the substantial effects of study design and procedures. The household study was designed before the 2009 pandemic, and we were able to take the opportunity to estimate the household secondary infection risks. For this reason, we agree with Farrell and colleagues1 that a weakness of our study is the small sample size. Rather than aiming to detect small differences in transmissibility, we used the existing data for statistical modeling to qualitatively validate our earlier findings.3

As Farrell and colleagues1 have noted, receipt of influenza vaccination could affect the results. Although valid estimation of secondary infection risks is based on the assumption of uniform susceptibility among household contacts,6 we currently lack good correlates of immunity. In a sensitivity analysis, we excluded everyone who received seasonal vaccine (eAppendix Table 3, http://links.lww.com/EDE/A519) and came to similar conclusions. Including or excluding those with antibody titers \( \geq 1:160 \) did not change secondary infection risks for seasonal and pandemic strains. In summary, the results on relative transmissibility were not influenced by vaccination and thus we believe are generalizable.

We chose to use an age cutoff of 15 years based on early work as- sessing antibody responses in children and adults7 and because this cutoff was used in several studies addressing transmission of influenza within households.8 The immune system undergoes changes during puberty, which would support the use of a threshold at this age or even younger.10 Alternatively, expanding the threshold to include all school-age children would have had minimal effect, as only 16 people (4%) in our study were 15–17 years of age.

Finally, we completely agree that there is a critical need to elucidate genetic variation between host generations of influenza infection.11

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On Compulsory Preregistration of Protocols

To the Editor:

Reading the paper by Lash and Vandenburgrouck1 is a relief in the face of extreme criticism on the failures of epidemiology. This is not because the authors state that epidemiologic research is not producing erroneous results but rather because they question statements that have been presented as uncontestable truths, namely the problem of false positives. However, I think that their proposal of registration of epidemiologic data is unrealistic and unnecessary in most fields of epidemiologic research.

We have inventoried mother–child cohorts in Europe2 and have created a registry of such cohorts (www.birthcohorts.net). This has been an enormous effort supported by funds from 5 European Union-funded projects (ChildrenGenoNetwork, Earnest, NewGeneris, Enrieco, and Chicos)—2 of them specifically defined as “Coordination and Support Action.” (The attempt to extend this nearly complete European registry to include mother–child cohorts in other parts of the world has failed.) This coordination has enhanced joint work in a field that includes some big studies (Norwegian MoBa, Danish National Birth Cohort, Alspac) and numerous fragmented initiatives spread around Europe. Putting together, European teams provided the possibility of conducting analyses with larger numbers and also promoted an active forum of discussion and development of new hypotheses through meetings of practically all European teams working in the area of mother–child cohorts. This was what we needed; the registry has been a means to this end.

Can this experience be generalized to include all areas of epidemi-
logic research? I doubt it. It would require a huge amount of funds and effort. In view of the current situation in Europe, where public health research is seriously underfunded, this is unrealistic. In addition, the practical problems for setting up registries and keeping them updated are far bigger than may be anticipated. I doubt that it would be even useful to create a general registry of such data. Such initiatives may be useful in specific research areas, but they are simply too complicated to include all areas of epidemiologic research. PubMed and additional direct contacts have efficiently solved the problem of identifying most of what is done.

We are simply doing OK as we are; epidemiologic research has shown a high capacity for creative thinking and self-evaluation. Perhaps, we need more thoughtful papers such as that by Lash and Vandenbroucke (and also less self-flagellation, and much more serious funding).

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_The authors respond:_

We thank Dr. Kogevinas for his collegial letter regarding our commentary, and especially his insights about the resources required to establish and maintain a public registry of epidemiologic datasets. We continue to believe that this counterproposal is a better idea than publicly registering all epidemiologic protocols and hypotheses. We do not, however, strongly encourage that this counterproposal should be undertaken (some of the language in our commentary was ambiguous regarding the strength of our conviction).

The experience of the European registry of mother–child cohorts, as explained by Dr. Kogevinas, indicates that the effort required to implement either type of registry is large, and quite likely too large to justify, especially in an era of dwindling research support. Particular research fields might choose to implement such a registry if the benefits are judged to outweigh the costs; the ENcEPP registry of pharmacoepidemiology research resources provides an example. If, however, the experience of other similar undertakings is the same as the experience described by Dr. Kogevinas, then we would agree with him and withdraw our support for a universal registry of contents of datasets.

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