Abortion and mental health: guidelines for proper scientific conduct ignored

We have serious concerns about the methodology of the quantitative synthesis by Coleman1 and want to highlight these to prevent readers and policy makers drawing erroneous conclusions, in particular the incorrect statement that ‘nearly 10% of the incidence of mental health problems was shown to be attributable to abortion’.

This quantitative synthesis and meta-analysis did not follow the robust methodologies now generally accepted for systematic reviews.2 There is no detail of the search strategy including search terms; the strategy is not comprehensive (only two databases included); other strategies to search the literature, including citation tracking, hand searching and contacting authors and experts in the field to try to minimise publication bias, were not carried out; and there was no assessment or rating of the quality of included studies, so that only those of at least reasonable quality are included in the meta-analysis. This is particularly important here as many of the primary studies included in this review have significant methodological limitations, including non-prospective design, non-standardised measures of mental disorders, lack of adjustment for pre-existing mental illness, lack of adjustment for other key confounders (e.g. social deprivation), non-comparability of exposed and non-exposed groups, and selection bias. This is especially concerning, given that previous reviews raised serious methodological concerns about some of the included studies, and came to different conclusions when these were excluded from analyses.3–5 Furthermore, results from several of the included studies linking abortion to mental health problems have since been re-analysed by other researchers. These studies, using the same data, have less biased sample selection techniques and control for pre-pregnancy factors known to influence poor mental health outcomes (i.e. rape history) and have found no significant links between abortion and subsequent poor mental health.6,7

A recent population-based cohort study conducted in Denmark published in the New England Journal of Medicine this year confirmed this. Munk-Olsen et al8 reported no difference between the incidence of first psychiatric contact before and after abortion. Importantly, the incidence of psychiatric contact is higher among women who underwent abortion; this is the result of a selection phenomenon and not a causal association because this relationship is evident before the abortion occurred.4 This is evidence that women having induced abortions constitute a population with higher pre-existing psychiatric morbidity.

Study selection and evaluation should be carried out by two independent raters; the Coleman review was carried out by one author only. Of the 22 papers included, 11 were authored by Coleman, the author of the review. This is a conflict of interest, and undermines the author’s ability to critically review the primary studies.

Finally, the synthesis of the data and the summary statistics are flawed. The criteria for synthesising data meant that several effect measures were included from the same study. Eleven of the included studies contributed more than one effect measure, with two studies contributing four measures each. Despite the clustering of effect measures by study, they are analysed as independent measures. This is an important limitation, since the use of several effect measures from a flawed study can magnify the bias.

Most importantly for readers of this study to know, is the erroneous conclusions drawn by the author regarding the population attributable risk (PAR). The underlying assumptions for estimating PAR include that there is a causal relationship between the risk factor (abortion) and the disease (mental ill health) and that there is independence of the considered risk factor from other factors that influence disease risk.9 These assumptions are clearly not met in this review and therefore it is completely inappropriate to calculate a PAR from these data.

Abortion and mental health is a politicised issue – it is therefore essential that research in this field is methodologically robust.

Declaration of interest

T.M.-O. was the lead author of the population-based cohort study cited in this letter.

References


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There appear to be many methodological as well as logical inconsistencies and interpretational difficulties with the report of Coleman,1 which might have diminished reviewers’ enthusiasm for its conclusions. Many of these have already been addressed by previous correspondence. We believe, however, that one methodological problem that has not yet been raised – the use of the population attributable risk (PAR) measure – is very important and merits comment. This might help readers avoid misunderstanding this study, and also other studies where the PAR is used.
In Coleman’s synthesis, the PAR measure has been applied inappropriately and, we believe, reported misleadingly. For example, the reported PAR for completed suicide is particularly high at 35%. For several reasons, readers should not interpret this figure as meaning that over a third of all suicides among women of reproductive age could be prevented if none of them underwent abortion. An inherent assumption in the PAR is that all other things would remain equal after the removal of a risk factor, which is clearly not true for abortion in this instance. Further, the aetiology of suicide is extremely complex, and in most cases cannot be attributed to a single adverse life event that is the one measured in a particular study. Women who die by suicide at some time following an abortion are likely to carry multiple distal and proximal risk factors as they proceed along their life course, as is true for most people of any age or gender who die by suicide, and it is fallacious to suggest that abortion can be isolated from other causal factors in these limited data-sets.

Second, in the first paragraph of the Discussion (p. 183), Coleman states with apparent certainty that ‘…nearly 10% of the incidence of mental health problems was shown to be directly attributable to abortion.’ This is about as unambiguous a statement of causality as could possibly be made, in the face of clear guidance on the potential pitfalls of drawing such conclusions when applying the PAR.2 Having stated the causality of the association with such certainty, the author then appears to backtrack in her concluding remarks (pp. 185–186) by making the following ambiguous statement, clearly contradicting the view expressed at the start of her Discussion:

‘Although an answer to the causal question is not readily discerned based on the data available, as more prospective studies with numerous controls are being published, indirect evidence for a causal connection is beginning to emerge.’

Following publication of just such a ‘prospective study with numerous controls’ in the New England Journal of Medicine in 2011,3 it might be appropriate for Priscilla Coleman (and colleagues supportive of her views) to reconsider their conclusions. This recent study3 provides the best data available from the largest unbiased sample on the association (or lack thereof) between excess risk of mental illness and abortion because that study is based on a large population sample, with measurement of mental illness both before and after the abortion event study. That study ‘does not support the hypothesis that there is an increased risk of mental disorders after a first-trimester induced abortion’ (quoted from abstract).


In her review of research on the mental health effects of abortion, Coleman1 stated:

‘In this highly politicised area of research it is imperative for researchers to apply scientifically based evaluation standards in a systematic, unbiased manner when synthesising and critiquing research findings. If not, authors open themselves up to accusations of shifting standards based on conclusions aligned with a particular political viewpoint. Moreover, the results may be dangerously misleading and result in misinformation guiding the practice of abortion.’

However, Coleman failed to follow well-accepted scientific standards for the conduct and reporting of systematic reviews and meta-analyses. Further, Coleman’s failure to state her obvious conflicts of interest in this review raises serious questions about biases in her analysis. Hence, the review is open to serious questions about the author’s scientific standards, methods, political viewpoints, and potentially misleading conclusions.

Widely accepted standards for systematic reviews and meta-analyses are contained in the published AMSTAR, MARS, MOOSE, and PRISMA statements.2-5 None of these standards were cited or followed by Coleman. AMSTAR is the only validated instrument for assessing the methodological quality of systematic reviews and meta-analysis. We assessed Coleman’s review according to the AMSTAR statement, and found that it failed to meet any of the eleven basic requirements for systematic reviews and meta-analysis included in AMSTAR.

Following AMSTAR, specific flaws of the Coleman review are as follows:

1. there was no public a priori design
2. there was no duplicate study selection or duplicate data extraction
3. the author did not describe the search strategy in sufficient detail
4. the review was limited to published studies, contrary to all published standards
5. a list of excluded studies was not provided
6. the author did not provide sufficient descriptive information on included studies, including demographic characteristics of participants
7. the scientific quality of included studies was not documented
8. scientific quality of included studies was not considered in formulating conclusions
9. appropriate methods were not used in combining the findings of studies (Coleman clearly violated the rule for avoiding dependencies in meta-analysis, when she synthesised 36 effects from 22 studies in Fig. 1)
10. the likelihood of publication bias was not assessed
11. conflicts of interest and sources of support were not acknowledged (no financial disclosures were made and no other potential conflicts were acknowledged).

An article in the British Journal of Psychiatry5 calls attention to the importance of non-financial conflicts of interest in the psychiatric literature. Coleman has at least two types of conflict of interest here. Among the most important of such conflicts is an agenda-driven bias, by which authors seek to influence legislation and social policy. David Reardon is a co-author with Coleman on seven articles included in the review and an author on an additional study in the review that does not involve Coleman as a co-author. Reardon is quite explicit about his agenda to instil fear of abortion as a way of facilitating passage of anti-abortion legislation.7

Coleman is the first author on 6 studies and co-author on 5 additional studies in her review; thus, she authored or co-authored fully half of the 22 studies included. According to the Cochrane Handbook,8 this is another potential conflict of interest, since it may ‘unduly influence judgements made in a review (concerning, for example, the inclusion or exclusion of studies, assessments of the risk of bias in included studies or the interpretation of results) . . . This should be disclosed in the review.
and, where possible, there should be an independent assessment of eligibility and risk of bias by a second author with no conflict of interest. Coleman did not obtain an independent assessment of the studies she authored or co-authored, nor did she acknowledge these conflicts in the review.

Coleman’s conclusion that the results of the studies in her review are ‘quite consistent’ (p. 183) is belied by visual inspection of the Forest plots, which include non-overlapping confidence intervals. Coleman should have reported results of heterogeneity tests (chi-squared and I²), which probably would have shown significant heterogeneity in results across studies (presumably that is why she chose the random effects model).

Some of the commentaries on Coleman’s review appear to be uninformed by current scientific standards for reviews. Comments by Fergusson et al are particularly misleading. Faced with variations in the methodological quality of available studies, it is essential for reviewers to weed out weaker studies. Valid conclusions can only be based on valid studies.

It is unclear how this paper got through peer review at the *Journal*. It appears that peer reviewers and the Editor ignored published standards for systematic reviews and meta-analyses. Given the serious methodological flaws contained in Coleman’s review and the author’s failure to report obvious conflicts of interest, we believe the article should be retracted.

Coleman’s recent meta-analysis¹ ignores guidelines for proper scientific conduct of meta-analyses of observational data. Her results violate at least three major principles of meta-analysis: she fails to assess the underlying validity of included studies; she fails to examine statistical heterogeneity; and she illogically combines estimates for distinct outcomes. Furthermore, she accuses previous reviews of lacking ‘reasonable justification’ or carefully conducted. However, it is essential that a summary estimate be accompanied by a qualitative description of risk of bias in included studies (which Coleman’s review lacked) since ‘potential biases in the original studies, relative to biases in RCTs, make the calculation of a single summary estimate of effect of exposure potentially misleading.’²

Coleman ignores other essential requirements of a high-quality statistical meta-analysis. She makes no attempt to present a replicable search strategy or article selection diagram. She attempts to justify excluding articles prior to 1995 by noting that study methodology has improved, but fails to adequately justify selected cut-off dates. Ultimately, she includes multiple methodologically weak studies, and excludes at least two older but methodologically stronger studies. She authored her review alone, despite Cochrane and PRISMA recommendations to involve multiple reviewers to reduce the possibility of investigator bias or error.³

Coleman makes disingenuous accusations about previous reviews. For example, she claims that our 2008 systematic review⁶ ‘overlooked’ ten articles which met inclusion criteria, and ‘lacked sufficient methodologically based selection criteria.’ This unfounded attack is puzzling, particularly since in 2008, we directly emailed to Coleman the reasons (consistent with our methodologically based selection criteria detailed on p.437) for excluding seven of these ten articles. The remaining three (not previously enquired about) also fail to meet inclusion criteria: two had a follow-up period of less than 90 days and the other compared medical v. surgical termination.

Coleman continues to ignore the scientific importance of accounting for pregnancy intention in this body of literature. If women who abort (many of which are unintended pregnancies) are compared against women who deliver (many of which are intended pregnancies), effects of unintended pregnancy are difficult to disentangle from effects of abortion. Circumstances surrounding an intentional v. an unintentional conception or pregnancy may be related to mental health outcomes. Most aborted pregnancies in the USA were unintended. Coleman wrongly assumes that since nearly half of pregnancies in the USA are unintended, most births are too, failing to acknowledge that almost half of unintended pregnancies end in abortion.⁸ Thus, her assertion that ‘the majority of women in the control groups in studies comparing abortion with term pregnancy actually delivered unintended pregnancies even if the variable was not directly assessed’ has no empirical grounding. Similarly, her assertion that a ‘no pregnancy’ group may be a ‘cleaner’ comparison group ignores the fact that the ‘no pregnancy’ group would not have experienced unintended pregnancy.

The scientific validity and rigour of Priscilla Coleman’s work has been questioned before. However, we are surprised and disappointed that the multiple egregious scientific errors in her

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Meta-analysis of observational data can be useful when carefully conducted. However, it is essential that a summary estimate be accompanied by a qualitative description of risk of bias in included studies (which Coleman’s review lacked) since ‘potential biases in the original studies, relative to biases in RCTs, make the calculation of a single summary estimate of effect of exposure potentially misleading.’²

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The scientific validity and rigour of Priscilla Coleman’s work has been questioned before. However, we are surprised and disappointed that the multiple egregious scientific errors in her
review went undetected by the editorial or peer-review process of the British Journal of Psychiatry.


Professor Coleman’s systematic review and meta-analysis of the literature on termination of pregnancy and mental health featured several significant omissions: an undisclosed conflict of interest; no assessment of publication bias; and no assessment of the quality of studies included. The search strategy was also inadequately reported, and the meta-analytic technique was faulty.

First, the paper states ‘Declaration of interest: None’. We believe this is incorrect. It seems that Professor Coleman is an anti-abortion campaigner, who has previously expressed the view that campaigning should include work in academic journals. For example, in a Powerpoint presentation on the website of the American Association of Pro-Life Obstetricians and Gynecologists, Professor Coleman states: ‘We need to develop organized research communities to continue the research, apply for grants, recruit young academics, critique data produced by pro-choice researchers, challenge politically biased professional organizations, train experts to testify, and disseminate cohesive summaries of evidence.’

The British Journal of Psychiatry has committed to the International Committee of Medical Journal Editors’ uniform requirements for declaration of conflict of interest. This requires the declaration of ‘any relevant non-financial associations or interests (personal, professional, political, institutional, religious, or other) that a reasonable reader would want to know about in relation to the submitted work.’ As noted in a recent editorial, ‘the difficult words here are “personal”, “relevant” and “reasonable”’. Given the role that campaigning has played in this issue, we believe this conflict of interest should have been declared to readers.

Second, unusually for a systematic review and meta-analysis, there was no attempt to account for the role of publication bias in the findings. We have replicated the meta-analysis by importing Coleman’s data into Stata 11. After verifying that the summary odds ratios and confidence intervals produced were identical, we went on to create a funnel plot (Fig. 1) using metafunnel. This found evidence strongly suggestive of publication bias in the literature presented. We further used Egger’s test using the metabias command in Stata 11, and again found very strong evidence suggesting publication bias (P < 0.0001).

Finally, we note that there was only one assessor for the studies, and several of the included studies had more than one outcome, which were used in the meta-analysis as if they were independent observations.

We believe that as a result of these features the paper falls far short of best practice in the execution of publication-standard meta-analyses.

Declaration of interest

B.G. writes newspaper articles and books on problems in science, and has written three times previously about flaws in evidence used to campaign for changes in UK legislation to reduce access to termination of pregnancy, once online and twice in print, out of approximately 2000 pieces published. Neither author is religious, neither has a history of engaging on the issue of termination of pregnancy beyond that mentioned here.


4 Tyrer P. From the Editor’s desk. Br J Psychiatry 2010; 196: 86.


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We strongly disagree with the conclusions of Coleman’s analysis of research about the relation between abortion and mental health. An earlier study by Munk-Olsen et al.,2 not mentioned in the study, concluded that, contrary to what is generally assumed, a first-trimester induced abortion was not followed by an increase in mental disorders. The strength of the study is that mental health problems are studied in women before and after an induced abortion, and not only after. From Dutch primary care data,3 we can confirm this in a case–control study in family practice, we compared the medical history of women 3 years before and 3 years after they had an induced abortion with a control group.4 Differences were found with regard to mental health (visits for mental health problems, psychopharmaceutical prescriptions or referrals to mental health facilities). However, compared with the control group, women who had an induced abortion had more social problems. This should be an important focus of attention in the care of women who choose to have an abortion.


The study by Coleman1 and the following comments may offer a further useful point of view to the bioethical debate. Irrespective of moral judgement, in the majority of cases abortion is performed by physicians to protect women’s mental health from an unintended/unwanted pregnancy or birth, but as a minimum what we can say is that evidence does not support any beneficial effect on women’s mental health as a result of having an abortion. On the public health level, abortion may therefore be considered no more than a procedure satisfying criteria for futility.2,3 On the individual level, any abortive procedure should be instead preceded by an in-depth analysis of the various factors known to interfere with the psychological outcomes. But as far as we know this is almost never the case. If women’s health is what abortion providers intend to preserve, they should accept a substantial revision of their protocols under the assistance of skilled psychiatrists.


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Editors’ response: The article by Coleman1 was submitted in October 2010 and accepted for publication in March 2011, so predated the Munk-Olsen paper,2 as Coleman has indicated in

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her reply below. The handling editor was W.W. and the paper was accepted after revision with two reviewers supporting publication and one recommending rejection. It was recognised that the paper was likely to attract attention and P.T. suggested that a commentary should be published alongside the article. Unfortunately the major concurrent work on this subject (commissioned by the Department of Health) had not then been completed and it was felt unfair to delay publication, so the article appeared without comment. Dr Coleman stated that she had no conflicts of interest to declare and when invited to revise this view subsequently when reminded of our guidance again reiterated this. She has again defended this in her letter; readers are free in the light of these full statements to come to their own conclusions. The failure to declare an interest is not a reason for retracting a systematic review even if failure was unequivocally demonstrated, and this situation is very different from other ones in which the publication of a paper has been retracted.5 We have nevertheless decided to give new guidance for the preparation of reviews in our authors’ instructions so there is greater clarity for both authors and reviewers. The correspondence and commentary in this issue indicates the importance of the subject and the value of an active correspondence column in a journal; it is not a reason to avoid the publication of a controversial subject.


Author’s reply: In the barrage of recent letters, the sentiments have varied widely and the many supportive arguments presented are worthy of additional comment; however, given space limitations, I have decided to focus on the criticisms to help ensure the results are given the attention deserved.

There are some comments that I believe are without basis and may not have been made with a more careful, less emotional read of the article. For example, Littell & Coyne suggested that scientific standards for systematic reviews were not followed. The protocol employed is detailed in the methodology section and the strategy was in line with recommendations in the Handbook of Research Synthesis and Meta-Analysis. Rather than focus on these types of comments, I address criticisms requiring more information from me to allow readers to make informed decisions regarding the merit of the issues raised.

The studies included in the meta-analysis have a relatively high degree of heterogeneity given the demographic and cultural differences in sampling, the variability in control groups and outcomes, and differences in third variable controls. Counter to the claim by Polis et al, heterogeneity was addressed by employing a random effects model. The random effects model yields an estimate of the mean of a distribution of true effects; whereas in the fixed effects model there is an assumption that all the included studies share one common effect. When assigning weights to studies in a fixed effects model, the smaller ones are afforded less importance, since the same effect is believed to be more precisely assessed in the larger studies. In contrast, in the random effects model, individual studies of varying sizes contribute data from distinct populations, all of which must be considered in the pooled estimate. Weighting is therefore more balanced in the random effects v. fixed effects model, with smaller studies given relatively more emphasis. In recognition of the heterogeneity, I not only employed the random effects model, but I ran separate meta-analyses based on distinct comparison groups and outcomes.

Goldacre & Lee provided a funnel plot analysis and presented it as evidence of publication bias. However, the funnel plot is largely inappropriate for heterogeneous meta-analyses, wherein studies are not likely from a single underlying population,1–4 and several investigators have warned that use of funnel plots with meta-analyses derived from heterogeneous samples may result in false-positive claims of publication bias.1–4 When funnel plot asymmetry is detected in a heterogeneous meta-analysis, the cause is likely to be essential differences between the smaller and larger studies. For example, the majority of the smaller studies included in my meta-analysis employed substance use outcome variables and these outcomes tend to yield the strongest, most robust effects.5,6 In addition, the larger studies were more likely than the smaller studies to include actual diagnoses for disorders, rarer events than cut-off scores on single surveys. In the context of this meta-analysis, the funnel plot most certainly does not provide evidence of publication bias.

My experience attempting to locate unpublished data/studies on abortion and mental health has been very disheartening over the past 15 years, with virtually all requests ignored. I suspect that reluctance to share unpublished data is an attempt to keep results that challenge contemporary views on abortion and indicate significant increased risks for adverse psychological effects out of the public domain. In contrast, I believe energy is likely invested in seeing to it that non-significant findings, suggesting abortion carries no increased psychological risks, find their way into the journals. If there is any topic wherein many editors, researchers and professional organisations are highly motivated to publish non-significant effects, it is this one, rendering publication bias less common than in other areas. Support for this notion can be found in the American Psychological Association’s (APA) 42-year history of abortion advocacy.

In 1969, the APA passed a resolution which made the pro-choice political position the organisation’s official stance and declared abortion a civil right. For decades the APA has aligned itself with major organisations with pro-choice social agendas, frequently submitting amicus briefs and providing congressional testimony. Martel7 recently discussed the APA’s position on abortion, among other issues, noting that the organisation’s stance has led them to promote psychological research and disseminate data to lawmakers to inform the public and advocate for societal change. Martel further pointed out that the political stance of the APA lacks the strong backing of empirical data. With this long history of abortion advocacy by the strongest professional psychology organisation in the world, politically motivated efforts to publish null findings to support and legitimise their position is logical.

As indicated under the methodology section of the meta-analysis, studies identified using the Medline and PsycINFO databases were included based on sample size, comparison groups, outcome variables, controls for third variables, use of odds ratios, and publication in English in peer-reviewed journals between 1995 and 2009. In an effort to isolate the effect of abortion on mental health, use of comparisons groups and controls for third variables are basic methodological requirements consistent with the Bradford Hill criteria.8 The majority of studies meeting these criteria and incorporated into the meta-analysis also had many other strong methodological features (multiple data points, nationally representative samples, etc.). I purposely avoided selecting from among the many more peripheral methodological criteria that could be argued as a necessary basis for including or excluding studies, when there is not universal agreement regarding...
strengths necessary to consider a study’s results sufficiently reliable and valid, nor is there consensus on the particular deficiencies necessary for the wholesale dismissal of a study.

Using the criteria outlined above, a significant proportion of the included studies (11/22) were articles that I authored or co-authored. However, having published 33 peer-reviewed articles, I believe I am more widely published on this topic than any other researcher in the world. It makes sense, therefore, that I am a co-author on a significant proportion of the included studies. Moreover, no studies satisfying the inclusion criteria were left out of the analyses.

Curiously the issue of my not including a study by Danish researchers Munk-Olsen et al published in the New England Journal of Medicine⁶ was raised despite the fact that the paper came out long after my meta-analysis was completed and submitted for publication. Incidentally, their paper is presented as offering more reliable conclusions than the meta-analysis.

However, there are several problematic features of this study. To begin with, Munk-Olsen et al note that previous studies lack controls for third variables, but the only third variables they consider are age and parity. There are no controls for pregnancy intendedness, pregnancy induced stress, mental health problems during pregnancy and after abortion, or other traumas. Many studies have been deemed inadequate based on only one of these variables not being accounted for (see APA Task Force Report⁵). The data indicated that rates of mental health problems were higher after abortion compared with childbirth (15.2% v. 6.7%); however, the generally comparable rates before and after abortion were used to negate a possible causal link between abortion and mental health. This reasoning is problematic as many women were likely disturbed to the point of seeking help precisely because they were pregnant and contemplating an abortion or they were involved in troubled relationships. These factors may have resolved, yet disturbance rates remained elevated because of the impact of the abortion. Further, the Danish Civil Registration System contains over 40 years of data, the group that did not request termination were retained. A major study of abortion and dissemination of information through the usual channels. I do not have many graduate students working with me or large grants, and it is alarming that a researcher with such modest resources was the first to conduct a major quantitative review.

Rather than hurling unfounded accusations of personal bias, we need to more effectively utilise the well-established methods of science to fairly scrutinise the methodologies of individual studies, expand the empirical investigation of abortion and mental health, and develop a consensus-based standardised set of criteria for ranking studies meriting inclusion in reviews. Without agreement, the selected standards may be used to manipulate conclusions. For example, the ranking system employed by Charles et al¹¹ ignored two central methodological considerations in prospective research designs: (a) percentage consenting to participate (no information was provided by the authors of the Gilchrist et al¹² study that this team ranked as ‘Very Good’); and (b) retention of participants over time. In the Gilchrist et al study, only 34.4% of the termination group and 43.4% of the group that did not request termination were retained. A major problem with nearly all the recently published narrative reviews was somewhat arbitrary exclusion criteria at best and the purposeful selection of specific criteria resulting in dismissal of large bodies of evidence with politically incorrect results at worst.

By raising concerns of publication bias and attempting to undermine the credibility of an individual researcher who managed to publish in a high-profile journal, several people have sought to shift attention from the truly shameful and systemic bias that permeates the psychology of abortion. Professional organisations in the USA and elsewhere have arrogantly sought to distort the scientific literature and paternalistically deny women the information they deserve to make fully informed healthcare choices and receive necessary mental health counselling when and if an abortion decision proves detrimental.

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7 Martel MM. The ethics of psychology’s role in politics and the development and institution of social policy. Ethics Behav 2009; 19: 103–11.

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Tony Zigmond's editorial about 'leverage' in English mental healthcare helps to demonstrate the promotion of coercion without appeal (as may be even more common in the USA) resulting from an apparent contradiction in local mental health law:

"Even when taking medication is a condition attached to a conditional discharge (from a restriction order, Mental Health Act 1989) or community treatment order, it would be unlawful to recall the individual solely because the individual decided to refuse the medication." 

Presumably at the time of conditional discharge (including the requirement that they continue prescribed medication) the person is well enough to cope with the requirement. They are coerced (by stated conditions), hopefully, because it has been demonstrated that without medication they are highly likely to become unwell again.

So what is different about the situation where the person is out of hospital and stops their medication, that they cannot be legally coerced to take it? Has the person changed in some way when they face a similar clinical risk?

Is it better to have a legal provision for this coercion outside the hospital, with all the necessary legal safeguards and reviews required (as under the Australian New South Wales Mental Health Act 2007) or to await decompensation and have caring people desperately trying to forestall such an event, possibly with illegal (and possibly inappropriate) 'leverage' pressures?

The article does not address the often characteristic accompanying distress to the person and their network when a person decompensates into more disorganised psychosis. Perhaps 'he died with his rights on'.

In the Australian New South Wales Act, intervention requires not only the 'least restrictive' option, but also the 'continuing condition' of a mentally ill person and risk of ANY harm. In other words, if the history indicates a chronic illness and the likelihood of decompensation off medication, this must be considered, even it the person is apparently getting by at the time.

Perhaps failure to initiate sensitive intervention, including coercion as necessary, with legal safeguards and appeal capacity, in such circumstances, is negligence. Such intervention may also be regarded as a 'loving act'.

To say that a person has the capacity to make a decision does not determine the quality or appropriateness of that decision.

To say that a person is 'capacious' because they are capable of apparently understanding their situation (and so 'having the capability of making decisions about their own treatment') and have 'insight' is to leave open much definitional argument, but it is not appropriate as the sole determinant of non-intervention in a situation of carefully determined clinical risk to the person and the legitimate distress of others.

In asking 'Who should have the final say?', Zigmond seems to assume that this question is resolved by choosing either the patient or the psychiatrist. In matters of coercion it is the law that has the final say and at its best it seeks to accommodate distress to the person and their network when a person decompensates into more disorganised psychosis. Perhaps 'he died with his rights on'.

Legal leverage

First, choice is shaped and essentially dependent on insight. Correlates of insight are no longer restricted to anosognosia-like views or neuropsychological dysfunction based on injuries to frontal, parietal or temporal lobes. Volumetric reductions in several cortical regions evinced by neuroimaging studies are well documented in chronic schizophrenia and first-episode psychosis. Consequently, lack of insight, unawareness of illness and the need for treatment can no longer be relegated to the domain of choice. Those involve a network of brain structures affected by the disorder. Even the law recognises this in assigning the 'but for mental disorder' designate in various medico-legal criteria.

Second, medico-legal provisions of treatment are unfortunately driven by public reactions to failures in the mental health system. The law is then made and takes precedence in determining the acceptable level of risk to which the society should be exposed. This is known as 'where the public peril begins'. In recognition of the implications of the choices made for and against treatment, the tension between autonomy and beneficence has not stopped being the most contentious of ethical quagmires.

Third, the rates of coercion cannot categorically be said to be increasing, as opined by Zigmond. To modify the perceived alarming statistics he referenced, other relevant factors should be recognised alongside the increasing number of formal compulsory hospital admissions (1987–2010). The population of English society has not only increased over the past three decades, it has become more diverse with migrants who affect rates of mental disorder as well as the potential for coercion. The tolerance for mental illness and societal acceptance is changing. Community support for patients, a by-product of economic prosperity, has dwindled, contributing to coercive approaches from both family and practitioners. Although litigation of practitioners is lower in the UK than in the USA, it is nonetheless relevant in determining physicians' attitudes towards voluntariness.

Finally, when the treatment of our patients considers the past and the future, progress in reducing stigma necessitates the use of leverage in some situations. The high rates of the use of leverage have been suggested as a good thing. The Americans have experienced a 'before and after' paradigm of reducing leverage. 'Rotting with their rights on' was not only a title of scientific publications but was the terminology used to describe the repercussions. The UK should learn from this that there are tragic and costly consequences in focusing only on choice. Despite decades of that experiment in the USA, the opinion to use leverage as a positive tool still exists.
10. Kress K. Rotting with their rights on: why the criteria for ending commitment or restraint of liberty need not be the same as the criteria for initiating commitment or restraint of liberty, and how the restraint may sometimes justifiably continue after its prerequisites are no longer satisfied. Behav Sci Law 2006; 24: 573–98.

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Author’s reply: We thank Patra & Subodh for their interest in and their very thorough reading of our study. Most of their questions are factual ones and we are grateful that we have been given an opportunity to clarify them. First, whether to call further treatment of patients in partial or full remission after the fully syndromic episode, as in our study, continuation/maintenance treatment is a terminological issue and not a further treatment of patients in partial or full remission after the fully syndromic episode. The study has sound methodology (i.e. randomised controlled trial) with adequate masking, in addition to being the first ever study to examine the effect of family psychoeducation for major depressive disorder. The authors describe the possible limitations of the study honestly. A source of funding (Grant-in-Aid for Scientific Research, Ministry of Health, Labour and Welfare, 2004) is also mentioned. However, there are some issues which should be further looked into. The aim was to examine family psychoeducation in the maintenance treatment of depression. However, the patients included were either on continuation or maintenance treatment. Patients who are in partial remission cannot be considered as being in a continuation/maintenance phase. Also it was not mentioned how many patients had single or recurrent episodes (patients with single episodes need not receive maintenance phase treatment). Any other psychiatric comorbidity (substance misuse or personality disorder) in the participants was not mentioned, although it has treatment implications. The health status and intellectual functioning of the primary family member included in the study was not mentioned, although these might compromise their active participation in psychoeducation sessions. The authors are silent on the ethical clearance of the study. For four caregivers psychoeducation sessions were done in the individual’s home and not in group sessions, which were not included in the final analysis and not part of the methodology mentioned – this could also have had an effect on the efficacy of the study. Remission was defined by the authors as a Hamilton Rating Scale for Depression (HRSD) score <6, but the normal score is mentioned as <7. The authors have not mentioned the reason for keeping a low score of HRSD in the study. Last, it could be a printing mistake, in the last line of Table 1 it is mentioned that high expressed emotion (as per FMSS) was seen in seven patients in the intervention group and none in the control group, but the results mentioned that it is seen in six patients in the intervention group and ten in the control group.

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Author’s reply: Thank you to Drs Scott-Orr and Mela for their interest. It seems to me that there are two issues here. First, should the law be discriminatory between patients with a physical illness and those with a mental illness? I think not and I’m pleased to say the United Nations Convention on the Rights of Persons with Disabilities (2006), to which the UK is a signatory, supports this view. The convention obligates States to (among many other things) ‘take all appropriate measures, including legislation, to modify or abolish existing laws, regulations, customs and practices that constitute discrimination against persons with disabilities’. To explain this further, the UN High Commissioner for Human Rights said,¹ ‘Legislation authorizing the institutionalization of persons with disabilities on the grounds of their disability without their free and informed consent must be abolished . . . This should not be interpreted to say that persons with disabilities cannot be lawfully subject to detention for care and treatment or to preventive detention, but that the legal grounds upon which restriction of liberty is determined must be de-linked from the disability and neutrally defined so as to apply to all persons on an equal basis.’

Second, should the law (for everyone) favour patient autonomy, medically determined best interest or a mixture? In other words, either everyone, with the capacity to make the decision, should be permitted to ‘die (or rot) with their rights on’ or nobody should. Or the authority to overrule capacitous refusal could be based on a neutral factor such as risk to other people. It should not be dependent on the stigma associated with certain terminology (a mental illness diagnosis).

2. United Nations. Newsam Centre, Leeds LS14 6WB, UK. Email: zigmond@doctors.net.uk
doi: 10.1192/bjp.200.1.82

Family psychoeducation for major depression: randomised controlled trial

The paper by Shimazu et al adds robustness to already existing evidence for the role of family psychoeducation in psychiatric disorders. The study has sound methodology (i.e. randomised controlled trial) with adequate masking, in addition to being the first ever study to examine the effect of family psychoeducation for major depressive disorder. The authors describe the possible limitations of the study honestly. A source of funding (Grant-in-Aid for Scientific Research, Ministry of Health, Labour and Welfare, 2004) is also mentioned. However, there are some issues which should be further looked into. The aim was to examine family psychoeducation in the maintenance treatment of depression. However, the patients included were either on continuation or maintenance treatment. Patients who are in partial remission cannot be considered as being in a continuation/maintenance phase. Also it was not mentioned how many patients had single or recurrent episodes (patients with single episodes need not receive maintenance phase treatment). Any other psychiatric comorbidity (substance misuse or personality disorder) in the participants was not mentioned, although it has treatment implications. The health status and intellectual functioning of the primary family member included in the study was not mentioned, although these might compromise their active participation in psychoeducation sessions. The authors are silent on the ethical clearance of the study. For four caregivers psychoeducation sessions were done in the individual’s home and not in group sessions, which were not included in the final analysis and not part of the methodology mentioned – this could also have had an effect on the efficacy of the study. Remission was defined by the authors as a Hamilton Rating Scale for Depression (HRSD) score <6, but the normal score is mentioned as <7. The authors have not mentioned the reason for keeping a low score of HRSD in the study. Last, it could be a printing mistake, in the last line of Table 1 it is mentioned that high expressed emotion (as per FMSS) was seen in seven patients in the intervention group and none in the control group, but the results mentioned that it is seen in six patients in the intervention group and ten in the control group.
Second, of the 57 patients who entered the trial, 27 had had a single episode of depression and 30 recurrent major depression. Those with their first lifetime major depressive episode received continuation treatment after their acute phase treatment. The comorbidities of our sample included three individuals with substance use disorder, ten with anxiety disorders and one with personality disorder. It is true that we did not methodically check the intellectual or health status of the primary family members but none apparently suffered from serious dysfunction in these regards. We regret the fact that we did not mention the ethics review committee’s approval in the published paper. It had been reviewed and approved by the ethic review committee of Kochi Medical School in 2003, prior to the commencement of the study. In addition, it is not true that four caregivers had their psycho-education sessions individually: only 4 out of 16 sessions for these four family members were conducted individually either in the hospital or at home. Altogether, 4 out of 216 sessions (1.9%) were individually delivered. We reasoned that in this first-ever efficacy trial of family psychoeducation for major depression, it would be better for us to deliver the best therapy possible. Our definition of remission reflected some arguments that the accepted threshold might be too high to define true remission. Changing the threshold for remission to 7/8, instead of 6/7, made little change to the findings.

Last, we would like to clarify that there were six (25%) families with high expressed emotion in the intervention group and ten (33%) in the control group.

Family psychoeducation for major depression: randomised controlled trial
Bichitra N. Patra and B. N. Subodh
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