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RE: Impact of COVID-19 on mental health research: is this the breaking point?

Research and COVID-19: why join the club?

25 July 2022

Despite considerable funding, few psychiatrists could point to genetic or neuroimaging studies that have changed day-to-day clinical practice over the past three decades. UK psychiatric epidemiology is unhealthy and national mental health data-sets are difficult, often impossible, to access. Health service research is either spurned as low impact or endlessly repeated in meta-analyses supporting service interventions which are visibly failing patients in the field. Sparasci and colleagues[1] highlight progressive national closures of research departments, with psychiatry subsumed under other university departments. Failure to deliver hoped-for breakthroughs to lead psychiatry into the 21st century will not be resolved by asking the wrong research questions. Could psychiatric academia have so little political influence in the real world because it increasingly aligns itself with populist ideologies with no evidence base?

COVID caused others to seriously re-evaluate the value of psychiatric research. Psychiatric studies were shut down because money was needed for a pandemic threatening to overwhelm the National Health Service. Government (but not academic psychiatry) soon grasped that it was not so much the virus posing threats to the nation's public mental health but mass unemployment and economic hardship. Yes, there were real neuropsychiatric harms from the virus. But exaggerating them to obtain grant funding failed to benefit psychiatry, and such funding was inevitably awarded to disciplines with relevant skills in population health sciences, public health, neurology and infectious diseases. Lack of new ideas and methods were exposed in a 'position' paper at the start of the COVID pandemic, aimed to set the agenda and capture grants.[2] Where are those recommendations now? None was substantially funded in psychiatry. The ultimate 'give away' that psychiatry was far out of its depth with COVID was rigid insistence that all future studies include full involvement from people with 'lived experience'[2] – for a potentially deadly viral illness.

The editorial[1] is important because it makes us question future research directions and academics' career prospects. Academic psychiatry has become like a steadily declining, formerly exclusive club, not readily admitting new members unless they share the same ideas and values, with old members steadily replaced by more compliant younger ones, controlled by a ruthless management. Why would anyone young want to join the club if it means lifetime earnings disparity compared with clinicians, pressures to achieve the impossible in terms of grants and impact factors and adhering to an exploitative career model of an 'independent researcher', with the myth of becoming self-funding through research grants? Like exclusive clubs, universities are businesses and psychiatrists are expensive commodities, easily replaced by other members of disciplines such as psychologists who are plentiful, better trained in research at undergraduate level and often desperate for jobs. Few will ever become research professors and most will spend majority of their academic time teaching. This brings in greatest earnings for universities where, for some, research is a loss leader to attract undergraduates. And, as committee membership lists increasingly show, it is now easy to obtain a visiting or honorary chair from a university and call yourself 'professor' with no discernible research credentials.

Declaration of interest

None

References

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2 Holmes EA, O'Connor R, Perry VH, Tracey I, Wesseley S, Arsenault L, et al. Multidisciplinary research priorities for COVID-19 pandemic: a call for action for mental health science. *Lancet Psychiatry* 2020; **7**: 547–60.

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RE: Extending the vulnerability–stress model of mental disorders: three-dimensional *NPSR1* × environment × coping interaction study in anxiety

No evidence that *NPSR1* is involved in anxiety

15 January 2021

A recent report claimed that a variant, rs324981, of the neuropeptide S receptor gene (*NPSR1*) modulated the relationships between childhood trauma, self-efficacy and trait anxiety, but the analyses performed were so seriously flawed as to render the conclusions completely invalid.[1]

The authors fail to mention in the abstract the main finding, which is that they found no association at all between rs324981 genotype and anxiety. Nor did they find an effect of genotype on anxiety in two-way interaction analyses. They only claimed an effect when genotype was included in a three-way interaction term. It is utterly implausible that a real effect would appear in this situation. The reason these results have appeared is clear from Figure 1, which shows that the apparent relation is driven by a handful of outliers which, purely by chance, have a similar configuration in the discovery and replication samples.

The authors report an unfeasibly small P-value of 4×10^{-8} to support their conclusion. This is simply a consequence of treating the values as if they followed a Gaussian distribution when they clearly do not. The linear regression analysis implemented in SPSS carries out an analysis of variance to obtain a P-value, and this analysis of variance assumes that the variables are normally distributed. The departure from normality does not prevent linear regression analysis from producing a least squares fit, but it does mean that the statistical significance of the findings cannot be assessed.