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Editorial

A Call for a Quite Debate on the Future of Genetics Research

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On 20th April 2009, Steve Jones wrote in the Telegraph that modern genetics research gave hope to patients suffering from diseases such as diabetes, cancer, multiple sclerosis and a variety of brain disorders, conditions could be blamed on a small set of common genetic variants which could be tracked down, scanned using a magic "chip" to enable further understanding of what had gone wrong, diagnose patients before symptoms appeared, and perhaps come up with a few cures.[1]

In the same article, Jones, in a pessimistic glance of genetics research outcome in this respect gave examples of how limited the "clinical" yield of genetics research is, however, he also gave examples of its success. Jones finally concluded his article with a statement that leaves us with the impression that he believes that the outcome of genetics research was ineffective as far as clinical practice is concerned. He stated that "… for their mountain (the genetics research) has labored and brought forth not much than a mouse!" Surprisingly to some, or perhaps not, Steve Jones is a professor of genetics at the University College of London. Hence, in line with what he stated, the authors believe that it is time to debate the long-term plan of genetics research.

On 20th May 2010, Craig Venter (father of the Human Genome Project) reported the design, synthesis and assembly of the 1.08-Mbp Mycoplasma mycoides JCVI-syn 1.0 (JVC are Craig Venter's initials.).[2] The microorganism is also known as the "chemically synthesized genome" or as Venter is fond of saying "… the first self-replicating species that we've had on the planet whose parent is a computer."[3] The "chemically synthesized genome" was achieved at a great expense ($40 million), and effort (20 people working for more than a decade).[4] Confidently, to make the assembled genome be unquestionably recognizable as synthetic, the computerized DNA sequence contained strings in bases that, in code, spell out an e-mail address, the names of many of the people involved in the project, and a few famous quotations.[4]

Subsequently, after many years of hope, genetics research may have been directed not towards providing answers to obstipated human diseases, rather it was directed towards commercial exploitation as forecasted by Venter.[5]

Verily, this new technical milestone, together with Jones' article, should accentuate a thorough discussion in the form of debate on whether modern genetics research has fulfilled its promise in finding solutions to tenacious human ailments. In the light of this debate, one should expect a review of the future plan and goals of this research field. Based on this background, the authors suggest the following proposition for this debate; Should our genetics research effort be addressed to directly serve our clinical needs?

At this point, it is important to declare that the authors have no intention to undermine the research effort on genetics or the knowledge it has accumulated over the years. Rather, it is meant to appraise genetics research output (relative to the input) in terms of clinical applications, thus we may optimize its cost-effectiveness and direct it to where it is most needed.

Research, as we believe, is an "investment" in the full sense of the term including monetary, effort and time outlays. If the concept of "research is investment" is accepted then, any research endeavor success should be measured by how much return was made from its initial expenditure; again, in terms of finance, effort and time.

Indisputably, there is a solid move towards establishing a long-lasting research platform on genetics in most institutes. It will be wise to account for how much funding has been injected both in the short- as well as the long-terms. Subsequently, we should identify how much of such "investment" has been translated into clinical application? And, how can we optimize its cost effectiveness?

To appraise any effort as an established practice in all renowned scientific communities is perceived and welcomed as a healthy exercise. From this standpoint, Moses et al. analyzed biomedical research funding in the USA during the period 1994 until 2004, which was mostly spent on genetics and neuroscience research, and noted that the following challenges are required to be brought about to enhance the productivity and benefit of biomedical research:[6]

1. More effective translation of basic scientific knowledge to clinical application
2. Critical appraisal of rapidly moving scientific areas to guide investment where clinical need is greatest
3. More specific information about sources and uses of research funds than is generally available to allow informed investment decisions.[6]

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If it is accepted that these challenges are as applicable to countries other than the USA, then our call for debate should also be accepted; a debate that should be conducted on the light of these challenges. Appropriately and evenly, such action should also be extended to all other biomedical research fields.

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