to do such a comprehensive examination of the paper, and indeed such a painstaking line by line review of the way it evolved from the work-in-progress that was originally released for public input to the published version. I understand that you were legal adviser for the Motion Picture Association for some years and that you provide that organisation with ongoing advice. Was your analysis done as part of your work for the MPA or some other client with an interest in the adoption of graduated response, or did you do it on your own initiative?

As a general rule I am most eager to engage in substantive debate about my research, and that's why I released a preliminary version of this paper for public comment and debate. If I had missed any element in any of the multiple jurisdictions and languages covered by the paper, I wanted to know about it in order to remedy that before publication. As part of that process I formed the view that my

initial treatment of the Danaher study was inadequate, and as you note, developed that analysis further in the final version of the paper.

In other circumstances I would put together a point by point response explaining each of the judgment calls with which you take issue. In this case however, it seems pointless to attempt to engage you in rational debate. The lens through which you view this research demonstrates your mind is already made up. You continue to insist that the article makes the case that graduated response does not work. This is despite the very clear explanations throughout the paper that its purpose is merely to evaluate the evidence about the efficacy of graduated response. The basis for your making that claim is the slightly clumsy wording that appeared in one sentence of a preliminary draft, and which was subsequently amended to ensure no such confusion arose. That is, you point to an edit I made to ensure there was no possibility anyone would be confused into thinking that the paper sought to show graduated response did not work...as evidence that the paper really was seeking to show graduated response does not work. And then you triumphantly note that I produce no evidence to justify such a conclusion! Given the prism through which you are viewing the paper, it is no wonder that you have different interpretations of the evidence. I'll content myself with noting that I agree to disagree.

I would encourage any interested readers of this public correspondence to examine the full text of my paper together with the criticisms you raise and make up their own minds as to the merits of the work.

All the best,  
Rebecca

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On Mon, Feb 2, 2015 at 10:29 AM, Thomas Dillon <[post@dillon.eu](mailto:post@dillon.eu)> wrote:

Dear Dr Giblin,

I am sorry to have injured your feelings, but I do not share your opinion of your article.

The overall effect of the article is to make the case that the graduated response does not work and should not be retained or further adopted. Your conclusion in the first version of the article was: "Much can be done to design copyright law in ways that will help achieve desired aims. But the graduated responses in place right now overwhelmingly fail to do so". The slight retreat in the revised version (the addition of "as far as we can tell from the available evidence") hardly diluted the message. So far as I can see you produce no evidence that graduated response does not reduce infringement, so as to justify such a conclusion. I regret to say that you seem rather prejudiced against what you call "Big Content" and over-eager to prove that the graduated response is ineffective.

It seems to me that you repeatedly treat absence of evidence as evidence of absence: “There is no evidence demonstrating a causal connection between graduated response and reduced infringement. If ‘effectiveness’ means reducing infringement, then it is not effective.” This is surely a non sequitur.

If anyone has said that the discrepancy in notices sent as between first-time notice recipients and second-time recipients of itself establishes that the graduated response is an effective enforcement tool, you are quite right to take them to task. But I think most people understand that such evidence is only consistent with a positive effect and does not prove it. But Hadopi has not relied solely on the proportion of second or third notices to support the argument for an effect. It has survey and anecdotal evidence of the working of the system (see Lescure Report at p. 377). When you reviewed Hadopi’s “Hadopi, 1 an ½ après son lancement” report, you failed to refer to their evidence on user behaviour, saying incorrectly that “every figure cited in support of the claim that the French law reduces infringement” came from an industry-connected source. You also did not mention that the Peer Media Technologies data referred to by Hadopi included a comparison of the French situation with the rest of the world, showing a substantial relative reduction (a fall from 6.2% of global infringing uploads to 4.5% in the period January 2011 to December 2011) – surely quite a suggestive finding.

It has long been obvious that the significance of the proportion of second-time to first-time recipients would depend on the proportion of the infringing public that was captured by monitoring and sent notices. If 100% of the infringers are being monitored, the number of second notices is strong evidence of behaviour change by first-notice recipients. But we do not know how universal the scanning for infringements is. Without this figure, we cannot compute the chance of receiving a second notice. I do not understand the objective basis for saying: “In sum, the probabilistic relationship between actual infringement and notices issued means that a large discrepancy between the various types of notices would inevitably occur even if recipient subscribers did not change their infringing behaviour at all”. We do not know the “probabilistic relationship” – just that there is one.

In addition, you appear to derive the number of likely second-notice recipients from the proportion of the number of first notices to the number of notified infringements (2m to 18.3m). But the number of infringements does not tell us how many potential notice-recipients (i.e., infringers) were detected. The 12% figure hence seems pretty meaningless.

What particularly surprised me about your article was the treatment of the research by Brett Danaher and others. In the version of the article that I originally read (which is still accessible online) you devoted more than a page to Le Monde’s attempt to disprove their results, but only one sentence to the researchers’ riposte: “The original Danaher study was subsequently amended to incorporate a new Appendix which, while not expressly referring to the Le Monde critique, used a different methodology in an attempt to suggest that iOS device penetration in France did not in fact drive the sales increase”. The failure to engage with Danaher’s evidence, which seemed to me to be conclusive against the Le Monde argument, was striking.

In the revised version of your article, you returned to the Danaher research to revise your response. You decided to dismiss it by reference to the Lescure Report, saying: “However, after considering all

of the available evidence, including the Danaher study, the Lescure Report found that even if HADOPI had brought about some reduction in P2P infringement, traffic had overwhelmingly been diverted to other infringing sources rather than to the legitimate market”.

But the Lescure Report did not express itself in that hypothetical way about the effects of the graduated response. The Report explicitly states that the Danaher study “tends to substantiate the idea of a positive impact of the graduated response on the lawful consumption of cultural content online” (p. 370), but says that the statistics tend to show that it has been of greater benefit to other forms of piracy than to the legal market (see p. 371). “Overwhelmingly” appears to be a rhetorical flight of your own. Lescure said that the graduated response needed to be retained, and that it should preserve its “dissuasive character indispensable to the effectiveness of author’s rights and the protection of the rights of creators” (p. 373).

For some reason in your revised version you omit to mention that Danaher had not just plotted search results for “iPhone”, but also examined actual data on iPhone penetration. This formed the basis of the annex to his original article, but you leave it unexamined. On the other hand, you treat as evidence against Danaher’s conclusions the fact that the French market for recorded music fell in 2011 to 2012 as against 2009 to 2010. But you offer no evidence as to what it would have been in the absence of HADOPI. In any case, the comparison should be with the period before HADOPI started to have an effect – 2007-2008. The 2009 Rennes study (which you rely on) showed that HADOPI started to have an effect on P2P usage even before the first notice was sent.

Your article has been very successful and has been widely reported: “Anti-piracy ‘strike’ schemes are not effective, research shows” (Torrentfreak); “Three strikes doesn't deter copyright infringement” (ZDNet); “Three-strikes laws do not reduce online piracy: study” (ITNews). I believe that the reporters have correctly understood your message.

I regret to say that I remain of the opinion that the mildest health warning I can issue is to call your article “somewhat tendentious”. I am happy to publish your email on the web site, however, so as to allow its rather limited audience to appreciate your dissatisfaction with my verdict.

Best wishes,

Thomas Dillon

**From:** Rebecca Giblin [mailto:[rebecca.giblin@monash.edu](mailto:rebecca.giblin@monash.edu)]

**Sent:** 22 January 2015 23:49

**To:** [post@dillon.eu](mailto:post@dillon.eu)

**Subject:** Evaluating Graduated Response

Dear Thomas,

I was surprised to come across your description of my research in Evaluating Graduated Response as being 'somewhat tendentious'.

When I started this major research project, which involved painstaking research over some 15 months, covering a wide variety of jurisdictions and languages, I certainly expected to find more evidence of efficacy than I was able to turn up. However, there was surprisingly little. For reasons described in detail in the paper, what evidence there was tended to be of extremely poor quality.

If there was quality evidence that I missed, there has been ample opportunity for it to be brought to my attention in the year and a half since this research became public. However, despite those findings upsetting a great many well-resourced people, nobody has done so.

The research points out for the first time some of the key logical fallacies being used in support of claims of efficacy of graduated response (in particular, with regard to notice volume data). This analysis is supported by mathematical modelling which, again, has gone unchallenged.

In addition, the paper has been peer reviewed by Faculty of Columbia Law School and ultimately published in the Columbia Journal of Law & the Arts, a well respected journal that is hardly known for being anti-copyright.

Furthermore, you have misconceived the purpose of the paper. Your full comment reads, 'The author makes a somewhat tendentious attempt to demonstrate that the graduated response does not achieve its intended effects.' That is not at all what the paper seeks to do: it only claims to evaluate the *evidence* that graduated response achieves its aims. That is very different indeed from what you represent.

In these circumstances it should be of no surprise to you that I find your use of 'tendentious' insulting and inaccurate.

Regards,

Rebecca

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