Our field demands big data and our membership has proven again and again that when it is available, we use it. We use it to address fundamental questions: How many Canadians have a diagnosable mental disorder? Is access to care driven by need, or is it influenced by non-need factors such as class or gender? What is the global burden of mental disorder?

I remember, not so long ago, the excitement felt on the eve of the release of our first national mental health survey—the Canadian Community Health Survey 1.2. The same enthusiasm is in the air again as the second national prevalence study is almost ready to be released. Our membership has been involved in other projects, too. Led by Michael Boyle from McMaster, the next Ontario Child Health Study—the first large-scale epidemiological study of child mental health in this country since the original OCHS back in 1983—was recently funded by CIHR.

In the next five years, we will have national and provincial epidemiological data on mental disorder and addictions spanning early childhood through to later life. Some of this data will be linkable to provincial health administration data. In Ontario, our members (Dr. Paul Kurdyak, Dr. Simone Vigod and yours truly) are responsible for leading a new program of mental health service and population health research using administrative data through ICES (Institute for Clinical Evaluative Sciences). The ability to link population survey data to utilization data in Canada’s largest province will keep the program busy for many years.

Interesting new questions will no doubt be posed and answered by our members (and colleagues): has the prevalence of disorder (children and adults) changed or remain stable over time? Has there been a narrowing or widening of socioeconomic gaps in mental disorder? As large numbers of baby-boomers continue to age, has the prevalence of disorder in older adults changed?

Big questions require big data. We will be rich in such data for the next few years, and CAPE members will continue to play a critical role in the analysis of population mental health and addictions data and in the dissemination and translation of this work.

John Cairney, Ph.D.
CAPE President
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The Age of Melancholy: Is It or Isn't It? And If It Isn't, It Ought To be

Jane M. Murphy, Ph.D.
Professor of Epidemiology and Psychiatry at Harvard University & Director of the Stirling County Study from 1975 onwards.

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We speak, of course, about depression. Think of all the adversities people are experiencing: war casualties, unemployment, dwindling economic resources, fragile marriage ties, etc., etc. Also we hear much about the increased use of antidepressant medication. And the Global Burden of Disease estimates suggests that before long depression will be second only to ischemic heart disease in its burden.

This is a good question for psychiatric epidemiology! The answer tends to depend on the methods used. Here we focus on diagnostic rather than dimensional measurement.

When structured diagnostic approaches became available in the early 1980s, several papers suggested an increasing rate of depression. These came from the Epidemiologic Catchment Area (ECA) study and similar investigations (1,2). They were based on samples chosen to represent a population at one point in time and the analysis focused on retrospective lifetime rates of depression: "In your lifetime, have you ever had two weeks or more when you felt sad, blue, depressed, etc.?" Younger people reported higher lifetime rates than older people thereby suggesting that depression was on the rise.

Among some investigators, such an interpretation was dismissed due to recall bias (3). To others, the evidence seemed convincing and for a period of time this latter view got support from a study of psychiatric incidence. Incidence and lifetime prevalence are essentially the same in that each is intended to give evidence of a first depression over a defined period of time. The difference is mainly that incidence is calculated after an initial investigation has indicated that the person had never had a depression up to that time. Incidence is a little less "retrospective" in this regard.

Often considered the "father of psychiatric incidence", the Lundby Study in Sweden conducted clinical interviews with a population in 1947 and again in 1972. The evidence suggested that an "age of melancholy" was in evidence with a rising incidence of depression (4). But for the second period comparing 1972 and 1997, the increase appeared to have abated (5).

Another epidemiologic strategy, one that has a number of strengths, involves drawing new samples of the same population periodically and studying them mainly in terms of current prevalence or one-year prevalence. As far as we know the earliest study to use this approach was the Stirling County Study in Atlantic Canada.
There, across samples drawn in 1952, 1970, and 1992, the overall rate of depression remained stable (6).

This strategy has recently been used in a number of other investigations. They have involved drawing from 2 to 4 consecutive representative samples from the mid-1990s into the 21st century in Canada, Britain, and South Australia (7,8,9). They have shown little or no increase. In the U.S., the National Comorbidity Survey (1990-2003) reported no increase when all types of psychiatric disorders were grouped together but this was without specific reference to depression (10). The National Epidemiologic Survey of Alcohol and Related Conditions (1991-2001) reported a 3% increase of depression with the interesting feature that mood disturbance remained stable but the full diagnosis increased (11). It was pointed out regarding the latter that "direct to consumer advertising" about antidepressants occurred over the interval of follow-up and may have influenced the findings. The Baltimore Follow-up Study, while not drawing new samples, reported both incidence and prevalence rates with the latter remaining stable over three periods between 1981 and 2004 (12).

While the answer to "is it or is it not?" still does not have a definite answer, the weight of evidence seems to be on the "not".

It bears emphasis in this regard that depression appears to be a disorder of extremely complex etiology with some such disorders having greater input from life experiences and others from genetic composition and many, if not most, having an admixture. It seems reasonable to suggest that a disorder of complex etiology would not change its rate drastically over relatively short periods of time. And further, if changes did occur, they might pertain to given age and gender segments of a population and not to the population as a whole.

In this regard, it is of interest that the Stirling, Baltimore, and South Australia studies all point to a modest increase among younger women, an increase offset by somewhat lower rates among other age/gender segments.

Such age/gender findings raise interesting and potentially researchable questions: are the "slings and arrows of outrageous fortune" descending more forcefully on younger women than on others? If so, are some of their disorders especially influenced by environmental forces? Can we learn anything useful about distinguishing between depressions of different etiologies by addressing this issue?

8. Spiers N, Brugha TS, Bebbington P, McManus S, Jenkins R, Meltzer H. Age and birth cohort differences in depression in repeated cross-

EDITORIAL
Questioning the Cost-Benefit of Research Ethics Boards

Discussion about ethics reviews of research involving human subjects does not seem to have yet reached serious levels in Canada. However, there are frequent concerns raised by some researchers and other stakeholders that local research ethics boards (REBs) often interfere with research projects by stipulating activities that incur delays, increase costs, and/or change the quality and nature of the study that has been proposed.

Generally speaking, each university and research institute has one or more research ethics boards that review research proposals to ensure that they conform to ethical treatment of human and animal subjects. While board practices differ somewhat, they have all agreed to follow the general framework and stipulations set out by the Tri-Council Policy Statement (TCPS). This document was produced collaboratively by the Medical Research Council of Canada (now the Canadian Institutes of Health Research), the Social Sciences and Humanities Research Council of Canada, and the Natural Sciences and Engineering Research Council of Canada.

Adherence to TCPS guidelines is not a legal issue in a direct sense. But the Government of Canada has mandated a system whereby funding by the three research councils is contingent on adherence to the TCPS research guidelines by the organization sponsoring the applicant (Grubisic 1998) - mainly Canadian universities. Of course, research institutes that are free-standing and are not supported by funding from any of the three major funding bodies can carry out investigations with impunity. However, many of the journals that publish health research require submissions to include a indication of approval from a local ethics review body to be noted in all articles to be considered for publication. Furthermore, health-related organizations that plan to allow scientists to gain access to their data often stipulate that a successful ethics review is a prerequisite for study commencement. The TCPS, in effect, conveys very effective control.

The stated founding value of the TCPS, respect for human dignity, has been operationalized as three core principles that serve as the basis for its guidelines. These are (1) concern for welfare, (2) respect for autonomy; and (3) respect for the equal moral status of all humans.

These basic values seem unassailable on the surface of things, but disagreements with this model of ethics review have arisen. Arguments have been made that boards have often shown incompetence and unfairness, and have imposed unreasonable and/or bureaucratic requirements. More specifically, a review of a summit meeting...
about IRBs (the American version of ethics research boards) produced a report indicating that concerns included censorship of topics, inconsistent decision-making, harassment, lack of accountability, biases, ineffective communication, research incompetence, unrealistic risk-assessment, lack of an appeal process, and a focus on participants’ rights to the neglect of scientific merit.\(^3\)

These concerns can be categorized into three broad groupings; (1) unfairness, (2) problems with the competence of board operations, and (3) the lack of balance between potential harm to subjects and potential benefits to the population at large. It is noteworthy that the third (balance) would remain an issue even if the first two (unfairness and competence) were deemed to be resolved.

Is the functioning of research ethics boards a problem for Canadian psychiatric epidemiologists involved in research? It remains to be seen, but the amount of informally expressed concern, coupled with the research findings noted above, suggests that a more detailed investigation of the issue would be in order. To that end, one possible series of lock-step investigations could include the following:

I. A brief survey of Canadian psychiatric epidemiologists to determine difficulties associated with ethics reviews, with details on the three above-noted issues (fairness, competence, and balance) and economic estimates of costs, including opportunity costs.

II. A literature review surrounding the detection of the need for ethics reviews in Canada, including pivotal cases (where research subjects were harmed as a consequence of participation in health research), their extent, and the development of purpose statements for research boards.

III. An analysis of Canadian research ethics board summary statistics to provide a picture of the number of health-related projects that are handled by Canadian universities on an annual basis, their nature, and the proportion that are passed, revised, etc.

IV. A cost-benefit analysis of randomly selected research ethics board submissions in Canada over a specified period. This could involve the recruitment of principal investigators that have applied for a REB review. Each could be asked to provide responses on the nature of the study, economic issues, and proposal disposition at key times (prior to the first review, following approval of the study or at abandonment, and at a suitable follow-up time after commencement of the study).

In the end, conclusions will be able to be drawn that will address:

1. The evidential basis for research ethics review,
2. a framework for evaluation of REB activity,
3. The extent and nature of REB activity in Canada.
4. The utility of REB activity in Canada

References


Angus H Thompson, Ph.D.
Editor
You are invited to Ottawa for CAPE’s 2013 Annual Scientific Symposium. Keynote speaker, Dr. Stephen Gilman, Harvard School of Public Health, will deliver an address that suits our theme perfectly (A developmental pathological stress response model of depression). The day will also include oral and poster presentations from researchers and students across the country. This year we will have one oral session devoted to student presentations, and seven moderated poster sessions. Another highlight of the day will be the presentation of the Alexander Leighton Prize and the winner’s address. It’s a program that you will find stimulating.

Registration for CAPE 2013 is formally closed, but a very few spaces are available. If interested, please contact Dr. Guérin (below) ASAP.

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The Conclusions of This Paper Are Probably Wrong
David L. Streiner, Ph.D., C.Psych

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Did you just get a grant from CIHR? Congratulations, but you should probably return the funds. Had a paper accepted by a journal? Marvelous, but ask the editor if you can withdraw it. The reason is that you do not want to be complicit in spreading even more erroneous results on an unsuspecting public. If John Ioannidis is to be believed, the majority of what is published is just plain wrong. In a widely cited paper (Ioannidis, 2005), he states that Simulations show that for most study designs and settings, it is more likely for a research claim to be false than true. Moreover, for many current scientific fields, claimed research findings may often be simply accurate measures of the prevailing bias. He attributes this to a number of factors, including small sample sizes, small effect sizes, unplanned analyses (i.e., data dredging), flexibility in choosing research designs, the research area being a hot one, and having a financial interest in the results (although it’s not clear how he simulated this). There’s no question that each of these factors can result in Type I errors; the question is why researchers should be surprised that the number of published papers in the literature with Type I errors exceeds 5%. Let me review just a few of them.

1. Publication bias. This exists at two levels. First, it is less likely that studies with negative findings will be submitted to a journal than those with positive results (e.g., Cooper et al., 1997). Then, even if you have the chutzpah to send in such a paper, it is less likely that it will be accepted for publication (Begg & Berlin, 1988). Compounding the problem, many journals have a policy that they will not publish replications (especially if they come up with negative results), the self-correcting nature of science is subverted to some degree.

2. Problems with null hypothesis significance testing (NHST). Jake Cohen, in his article with the delightful title of The earth is round ($p < .05$) (Cohen, 1994) raised the issue of problems with NHST, and he was far from the first to do so. Indeed, I daresay that the number of articles on this topic is surpassed only by the number discussing Justin Bieber’s latest escapade. One major problem is that NHST is answering a question that no one is asking. Basically, it answers the question, What is the probability of these (or more extreme) data, given that the null hypothesis ($H_0$) is true? or, in statistical jargon, $P(D|H_0)$. However, what we really want to know is $P(H_0|D)$; that is, What is the probability that $H_0$ is true, given our data? and the problem is that $P(D|H_0) \neq P(H_0|D)$. His solutions, adopted by the American Psychological Association (Wilkinson and the Task Force on Statistical Inference, 1999), include less reliance on a dichotomous significant/not significant decision and greater use of confidence intervals, effect sizes, and graphical presentation of findings.

3. Misinterpretation of the probability of replication. The last issue I want to mention is the probability of replicating a significant finding. Let’s assume you do a study and find that the results are significant at exactly $p = .05$. What is the probability that if you repeat exactly the same study the same sampling frame, same sample size, same method, and same analysis you will
again find a significant result? If you said 95%, you have the honour of belonging to the majority of people. Unfortunately, the majority of people are wrong; the correct answer is 50% (Norman & Streiner, 2008; Posavac, 2002). With the first study, $H_0$ can be pictured as a normal curve, with a mean of $d = 0$, where $d$ is the difference between the group means. Out in the tails are critical values (CVs), beyond which differences are statistically significant. For the replication, though, the $H_0$ curve is centered over the CV, so that the right half reflects even more extreme differences, but the left half is closer to the original $d = 0$. So, assuming that the results are normally distributed, half will be non-significant.

So, many published findings are indeed false. But this does not reflect cupidity or stupidity, but simply the way science operates. It ain’t perfect, but it’s what we have and we have to learn to be skeptical until results are replicated.

References

Editor’s note: See Button et al. on bias in neuroscience 2013 http://www.nature.com/nrn/journal/v14/n5/full/nrn3475.html