Suppose that your house needs some restoration, and that you call a master mason asking for an estimate. If the mason replies at once that he will quote 1000€ for himself, plus 500€ for each helper apprentice, you will likely be puzzled, if not annoyed. Surely you have good reasons to complain, reasoning that the job you ask for should be remunerated with a fixed amount, irrespective of the number of labourers it requires. Yet, this is not a criterium that we usually apply when evaluating the CV of an applicant for an academic position or for a grant. We may examine the number of papers the applicant has made, where they have been published, or how many citations they have obtained. More recently, we would surely check the Hirsch h-index [1], or exploit more sophisticated indicators. Rarely we look for the extent of coauthoring: a good paper is a good paper and, in terms of the applicant prestige, it is often regarded to be equally worth regardless it is signed by one, five, or two hundreds coauthors. Possibly, if the applicant is the first author, who presumably made the hard job, or the last one, usually the lab "master mason", you may grant her or him an additional bonus. But that’s all. After all, recovering quantitative information of this kind from search services like ISI or Scopus, even something simple as the average number of coauthors per paper, is not immediate (just try!).

Suppose however that the mason refutes your argument by claiming that the more people do the job, the better it comes out. You may be skeptical, but you will not easily come out with general abstract arguments for or against such a claim. Like a cosmologist who has a single Universe to investigate (if she or he is an experimentalist, at least), you have just this house to test, and relying on repetitive trials is out of question (besides expensive). Grounding discussions about coauthoring on abstract arguments is conversely not uncommon in the scientific community, at least in my native country. Some colleagues argue that, yes, discouraging excessive coauthoring is probably sensible, but that a penalty consisting in simply dividing the citations of a given paper by the number $N$ of authors is probably excessive. So they suggest using diverse sublinear functional forms of scaling, such as dividing by $\sqrt{N}$, usually on the basis of some kind of a priori reasoning. Some others (mostly experimentalists), however, reply that being able to build up a collaboration network is a virtue that should be acknowledged, hence no scaling should be applied if $N$ is still moderately large, say, smaller than 5 or 10. When questioned, certain physicists - for some obscure reason, usually high energy experimentalists - even let the matter drop at once, branding talks of this kind as "absurd".

The fact is, at variance with the former case, we do have a sensible, albeit not perfect way to quantify how
much coauthoring impacts on the recognition of a publication by looking at the total number of citations it has received after some years. I have then considered the number of citations in the first 6 years, according to ISI Web of Knowledge (WoK), by all manuscripts published in Physical Review Letters [2] in 2007 (about 3700 records, including comments but not replies and corrections). I have then sorted these papers in groups on the basis of the number of authors, and evaluated the average and standard deviation of the number of citations $c$ for each group. A first striking evidence from the results, shown in Fig. 1, is that $c$ grows by a mere factor of two when $N$ increases from 1 to 10, namely, just a little more than 8% for each additional author. Equally surprising is that, as clearly evidenced by the purple band in Fig. 1, very large collaborations do not seem to yield, at least on the average, a much greater impact on the scientific community. In other words, if we “reward” each author on the average, a much greater impact on the scientific productivity of an author. In fact, the ratio $h/Np$ of the number of papers that contribute to the $h$-index to the total number of papers $Np$ an author publishes (which we could consider as a kind of “success ratio”) rapidly decreases with $Np$. Actually, the main body of Fig. 2 shows that $h$ is quite well fitted by a linear dependence on $\sqrt{Np}$, except for $Np \gtrsim 400$, where some saturation may be present. What is really surprising is the very limited dispersion of the data around the mean. As a matter of fact, the ratio between the actual value of $h(Np)$ for the individual authors and the value one gets from the fit to the data has an approximately gaussian distribution, with a standard deviation $\sigma = 0.23$.

In simple words, this means the following: tell me the total number of papers you have published, and I’ll predict your $h$-index within 20 – 30% accuracy. More seriously, this result cast doubts on the amount of novel information the $h$-index carries per se, besides a simple reshuffling of a basic and rather trivial information about the total scientific productivity of an author. In fact, provided that these general observations are confirmed by testing a much larger and varied sample besides the limited and rather selected one I have considered, surely not representative of the whole population of physicists [3], a more meaningful bibliometric parameter would actually be the deviation $\delta h = h/h_{\text{geo}} - 1$.

The combination of the basic independence of the value of a scientific paper from $N$ with the former tight statistical relation between $Np$ and $h$ would, if confirmed, be particularly significant for those physicists belonging to large collaborations such as Atlas, LHCb, CDF, and so on. The main body of Fig. 3 shows that the frequency distribution of the $h$-index for those authors considered in Fig. 2 which has an average value $\langle h \rangle \simeq 27$ and a relative standard deviation $\sigma_h/h \simeq 0.63$ is, as may be expected, considerably skewed. The distribution is indeed approximately fitted by a Gamma PDF with an expectation value $\langle h \rangle \simeq 25$ and a much lower mode $h_{\text{max}} \simeq 14$. However, the inset shows that the same distribution,
when restricted only to those authors belonging to large collaboration groups, has a rather different shape, being almost symmetric, with a larger average value $\bar{h} \simeq 34$ but a lower relative standard deviation $\sigma_h/\bar{h} \simeq 0.47$. These means that these authors, besides being inclined to publish more (recall, however than, on the average, collaboration papers are not cited much more than papers with a few authors), and form a more homogeneous group in term of their overall “scientific success”. Note that, in this restricted distribution, low values of the $h$-index are consistently less represented. hence, either young scientists are less frequently included in the authors’ list or, more likely, belonging to large collaboration groups rewards young physicists by allowing them to coauthor so many papers that their bibliometric parameters rapidly rise to values which are typical of more mature scientists. In any case, the relative homogeneity of the population, together with the limited credit that, according to Fig. 1, should be given to a single individual for the acknowledgement of works made by large groups, makes the $h$-index a rather poor evaluation parameter to differentiate among young high-energy or nuclear physicists.

As I mentioned in the abstract, this little divertissement should not be taken too seriously, for any sound conclusions must be corroborated by a much more extensive and rigorous statistical analysis. The former observations, however, lead me to two considerations. For what concerns myself, in the future I would not like to take part in committees where hiring or funding of young scientists is made only on bibliometric bases, renouncing to the pleasure of interviewing, even shortly, the candidates. For what concerns my fellow countrymen, the warning is that no bibliometric approach to hiring and promoting, however refined, will ever ensure a real improvement of our academic institutions, unless there are ultimate motivations to long for scientific quality. And this, in a country where competition between universities is still seen with suspicion - “rating, but not ranking” is a basic recommendation of our National University Council (CUN) [7]- is far from being a priori ensured.

Finally, let me thank Pietro Cicuta for having invited me here in Cambridge, where (besides doing some real work), I managed to find some time for idling with these trifles. I have also took pleasure from discussing these issues with Wilson Poon, a scientist well on the right (in both senses) side of the gaussian in Fig. 2.

FIG. 2. Main figure: Average Hirsch index $h$ as a function of the square root of the number of published papers $n_p$, for a set of 470 scientists co-authoring the top 10% cited papers published by Phys. Rev. Lett. in 2012, fitted as $h_{\text{teo}} = (2.72 \pm 0.05)n_p^{1/2} - (2.5 \pm 0.5)$. The dependence on $n_p$ of the “success ratio” $h/n_p$ is shown in the upper inset. The lower inset gives the relative frequency distribution of the quantity $h/h_{\text{teo}}$ for the whole set of investigated authors, fitted with a gaussian of standard deviation $\sigma = 0.23$.

FIG. 3. Frequency distribution of the $h$-index for the author set shown in Fig. 2 fitted with a Gamma distribution $\Gamma(h; \alpha = 2.27, \beta = 11.0)$. The inset displays the frequency distribution for the subset of about 150 authors belonging to large collaborations.

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[1] J. E. Hirsch, Proc. Natl. Acad. Sci. U. S. A. 102, 16569 (2005).

[2] Admittedly, scientists publishing in PRL are already a
rather selected group: those physicists that can boast many papers in this prestigious journal, still a reference in our community, is probably a minor fraction. Nevertheless, the latter arguably includes also those young scientists we may wish to consider for a position.

[3] According to the traditional Italian saying “fatta la legge, gabbato lo santo”, which roughly means “once the rule is established, the saint is duped”.

[4] At least, $\sqrt{c}$ is obviously an upper bound for $h$.

[5] Such a test, which could be easily made by ISI or Scopus, would likely yield a larger dispersion of $h$ around $h_{teo}$.

[6] I leave it to the reader to brood over the origin of this peculiar distribution, suggesting that sampling over the PRL authors is basically a random Poisson process. For the aims of this paper, it is sufficient to note that the distribution covers a wide spectrum of values for the $h$-index, fairly representing both young postdocs and ageing professors like me.

[7] CUN official declaration to the Ministry on the Evaluation of the Quality of Research (VQR), 16 July 2013.