Dr. Kam Bhui

Editor-in Chief, BJPsych

September 8, 2022

Dear Dr. Bhui,

At your request, I am submitting this document providing my response to the letter to the British Journal of Psychiatry Editorial Board received June 17, 2022, calling for retraction of my article, "Abortion and mental health: quantitative synthesis and analysis of research published 1995-2011" published in the Journal in 2011. Points raised in the letter and in the analysis by a panel of experts comprised of Editorial Board members are addressed.

The retraction letter and the two-paragraph analysis by the panel are largely based on sweeping unsupported generalities lacking a defensible connection to my study. In contrast, I address each issue raised and provide a detailed analysis of the inappropriateness and irrelevance of the specific challenges to the conduct and write-up of my study, with references to peer-reviewed articles and other highly credible sources as needed.

Prior to launching into my full rebuttal, I provide a synopsis of how well-received my article was in the original peer-reviewed process under the direction of Dr. Peter Tyrer, by the Journal following publication, and by scholars. In addition, I offer evidence of work by researchers whose results are consistent with mine and were published in high impact journals.

Following my rebuttal of the concerns leveled against the article, I offer evidence of a reproductive rights, pro-choice bias among the signatories and explain my perspective on why the attacks on my work have been so baseless and relentless. I further provide evidence that I have not been the only recipient of this form of bullying due to publishing research results that run counter to a political agenda.

Every one of the signatories is a highly experienced researcher and they all know their accusations are spurious. Their motivation in contacting you is not about the science; it is about discrediting me as a researcher and expert witnesses for political reasons. My meta-analysis has informed many of the opinions I have offered under oath in litigation in the United States. Two of the individuals who are signatories to the letter demanding a retraction (Dr. Biggs and Dr. Steinberg) have served as experts on the opposing side in several cases. Further, five of the signatories of the retraction request are Turnaway Study researchers. On numerous occasions, I have provided expert testimony regarding the serious methodological flaws of this study. I recently published a paper, "A Case of Self-Correction in Science Upended by Political Motivation and Unvetted Findings" in Frontiers in Psychology (Coleman, 2022). This outlet is the top-ranking journal in all of Psychology according to Google (https://scholar.google.com/citations?view_op=top_venues&hl=en&vq=med_psychology). The article was published less than 3 months ago and there have been 6422 views as of today, exceeding 82% of all Frontiers articles from their 140 journals over the last 12 months.

I. Section 1: Reception of the Article and Alignment of Results with an Expansive Peer-Reviewed Literature

A. Submission and Following Publication

When my paper was submitted for publication in 2011, Dr. Tyrer sent it out to three individuals for review and I completed two rounds of receiving feedback and responding prior to acceptance. Compared to other articles I have submitted for publication over the last three decades, few changes were deemed necessary. I distinctly remember one reviewer noting the review was a long overdue contribution to the literature, with no discussion of the methodology other than to say the rules for selection and synthesis were appropriate. A second reviewer had only minor suggestions to add clarity, and a third reviewer voiced concerns regarding a perceived discrepancy between the data reported in my quantitative review and his/or her own clinical experiences among other very generalized comments, each of which I addressed to the satisfaction of Dr. Tyrer upon resubmission.

After the article was published, there were positive and negative letters to the editor. These letters are obviously in the realm of commentary and are not peer-reviewed. Dr. Tyrer allowed a discussion to ensue and published my response. This type of discourse is precisely how science advances, not by pulling a solid article that passed peer-review over a decade ago with replicable findings that challenge the dominant political narrative, simply because a group of vocal ideologues issued a high-pressure demand with no scientific merit.

B. A Decade of Informing Policy

In the years following publication, I presented the findings to many interested audiences in the U.S. and abroad, including presentations in parliament houses in Great Britain, Northern Ireland, New South Wales, and Queensland. Further, I included the results of the meta-analysis in expert reports for numerous civil cases in state and federal courts throughout the U.S. In every case, my opinions were admitted into evidence. Among the cases wherein the results of my meta-analysis factored into my opinions are the following:

JANE DOE NO. 1; JANE DOE NO. 2; JANE DOE NO. 3; WILLIAM MUDD MARTIN HASKELL, M.D.; CASSIE HERR, N.P.; KELLY MCKINNEY, N.P.; and WOMEN'S MED GROUP PROFESSIONAL CORPORATION, Plaintiffs, v. ATTORNEY GENERAL OF INDIANA; COM-MISSIONER OF THE INDIANA STATE DE-PARTMENT OF HEALTH; MEDICAL LI-CENSING BOARD OF INDIANA; INDIANA STATE BOARD OF NURSING; and MARION COUNTY.

AMERICAN COLLEGE OF OBSTETRICIANS AND GYNECOLOGISTS et al., Plaintiffs, vs. UNITED STATES FOOD AND DRUG ADMINISTRATION, et. al., Defendants.

Affidavit filed with AMICUS CURIAE BRIEF OF FORMER ABORTION PROVIDERS; THE NATIONAL ASSOCIATION OF CATHOLIC NURSES, U.S.A.; AND THE NATIONAL CATHOLIC BIOETHICS CENTER IN SUPPORT OF RESPONDENT, June Medical Services LLC v. Gee, SUPREME COURT of the UNITED STATES.

PLANNED PARENTHOOD ASSOCIATION OH UTAH, Plaintiffs v. JOSEPH MINER, et al, in the UNITED STATES DISTRICT COURT FOR THE DISTRICT of UTAH.

REPRODUCTIVE HEALTH SERVICES OF PLANNED PARENTHOOD OF THE ST. LOUIS REGION, INC., on behalf of itself, its physicians, its staff, and its patients, and COLLEEN P. MCNICHOLAS, D.O., M.S.C.I., F.A.C.O.G., on behalf of herself and her patients, Plaintiffs, v. MICHAEL L. PARSON, in his official capacity as Governor of the State of Missouri; ERIC S. SCHMITT, in his official capacity as Attorney General of the State of Missouri, et al., Defendants, in THE UNITED STATES DISTRICT COURT FOR THE WESTERN DISTRICT OF MISSOURI CENTRAL DIVISION.

WHOLE WOMAN'S HEALTH ALLIANCE; ALL- OPTIONS, INC.; and JEFFREY GLAZER, MD, Plaintiffs v. CURTIS HILL, Attorney General of Indiana, in his official capacity et al., Defendants, in the UNITED STATES DISTRICT COURT FOR THE SOUTHERN DISTRICT OF INDIANA INDIANAPOLIS DIVISION.

CAITLIN BERNARD, M.D., et al., Plaintiffs, v. THE INDIVIDUAL MEMBERS OF THE INDIANA MEDICAL LICENSING BOARD, et al., in the UNITED STATES DISTRICT COURT, SOUTHERN DISTRICT, INDIANAPOLIS DIVISION.

ADAMS & BOYLE, P.C., on behalf of itself and its patients; et al., Plaintiffs, v. HERBERT H. SLATERY III, Attorney General of Tennessee, in his official capacity; et al. In the United States District Court for the MIDDLE DISTRICT OF TENNESSEE NASHVILLE DIVISION.

COMPREHENSIVE HEALTH PLANNED PARENTHOOD GREAT PLAINS, et. al., Plaintiffs, v. JOSHUA D. HAWLEY, in his official capacity as Attorney General of Missouri, et. al., Defendants. In the Circuit Court of Jackson County, Missouri at Kansas City.

GAINESVILLE WOMAN CARE LLC d/b/a BREAD AND ROSES WOMEN'S HEALTH CENTER, on behalf of itself, its doctor, and its patients; and MEDICAL STUDENTS FOR CHOICE, on behalf of its members and their patients, Plaintiffs, v. STATE OF FLORIDA; FLORIDA DEPARTMENT OF HEALTH; JOHN H. ARMSTRONG, M.D., in his official capacity as Secretary of Health for the State of Florida et al. In the Circuit Court of the Second Judicial Circuit in and for Leon County, Florida.

C. Current British Journal of Psychiatry Data on the Quality of Interest in the Article

The BJP describes the success of the article with various indicators on its website as detailed below. These data attest to the unusually high level of quality interest in the article. With the enormous amount of attention that the meta-analysis has garnered from academics, clinicians, and government officials, it is important to note that no one has ever performed a re-analysis of the data and found discrepant results. All data necessary to do so is in the public domain and could have been done at any point by critics over the last decade. Clearly the article has met an extremely high bar of public scrutiny.

a) https://www.cambridge.org/core/journals/the-british-journal-of-psychiatry/most-read

This page identifies the top ten most read articles in the journal based on the number of full text views and downloads over the last month and it is updated every day. When accessed on September 3rd, my article had received the highest Altmetric attention score from among the ten listed articles.

b) https://www.cambridge.org/core/journals/the-british-journal-of-psychiatry/article/abortion-and-mental-health-quantitative-synthesis-and-analysis-of-research-published-19952009/E8D556AAE1C1D2F0F8B060B28BEE6C3D#metrics

This page explains the elements that factor into the Altmetric attention score and is reproduced in part below.

Altmetric attention score

Picked up by **27** news outlets
Blogged by **6**
Tweeted by **668**
On **34** Facebook pages
Referenced in **7** Wikipedia pages
Mentioned in **4** Google+ posts
Reddited by **4**
Mentioned in **1** Q&A threads
281 readers on Mendeley
1 readers on CiteULike

Full text views

Total number of HTML views: 38439

Total number of PDF views:14288

Abstract views

Total abstract views: 113205

The above data is updated every 24 hours.

c) https://cambridge.altmetric.com/details/374123#score

This page is generated from the tab, "Attention Score in Context" from the previous page. Information relevant to my meta-analysis is captured below.

"This research output has an Altmetric Attention Score of 848**.** This is our ***high-level measure of the quality and quantity of online attention that it has received.*** This Attention Score, as well as the ranking and number of research outputs shown below, was calculated when the research output was last mentioned on 31 August 2022**."**

ALL RESEARCH OUTPUTS

#16,107 of 21,941,598 outputs: ***Top .07%.***

OUTPUTS FROM BRITISH JOURNAL OF PSYCHIATRY

#8 of 5,925 outputs: ***Top .14%***

ALL OUTPUTS OF SIMILAR AGE

#32 of 92,546 outputs: ***Top .03%***

OUTPUTS OF SIMILAR AGE FROM BRITISH JOURNAL OF PSYCHIATRY

#1 of 19 outputs: ***Top 5.26%***

"Altmetric has tracked 21,941,598 research outputs across all sources so far. Compared to these this one has done particularly well and is in the 99th percentile: it's in the top 5% of all research outputs ever tracked **by Altmetric."**

The following affirming tweet was posted by BJPsych Journals regarding my article on February 11th, 2020.

https://twitter.com/TheBJPsych/status/1227281159266197504?s=20&t=4oxhJYPl1Eej22FWR7XCrw

"...there has been considerable debate among #academics regarding the extent to which #abortion poses serious #mentalhealth risks to #women." Find out more in the #BJPsych's most read article by Prof Priscilla Coleman."

@CambUP_Psych

#WomenInScience

D. Academic Citations to the Article Over the Years

According to google scholar, my article has been referenced 311 times by authors of articles in many journals with high impact factors. Exemplars are offered below.

> Coelho, F. M., Pinheiro, R. T., Silva, R. A., de Ávila Quevedo, L., de Mattos Souza, L. D., de Matos, M. B., Castelli, R. D., & Pinheiro, K. A. (2014). Parental bonding and suicidality in pregnant teenagers: a population-based study in southern Brazil. *Social psychiatry and psychiatric epidemiology*, *49*(8), 1241–1248. https://doi.org/10.1007/s00127-014-0832-1 (IF= 2.537)
>
> Fergusson, D. M., Horwood, L. J., & Boden, J. M. (2013). Does abortion reduce the mental health risks of unwanted or unintended pregnancy? A re-appraisal of the evidence. *The Australian and New Zealand journal of psychiatry*, *47*(9), 819–827. https://doi.org/10.1177/0004867413484597 (IF=5.598)
>
> Kulathilaka, S., Hanwella, R., & de Silva, V. A. (2016). Depressive disorder and grief following spontaneous abortion. *BMC psychiatry*, *16*, 100. https://doi.org/10.1186/s12888-016-0812-y (IF=3.388)

Pinheiro, R. T., da Cunha Coelho, F. M., da Silva, R. A., de Ávila Quevedo, L., de Mattos Souza, L. D., Castelli, R. D., de Matos, M. B., & Pinheiro, K. A. (2012). Suicidal behavior in pregnant teenagers in southern Brazil: social, obstetric and psychiatric correlates. *Journal of affective disorders*, *136*(3), 520–525. https://doi.org/10.1016/j.jad.2011.10.037 (IF=4.839)

E. Consistency of My Meta-Analysis Results with Other Quality Research

The results of my meta-analysis indicating women who undergo an abortion are at an increased risk for mental health problems are consistent with an expansive world literature entirely ignored by the signatories. Scientific studies documenting this association are published in leading peer-reviewed journals in psychology and medicine. There are now dozens of large-scale prospective studies with 1000s of participants incorporating different types of comparison groups and a variety of control techniques, effectively fortifying the level of confidence in the results derived. Potentially confounding variables controlled in the various studies include prior mental health, reproductive history, experience of abuse of various forms, and several demographic variables thereby increasing the reliability and validity of the findings.

The table below summarizes the results of several of the more recent large-scale investigations from across the globe. In the 2018 study by Luo and colleagues (2018), wherein the primary outcome was suicidal ideation, a stronger association was observed between abortion history and suicidal ideation among women who did not have pre-existing anxiety or depression. The authors noted, "The stronger association among those without anxiety or depression further corroborates our inference that induced abortion was associated with suicidality independent of mental disorders among this population." (p. 7). Luo concluded, "An improvement of mental health of the population requires policy change, medical system support, enhanced communication between the service seekers and health care providers." (p. 10). Another notable, recently published study in the table below is by Jacob and colleagues (2019b) and is based on over 35,000 German women with data derived from gynecological practices where diagnoses are continuously documented, enabling unbiased exposure assessment (no recall bias). In this study, significant associations between abortion history and several psychiatric disorders were identified. The authors concluded, "Based on these results, information on the potential impact of induced abortion on mental health should be given to women before the abortion procedure is scheduled." (p. 78). Finally, and arguably of most importance with reference to the current discussion, is Sullins' (2016) extremely well-controlled study incorporating a nationally representative sample, wherein he reported a population attributable risk (PAR) of 8.7%, close to the 9.9% PAR derived in my meta-analysis.

Table 1: Recently Published Large Scale Research Studies on the Association between Abortion and Mental Health

Study	**Results**
Gong, X., Hao, J., Tao, F., Zhang, J., Wang, H., & Xu, R. (2013). Pregnancy loss and anxiety and depression during subsequent pregnancies: Data from the C-ABC study. European Journal of	Large Chinese study (over 20,000 women), 7683 of whom had an abortion. Abortion was related to increased risk of depression (OR: 1.381) and anxiety (OR: 1.211) in the first trimester of a later pregnancy after controlling for age, education, pre-pregnancy

Obstetrics, Gynecology, and Reproductive Biology, 166(1), 30–36.	MBI, income, and residence. The comparison group was women experiencing a first pregnancy.
Gissler, M., Karalis, E., & Ulander, V.M. (2015). Decreased suicide rate after induced abortion, after the Current Care Guidelines in Finland 1987-2012. Scand J Public Health, 43(1), 99-101.	Examined suicide post-abortion between 1987 and 2012 in Finland. A 2-fold increased risk of suicide was observed even after new guidelines required post-abortion follow-up sessions at 2-3 weeks to monitor women's mental health.
Jacob, L., Gerhard, C., Kostev, K., & Kalder, M. (2019a). Association between induced abortion, spontaneous abortion, and infertility respectively and the risk of psychiatric disorders in 57,770 women followed in gynecological practices in Germany. Journal of Affective Disorders, 251, 107–113.	Case-control study from the Disease Analyzer Database (IQVIA). Induced abortion was positively associated with the elevated risk of psychiatric disorders (ORs ranging from 1.75 to 2.01).
Jacob, L., Kostev, K., Gerhard, C., & Kalder, M. (2019b). Relationship between induced abortion and the incidence of depression, anxiety disorder, adjustment disorder, and somatoform disorder in Germany. Journal of Psychiatric Research, 114, 75–79.	Examined women with a first abortion in 281 gynecological practices in Germany. Included 17581 women with an abortion experience and 17581 matched controls who had a live birth. Induced abortion predicted depression (HR=1.34), adjustment disorder (HR=1.45), and somatoform disorder (HR=1.56) across the 10 year study period.
Lega, I., Maraschini, A., D'Aloja, P., Andreozzi, S., Spettoli, D., Giangreco, M., Vichi, M., Loghi, M., Donati, S., & Regional Maternal Mortality Working Group (2020). Maternal suicide in Italy. Archives of Women's Mental Health, 23(2), 199–206.	Data were gathered from 10 regions in Italy. The suicide rate was 1.18 per 100,000 among women who gave birth (n = 2,876,193) and 2.77 among women who aborted (n = 650,549), a statistically significant difference.
Luo, M., Jiang, X., Wang, Y., Wang, Z., Shen, Q., Li, R., & Cai, Y. (2018). Association between induced abortion and suicidal ideation among unmarried female migrant workers in three metropolitan cities in China: A cross-sectional study. BMC Public Health, 18(1), 625.	Examined 5115 unmarried females from Shanghai, Beijing, and Guangzhou. Abortion was associated with nearly double the odds of suicidal ideation (OR = 1.89) after adjustment for numerous controls (age, education, years in the working place, tobacco use, alcohol consumption, daily internet use, attitude towards premarital pregnancy, multiple induced abortion, self-esteem, loneliness, depression, and anxiety disorders.) The association was stronger in those aged > 25 (OR = 3.37), among women with > 5 years in the work force (OR = 2.98), in the non-anxiety group (OR = 2.28, and in the non-depression group (OR = 2.94).

McCarthy, F. P., Moss-Morris, R., Khashan, A. S., North, R. A., Baker, P. N., Dekker, G., Poston, L., McCowan, L., Walker, J. J., Kenny, L. C., & O'Donoghue, K. (2015). Previous pregnancy loss has an adverse impact on distress and behaviour in subsequent pregnancy. BJOG: An International Journal of Obstetrics and Gynaecology, 122(13), 1757–1764.	Women with one prior abortion had elevated stress (adjusted mean difference=0.65) and depression (aOR= 1.25) at 15 weeks of gestation. Women with two prior abortions had increased perceived stress (adjusted mean difference=1.43) and depression (aOR=1.67).
Sullins D. P. (2016). Abortion, substance abuse and mental health in early adulthood: Thirteen-year longitudinal evidence from the United States. SAGE Open Medicine, 4,	In a U.S. sample, after extensive control for other pregnancy outcomes and sociodemographic variables, abortion was associated with increased overall risk of mental health disorders (OR:1.45). A Population Attributable Risk analysis showed 8.7% of the prevalence of mental disorders was attributable to abortion.
Wie, J. H., Nam, S. K., Ko, H. S., Shin, J. C., Park, I. Y., & Lee, Y. (2019). The association between abortion experience and postmenopausal suicidal ideation and mental health: Results from the 5th Korean National Health and Nutrition Examination Survey (KNHANES V). Taiwanese Journal of Obstetrics & Gynecology, 58(1), 153–158.	After adjusting for several demographic controls, women who had three abortions experienced elevated risk for suicidal ideation (OR: 1.510). This level of risk was significant even after controlling for depression (OR: 1.391). Risk of depressive mood in daily life was likewise elevated with more abortions even after controlling for depression (OR: 1.657).

In a 2013 narrative review of literature published between 1995 and 2011, incorporating 30 peer-reviewed journal articles by Italian researchers Bellieni and Buonocore, the authors concluded "The studies analyzed here show that abortion is a risk factor for mental illness when compared to childbirth." Udzma, and Achadi (2019) published an analysis of studies examining factors related to depression in pregnancy and they reported that a maternal history of abortion was a significant factor in four out of the six studies examined. There have been several others over the years. Back in 2003, Thorp et al. published a review employing strict inclusion criteria and concluded that induced abortion increased the risk for "mood disorders substantial enough to provoke attempts of self-harm."

F. The Royal College Included My Meta-Analysis in their Review

When the National Collaborating Centre for Mental Health (NCCMH), Royal College of Psychiatrists published their review of the abortion and mental health literature in 2011, they chose to include my meta-analysis despite having ignored many others without a stated rationale. However, they misrepresented the article, and the factual errors made can be readily identified with a quick read of my article. For example, they stated on pages 18 and 19 of their report, "In summary, the APA, Charles and Coleman reviews came to the following conclusions:

> "1. There was a large number of studies that examined the relationship between abortion and mental health, but many were of poor or only fair quality and most had significant methodological problems. 2. There were no rigorous studies that reliably established the prevalence of mental health problems following abortion that resulted directly from the effect of the abortion rather than other confounding factors. 3. From the studies considered, the approximate rates of mental health problems following abortion did not appear to be greatly different from rates of mental health problems in the general US population, although there was some uncertainty regarding this finding, 4. Some factors appeared to be associated with poorer mental health outcomes following abortion, including the stigma associated with abortion the need for secrecy regarding the abortion, personal characteristics, interpersonal concerns, level of social support and previous mental health problems. Previous mental health problems were identified as the most important factor associated with poorer mental health outcomes following abortion. 5. Within the Charles review, the higher the quality of the study, the less likely it was for differences to be found in the relative risk for adverse outcomes following abortion when compared with a group of women with an unwanted pregnancy. The converse appeared to be the case for lower quality studies. 6. When only higher quality studies were included in the analysis, the relative risk of mental ill health was no greater following a first-trimester legal abortion than following delivery at full term of an unplanned pregnancy. 7. A meta-analysis of the studies in the Coleman review suggested that abortion was associated with increased risk of mental health problems across different comparison groups and different diagnostic categories. However, previous mental health problems were not controlled for within the review."

These conclusions are not an accurate reflection of my paper, with the exception of the first part of number 7. It was not appropriate 11 years ago to publish a false analysis of my article, and it is not appropriate today for activists to call for a retraction because they are uncomfortable with the results and the study is having a major impact in terms of informing clinical practice and policy.

II. Section 2: Responses to Criticism Leveled against the Article

A. Meta-Analysis Followed Accepted Protocol

The signatories calling for retraction state on the first page of their letter, "Coleman's analysis failed at every step to meet scientific standards for meta-analytic reviews (including standards in place in 2011, such as AMSTAR, MOOSE, PRISMA, and others [13–17])." This is patently false, and I will explain in detail. Please note, the authors of the retraction letter failed to explain this and other criticisms in any detail, reminiscent of many of the negative letters published a decade ago.

In 1976, Gene Glass introduced the meta-analysis technique and over the last several decades many scholars and organizations have provided guidelines for conducting compelling analyses. Harvard Countway Library offers an extensive list of the various guidelines on their internet site titled, "Systematic Reviews and Meta Analysis A resource for finding data sources, filters, and standards to support systematic searches of the biomedical literature." (Guides and Standards - Systematic Reviews and Meta Analysis - Research Guides at Harvard Library). As indicated in my article, I used a classic book (The Handbook of Research Synthesis and Meta-

Analysis, 2nd edn (eds H Cooper, LV Hedges, JC Valentine): 147–58. Russell Sage Foundation, 2009). The Harvard site illustrates the fact that there are numerous published recommendations for performing a meta-analysis with different emphases and few hard-and-fast rules across the various sources, yet there is a core set of steps researchers should follow. Mikolajewicz and Komarova (2019) in their article titled "Meta-Analytic Methodology for Basic Research: A Practical Guide," noted, "All meta-analytic efforts prescribe to a similar workflow" (p. 2). These authors list 7 steps, all of which I followed in my meta-analysis.

My critics could have attended more closely to the content of my article and letter to the editor following publication and would have undoubtedly realized I followed the accepted protocol. However, they clearly did not, so below I list the steps outlined by Mikolajewicz and Komarova (2019) and provide relevant excerpts or references to sections from my article addressing each step. This gives some basic foundation and I then move to defending the more complex allegations briefly alluded to by the signatories, who throw out a laundry list of supposed flaws in their letter, referencing previous commentary submitted to the journal along with Steinberg's (2012) article in Contraception, which I also discuss in detail below, "Some of the errors in Coleman's analysis include using inadequate search strategies and search documentation, using inadequate methods for the selection of eligible studies, failing to include two independent raters for study selection and evaluation, failing to assess the methodological quality of included studies, failing to examine statistical heterogeneity, inappropriately synthesizing 36 effects from 22 studies without considering their dependence, failing to consider effects of publication bias, illogically combining estimates for distinct outcomes, and inappropriately calculating and interpreting population attributable risk."

Meta-analysis Steps Outlined by Mikolajewicz and Komarova (2019)

1) Formulate research question and define primary and secondary objectives.

Coleman meta-analysis excerpts:

"Through a process of systematically combining the quantitative results from numerous studies addressing the same basic question (e.g., 'is there an association between abortion and mental health?') far more reliable results are produced than from particular studies that are limited in size and scope. Moreover, as a methodology wherein studies are weighted based on objective scientific criteria, meta-analysis offers a logical, more objective alternative to qualitative reviews when the area of study is embedded in political controversy. Therefore, in an effort to provide a long overdue, dispassionate analysis of the literature on abortion and mental health, the primary objective of this review was to conduct meta-analyses of associations between induced abortion and adverse mental health outcomes (depression, anxiety, substance use and suicidal behaviour) with sensitivity to the use of distinct control groups employed in the various studies (no abortion, unintended pregnancy delivered, pregnancy delivered)." (p. 181).

"A secondary objective of this review was to calculate population-attributable risk (PAR) percentages using pooled odds ratios derived from the meta-analysis subdivided by outcome measures. These statistics reflect the incidence of a disorder in the exposed sample (e.g. women

who have undergone abortion) that is directly due to the exposure (the abortion procedure)." (p.181).

2) Identify relevant literature

Coleman meta-analysis excerpt:

"Studies identified using the Medline and PsycINFO databases were included in this review if they met the following criteria: a sample size of 100 or more participants; use of a comparison group (no abortion, pregnancy delivered or unintended pregnancy delivered); one or more mental health outcome variables (depression, anxiety, alcohol use, marijuana use or suicidal behaviour); controls for third variables; use of odds ratios to express effects observed to facilitate calculation of readily interpretable pooled odds ratios and PAR statistics; publication in English in peer-reviewed journals between 1995 and 2009." (p. 181).

These objective selection criteria were employed to weed out poorer studies, yet the panel and the signatories claimed I did not screen for study quality. Further as the chart in the back of my article illustrates, in my meta-analysis, 12 out of 22 of the included studies incorporated controls for prior mental health with 2 additional studies using proxy variables. All the included studies had multiple controls for variables likely to differ between women choosing to abort and women choosing to continue a pregnancy until birth.

The search terms employed were readily derived from the research question, "abortion", "mental health", "depression", "anxiety", "alcohol use", "marijuana use", "suicidal behavior", and various synonyms (e.g., "pregnancy termination" and "psychological outcomes") as well as subcategories of the outcomes (e.g., "PTSD" and "substance use"). It did not seem necessary to list them. The reviewers apparently concurred because they did not ask for this level of detail. As an active researcher on the topic, I regularly watched for and retrieved published articles over the study period, and I assigned student assistants to do so as well. My electronic searches of the two data bases did not yield any studies that I had not previously identified.

There is concern voiced in the letter that two independent raters were not used for study selection and evaluation. However, this is not an absolute requirement when conducting a meta-analysis, particularly when dealing with a limited number of studies and the selection criteria do not have room for subjective interpretation. Many meta-analyses have been conducted by single authors and are published in high quality journals. Several examples are listed below.

DiMatteo, M. R. (2004). Social Support and Patient Adherence to Medical Treatment: A Meta-Analysis. Health Psychology, 23(2), 207–218. https://doi.org/10.1037/0278-6133.23.2.207

Frattaroli, J. (2006). Experimental disclosure and its moderators: A meta-analysis. Psychological Bulletin, 132(6), 823–865. https://doi.org/10.1037/0033-2909.132.6.823

Hillocks, G. (1984). What works in teaching composition: A meta-analysis of experimental treatment studies. *American Journal of Education, 93*(1), 133–170. https://doi.org/10.1086/443789.

Hyde, J. S. (1981). How large are cognitive gender differences? A meta-analysis using !w² and d.. American Psychologist, 36(8), 892–901. https://doi.org/10.1037/0003-066X.36.8.892.

Huang C. (2010). Internet use and psychological well-being: a meta-analysis. Cyberpsychology, behavior and social networking, 13(3), 241–249. https://doi.org/10.1089/cyber.2009.0217.

Khaleque, A. (2013). Perceived parental warmth, and children's psychological adjustment, and personality dispositions: A meta-analysis. Journal of Child and Family Studies, 22(2), 297–306. https://doi.org/10.1007/s10826-012-9579-z

Kim, K. H. (2005). Can Only Intelligent People Be Creative? A Meta-Analysis. *Journal of Secondary Gifted Education*, *16*(2–3), 57–66. https://doi.org/10.4219/jsge-2005-473

Kraus, S. J. (1995). Attitudes and the Prediction of Behavior: A Meta-Analysis of the Empirical Literature. Personality and Social Psychology Bulletin, 21(1), 58–75. https://doi.org/10.1177/0146167295211007

Ross, J. A. (1988). Controlling Variables: A Meta-Analysis of Training Studies. *Review of Educational Research*, *58*(4), 405–437.

3) Extract and consolidate study-level data

Coleman meta-analysis excerpt:

"In addition to the above criteria, rules for extracting and synthesising data derived from the studies selected were developed based on the recommendations outlined by Lipsey[25], to avoid overrepresentation of particular samples and statistical dependences among effects, and generally to ensure the most conservative and unbiased assemblage of results from the individual studies exhibiting considerable variability in reporting.

(a) Relevant studies contributed a maximum of one effect per outcome. When authors reported more than one effect per variable based on separate analyses conducted for distinct demographic groups, or when different diagnoses were reported on within a general class such as anxiety or depression, a composite odds ratio was derived to avoid overweighting in favour of particular studies.

(b) When studies had more than one comparison group, selection rules were employed to provide more weight to comparisons wherein the control group was most closely matched to the abortion group. Specifically, if 'unintended pregnancy delivered' was used the results relative to this group were selected, and when only 'pregnancy delivered' and 'no abortion' comparison groups were used, the effects pertaining to the 'pregnancy delivered' group were selected.

(c) In situations wherein separate results were reported based on one v. two or more abortions, the results specific to one abortion were selected to enable sampling of a more homogeneous population. There are studies suggesting differential effects based on the number of abortions.[26,27]

(d) When particular authors used the same sample and variables in more than one publication, only the most recent publication was selected. When the same data-set was used by different groups, both sets of results were included when distinct samples were defined."

4) Data appraisal and preparation

Coleman meta-analysis excerpts:

"Meta-analyses were conducted using Comprehensive Meta-Analysis version 2.0 for Windows (Biostat, www.meta-analysis.com). Random effects meta-analyses were computed based on the sociodemographic heterogeneity of the study samples.[43] The random effects model takes into account two sources of variance (within-study error and variation in the true effects across studies) with the study weights designed to minimise both sources of variance.[43] A pooled odds ratio was computed using the full 36 effects extracted. In addition, two sets of subgroup pooled odds ratios were calculated based on the type of comparison group used and on specific forms of mental health problems. Adjusted odds ratios with controls for third variables were used in all the random effects meta-analyses. Finally, PAR percentages were computed using the pooled odds ratios (OR) derived from the random effects model subdivided by outcome measures. The PAR percentages were calculated using the formula 1006(Px(OR71))/ (1 + Px(OR71)), where Px is the estimate of population exposure; Px is calculated as c/ (c + d), where c is the number of women in the abortion group who did not experience the mental illness in question and d is the number of women in the 'no abortion' group who were identified as not having the mental illness examined." (p.181).

5) Synthesize study-level data into summary measure

Coleman meta-analysis excerpt:

"After applying the inclusion criteria and rules detailed above, the sample consisted of 22 peer-reviewed studies (15 from the USA and 7 from other countries);[3,20–22,24,26–42] these comprised 36 measures of effect (9 alcohol use/misuse, 5 marijuana, 7 anxiety, 11 depression, 4 suicidal behaviour) and a total of 877 181 participants, of whom 163 831 had experienced an abortion (see online Table DS1). The first random effects meta-analysis, which included 36 adjusted odds ratios from the 22 studies identified, resulted in a pooled odds ratio of 1.81 (95% CI 1.57–2.09, P50.0001). The results of this analysis indicated that women who have had an abortion experienced an 81% higher risk of mental health problems of various forms when compared with women who had not had an abortion (Fig. 1)." (p.182).

6) Exploratory analyses

Coleman meta-analysis excerpts:

"Results of a second random effects meta-analysis, wherein separate effects were produced based on the type of outcome measure, are provided in Fig. 2. All effects were statistically significant, with the largest pooled odds ratio derived for marijuana use (OR = 3.30, 95% CI 1.64– 7.44, P= 0.001), followed by suicide behaviours (OR = 2.55, 95% CI 1.31–4.96, P= 0.006), alcohol use/misuse (OR = 2.10, 95% CI 1.77–2.49, P<.0001), depression (OR = 1.37, 95% CI 1.22– 1.53, P<.0001) and anxiety (OR = 1.34, 95% CI 1.12–1.59, P<.0001). These results indicate that the level of increased risk associated with abortion varies from 34% to 230% depending on the nature of the outcome." (p. 182)

"In the third random effects meta-analysis (Fig. 3) three separate pooled odds ratios were produced based on the type of comparison group employed in the respective studies. When women who had terminated a pregnancy were compared with women who had not done so relative to all mental health problems, the result was statistically significant (OR = 1.59, 95% CI 1.36–1.85, P<.0001). When women who terminated a pregnancy were compared with women who carried to term, using the full set of mental health variables, the result was considerably stronger (OR = 2.38, 95% CI 1.62–3.50, P<.0001). Finally, when 'unintended pregnancy carried to term' operated as the comparison group, the result was likewise statistically significant and closer to the result relative to the 'no abortion' comparison group (OR = 1.55, 95% CI 1.30–1.83, P<.0001). These data indicate that regardless of the type of comparison group used, abortion is associated with an enhanced risk of experiencing mental health problems, with the magnitude of this risk ranging from 55% to 138%." (p. 182).

"The last set of analyses involved calculation of PAR percentages based on pooled odds ratio estimates. The overall PAR percentage was nearly 10%, with the range for particular mental health problems extending from 8.3% for anxiety to 26.5% for marijuana use (Table 1). In addition, a pooled odds ratio for the two large-scale studies in which actual suicide was measured yielded a significant result (OR = 4.11, 95% CI 1.82– 9.31) and a PAR percentage of 34.9% was derived using this pooled odds ratio." (pp. 182-83).

7) Knowledge synthesis (interpretation of findings and recommendations for future work.

Thoroughly addressed in the discussion section (see pages 183-185).

B. Specific Issues Raised in the Retraction Letter and by the Panel Addressed in My Letter to the Editor of November 2011 and Developed More Fully Herein

In my letter to the editor after the meta-analysis was published (full text is provided in Appendix A), I covered each of the primary criticisms from submitted letters, namely heterogeneity, publication bias, selection criteria, and personal bias. These are some of the issues listed by the signatories in their retraction request and by the appraisal panel. For example, as related to the issue of heterogeneity, I first explained my recognition of the heterogeneity reflected across the included studies, based on demographic and cultural differences in sampling, the variability in control groups and outcomes, and differences in third variable controls. Then I explained how I dealt with the differences by employing a random effects model. In the random effects model, individual studies of varying sizes contribute data from distinct populations, all of which are considered in the pooled estimate. Weighting is more balanced in the random effects model, compared to the alternative fixed effects model in order to effectively deal with heterogeneity. I also ran separate meta-analyses based on distinct comparison groups and outcomes to further address the heterogeneity. The methods I employed are widely accepted and commonly used ways of addressing heterogeneity in meta-analysis.

The second area of criticism in letters to the editor dealt with publication bias. When conducting a review as I did, authors are expected to try to identify unpublished studies to include in their analyses to avoid publication bias or an over-representation of statistically

significant findings. As I explained in my letter to the editor, my experience attempting to locate unpublished studies on abortion and mental health was very disheartening, with virtually all requests ignored. Moreover, I noted that if there is any topic wherein many editors, researchers, and professional organizations are highly motivated to publish non-significant effects, it is this one. There are strong political incentives for demonstrating no association between abortion and mental health, rendering publication bias less common on this topic than others. In other words, there are likely fewer studies showing no effects hiding in researchers' file drawers.

In one of the letters to the editor, Goldacre and Lee provided a funnel plot analysis of the meta-analysis data and presented it as evidence of publication bias. Funnel plots indicate the extent to which the study results included in a meta-analysis are scattered symmetrically around a central effect. Reliance on funnel plots for detection of publication bias has been actively challenged (Ioannidis & Trikalinos, 2007; Lau, Ioannidis, Terrin, Schmid, & Olkin, 2007; Terrin, Schmid, & Lau, et al., 2003; Ioannidis, 2005). Most notably, the funnel plot is viewed as largely inappropriate for heterogeneous meta-analyses, wherein studies are not likely from a single underlying population (Ioannidis & Trikalinos, 2007; Lau, Ioannidis, Terrin, Schmid, & Olkin, 2007; Terrin, Schmid, & Lau, et al., 2003; Ioannidis, 2005). 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 (Ioannidis & Trikalinos, 2007; Lau, Ioannidis, Terrin, Schmid, & Olkin, 2007; Terrin, Schmid, & Lau, et al., 2003; Ioannidis, 2005). When funnel plot asymmetry is detected in a heterogeneous meta-analysis, the cause is most likely due to essential differences between the smaller and larger studies. In particular, larger studies typically involve considerably more investment in time and resources and are therefore more likely to be more rigorous. Other differences may exist as well. For example, the majority of the smaller studies included in my meta-analysis employed substance use outcome variables and the available data on abortion and mental health has shown that substance use/abuse outcomes tend to yield the strongest, most robust effects (Coleman, 2005; Mota, Burnett, & Sareen, 2010). In addition, the largest studies incorporated into my meta-analysis tended to have outcomes that included actual diagnoses for disorders, rarer events than high scores on single surveys, more characteristic of the outcome assessments in the smaller studies.

As noted by Lau and colleagues (2006), a funnel plot is a tool of evidence-based medicine that is ironically not itself evidence based: "There is no gold standard against which to compare the results of funnel plot tests. A true standard measure of publication bias would require prospective registries of trials with detailed knowledge of which studies have been published and which are unpublished. It would then be feasible to test whether tests of publication bias capture accurately the presence of unpublished studies and whether one variant performs better than others." The attempt by Goldacre and Lee to discredit the results of my meta-analysis based on an allegation of publication bias using an evaluation standard that is not empirically backed and is not appropriately applied to a heterogeneous meta-analysis was unfounded. Although it is certainly possible that some level of publication bias is present in this meta-analysis, the funnel plot provided clearly does not offer definitive evidence of it.

With regard to issues leveled against my meta-analysis pertaining to selection criteria and the validity of the underlying studies, I explained in my letter to the editor and above that my selection methods were explicitly laid out in my study (they are pasted above). Again, as indicated under the methodology section, 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. Most of the studies meeting these criteria and incorporated into the meta-analysis also had many other strong methodological features (e.g., multiple data points, nationally representative samples).

Unfounded allegations of personal bias were leveled against me in letters to the editor. As I explained in my letter to the editor, half of the articles I included were ones I authored or co-authored. However, at that time I had published 33 peer-reviewed articles and was more widely published on the topic than any other researcher I was aware of. It made perfect sense, therefore, that I was a co-author on a significant proportion of the included studies. All my included studies met the stringent selection criteria and studies of mine that failed to meet the criteria were not included. In the letter, I also addressed the fact that I had never held membership in any pro-life organizations and my interest in the issue was to produce and synthesize high quality scientific data on a highly contentious topic for the ultimate purpose of effectively serving the needs of women.

C. Specific Issues Raised in the Retraction Letter, by the Panel, and/or by one of the Signatories, Dr. Steinberg in her Contraception Article Cited by the Signatories

In this next segment, I address criticism leveled by Steinberg and colleagues in their article titled, "Fatal Flaws in a Recent Meta-Analysis on abortion and mental health." Because there is overlap with issues addressed above, I focus on the non-overlapping issues. As far as I know these authors did not submit their article to the BJP for publication, but instead chose an outlet for their opinions in the journal associated with the Society for Family Planning, which boldly proclaims on the front page of their website, "We believe in just and equitable abortion and contraception informed by science."

Steinberg and colleagues begin their commentary by stating that several recent reviews have concluded "in the aggregate, termination of an unintended pregnancy does not cause mental health problems compared to carrying an unintended pregnancy to term." However, the authors of the reviews concluded that abortion does not pose serious mental health risks above those associated with unintended pregnancy carried to term. Abortion is not a variable that can be manipulated and therefore the focus is on abortion as a potential risk factor for mental health problems as opposed to operating as a direct cause. The cited reviews (Charles, Polis, Sridhara, & Blum RW. 2008; Major et al., 2009; Robinson, Stotland, Russo, Lang & Occhiogrosso, 2009; Royal College of Psychiatrists; 2011) are praised for incorporating high quality studies and for being unbiased despite serious, well-documented individual and collective shortcomings. I address problematic elements of each of these reviews below.

Steinberg's background (also included in the signatories bias chart in the last section of this document) reveals she has received significant funds for her research from politically-driven organizations. For example, she was a recipient of the Charlotte Ellertson Social Science Postdoctoral Fellowship in Abortion and Reproductive Health granted by Ibis Reproductive Health. Although the website of the organization has changed in the years since the Steinberg et al. paper was published, previously there was a statement indicating the first long-range goal is to "Increase access to safe, affordable, high-quality abortion care and establish such care as a human and reproductive right*."* (http://ibisreproductivehealth.org/about/). In Steinberg and colleagues' zealotry, they used the term "error" throughout their disparaging commentary of the meta-analysis. Obviously, the term "error" is widely interpreted to mean a mistake or a departure from what is true or correct as opposed to reflecting a weakness or shortcoming. These authors use the more dramatic term "error" in an effort to sell their questionable critique as more definitive.

The first two "errors" identified by Steinberg and colleagues are "violating guidelines for conducting a meta-analysis" and "not accounting for dependence of effect sizes." In the first section of this rebuttal, I detailed how my methods followed established principles and I would like to emphasize again that as indicated in detail in the meta-analysis article, the strategies employed for extraction and synthesis of effects were in line with the widely respected recommendations in the Handbook of Research Synthesis and Meta-Analysis (chapter in Lipsey, 2009) to minimize over-representation of particular samples and statistical dependencies among effects. What is most important to reiterate here is relevant studies contributed a maximum of one effect per outcome and when authors reported more than one effect per variable based on separate analyses conducted for distinct demographic groups, or when different diagnoses were reported on within a general class such as anxiety or depression, a composite odds ratio was derived to avoid overweighting in favor of particular studies. Further, when specific authors used the same sample and variables in more than one publication, only the most recent publication was selected. When the same data set was used by different groups, both sets of results were included only when distinct samples were defined.

The third "error" Steinberg and colleagues described is presumed misuse of the Population Attributable Risk (PAR) statistic. They claim it is not appropriate to calculate the PAR unless causation is demonstrated. However, the vast majority of epidemiological studies devoted to assessing risk factors for a wide range of disease states rely heavily on use of the PAR for similar types of analyses. The risk factors often involve elective/lifestyle behaviors, demographic factors, and family history variables that cannot be manipulated and preclude determination of causal associations with various conditions and diseases. For example, in a population-based study designed to examine risk factors for breast cancer by Clarke, Purdie, and Glasser (2006), the authors reported that "PARs were 2–11% for EPRT use, 1–20% for alcohol consumption, and 2–15% for physical inactivity." Other examples among the dozens that are relevant include the extensive research on the following topics: smoking and lung cancer, stress and heart disease, and exposure to interpersonal violence and depression. Steinberg and colleagues also criticized the use of the pooled odds ratios derived from the meta-analyses to calculate the PAR; however, since the original studies employed odds ratios as opposed to

relative risk statistics, this was the only logical and appropriate choice. As indicated preciously, Sullins (2016) also utilized this statistic in his large-scale article on abortion and mental health.

The fourth "error" identified by the Steinberg and colleagues is "not adhering to the stated inclusion and exclusion criteria" as they found a single article that they indicated I had missed. I did not "miss" this study; the data in the article in question by Russo and Denious (2001) could not be incorporated into the meta-analysis, because the authors did not include confidence intervals, nor did they provide standard errors necessary to calculate confidence intervals. Interestingly, Steinberg and colleagues have only praise for the Royal College of Psychiatrists' review when dozens of studies meeting their criteria were not included across all four categories examined (e.g., they missed 51 published studies meeting their criteria for studies related to risk-factors associated with post-abortion psychological health.) Numerous studies were likewise not included without explanation in the Charles et al, Robinson et al., and Major et al. reviews. For example, Robinson et al. ignored many methodologically sound studies (20 to be exact) that yielded results counter to the authors' obvious ideologically driven conclusions with no rationale offered. These reviews are discussed in detail below.

In the fifth "error" described by Steinberg and associates they allege that comparison groups were misclassified. Perhaps they missed the fact that several studies had more than one comparison group as indicated by the table of studies provided in the meta-analysis article. As noted in the meta-analysis, when studies had more than one comparison group, selection rules dictated that if 'unintended pregnancy delivered' was used the results relative to this group were selected, and when only 'pregnancy delivered' and 'no abortion' comparison groups were used, the effects pertaining to the 'pregnancy delivered' group were selected. This was done in order to select comparison groups that were most closely matched with the abortion group by minimizing confounding variables.

In their description of a sixth "error" Steinberg et al. claim that it is inappropriate to include studies using a different number and/or forms of control variables. However, this situation is quite typical for a meta-analysis, because it is rare to have multiple studies control for the exact same extraneous variables. In fact, the random-effects model is designed for situations wherein there are both heterogeneous outcomes and controls.

In the seventh cited "error", the authors of the critique claim that I made inappropriate inferences from pregnancy intendedness to birth intendedness. They report that births resulting from unintended pregnancies are approximately 35%; whereas abortions result from unintended pregnancies in 95% of cases. Although I don't argue with these statistics, I cited figures reflecting intendedness as measured in pregnancy as these numbers are bound to be more accurate. Outcome comparisons between women who abort and deliver ideally assess pregnancy intendedness soon after the pregnancy is discovered, as women who abort would be more inclined to report the pregnancy was unintended if they ended up terminating; and those making a conscious decision to keep their infants would seem inclined to positively distort their initial intentions, particularly if they bond well with their infants. The ideal comparisons are between women who abort or deliver a pregnancy identified as unintended soon after the pregnancy is discovered. The comparability of these two groups would be further enhanced if the group that ultimately carries to term consulted with an abortion provider.

As explained above, my meta-analysis employed stringent inclusion criteria. However, Steinberg and colleagues maintained that I inappropriately included studies of varying quality, several of which they termed "poor" in the meta-analysis. They relied primarily on the review of the Royal College of Psychiatrists (RCP) as a basis for their quality argument. As described below, the RCP review cannot be relied upon for assessments of the quality of studies on abortion and mental health due to serious shortcomings. My meta-analysis incorporated more studies into the final analyses with controls for prior psychological problems than the RCP review. Moreover, the conclusions derived from the meta-analysis were based on more studies with controls for prior psychological history than the Charles and the APA reviews as well.

D. Signatories Ignore Positive Letters to the Editor Following Publication

Given the signatories are apparently bent on retraction, they fail to consider the praise for my meta-analysis voiced by several researchers. Two examples are described below, and Appendix B offers more detail.

Subsequent to publication of my meta-analysis, renowned researcher, Dr. David Fergusson from New Zealand, with over 500 journal articles to his name, published a letter in the British Journal of Psychiatry titled "A Further Meta-Analysis" announcing that his own independent meta-analysis was consistent with the results of my study. In his letter he noted, "The implications of this analysis are inescapable: despite the claims made in previous reviews about the absence of association between abortion and mental health, when data are pooled across studies there is consistent evidence suggesting that women having abortions are at modestly increased risks of mental health problems when compared with women coming to term with unplanned/unwanted pregnancies."

In another letter to the editor led by Chilean researcher Dr. Elard Koch and coauthored by two American researchers titled "The elusive problem of causation on the relationship between abortion and mental health problems. Does it really matter to avoid public health recommendations? the following affirming commentary was provided:

> Previous letters by Howard et al., Robinson et al., Lagro-Janssen et al. submitted immediately after the publication (see below) do not seem to even understand what Coleman really did or at least they are underestimating the rigorous methodology applied by the author, quoting substantially weaker studies or basing on the single study by Munk-Olsen et al. [5] - published after the submission of Coleman's study to the British Journal of Psychiatry - to dismiss any evidence suggesting that abortion may have adverse effects on mental health.

E. Inconsistency of the Meta-Analysis Results with "More Rigorous" Studies and with Conclusions of Professional Organizations

On the second page of their letter calling for retraction of my article, the authors state, "Coleman's conclusion about the impact of abortion on mental health conflicts with numerous methodologically more rigorous studies [18–27]. It also contradicts with the current scientific

consensus and conclusions reached by major professional associations, including the Royal College of Psychiatrists, the American Psychological Association, and the American Psychiatric Association" [28–30].

The signatories fail to explain the basis for the first statement, how exactly are the 10 studies more rigorous than my review? Of the 10 studies they cite, one does not relate to abortion and mental health (17), four are Turnaway study articles (19, 20, 21, & 23), one is 32 years-old (18), and two are narrative reviews with blatant shortcomings (22, 24). I have analyzed the Turnaway Study and the two narrative reviews for various audiences. Most recently I addressed the Turnaway Study in a peer-reviewed article as described at the outset of this rebuttal (Coleman, 2022), Regarding the second point in the above paragraph of the retraction request, stating that my meta-analysis results are not aligned with conclusions of professional organizations, I served as an official reviewer for two of these reports and I have critiqued them extensively on grounds of bias and methodological limitations in published articles (including my above cited 2022 Frontiers article), in expert reports, and under oath when testifying orally for numerous civil cases.

In the discussion below, I highlight the shortcomings of the supposedly more rigorous Turnaway Study and of the APA and Royal College Reviews. The signatories chose a highly selective small fraction of the world literature to back their claims and as detailed earlier, there is a large body of highly credible evidence that aligns with my results. Appendix C also provides a list of 30 studies (far from exhaustive) over-looked by the signatories with results indicating abortion increases risk for post-abortion emotional and mental health challenges. Cherry picking a very small percentage of material from the professional literature and failing to explain the critical discrepancies in rigor between the set of studies/reviews aligned with the signatories' political views (discussed in Section 3 below) and my article is deceptive. Ironically, the signatories claimed my review was incomplete, yet as explained earlier in this section, all studies meeting selection criteria were incorporated into the review with the exception of only one that failed to provide the necessary numerical values to include it in the pooled odds ratios.

Critique of Robinson, Stotland, Russo, Lang, and Occhiogrosso Review (2009)

The authors' primary conclusion that "the most well-controlled studies continue to demonstrate that there is no convincing evidence that induced abortion of an unwanted pregnancy is per se a significant risk factor for psychiatric illness" is entirely unfounded for significant scientific reasons. A few of the problems are highlighted below.

1) The most glaring problem with the article is the arbitrary number of papers selected to review and the method for choosing published reports to analyze. The authors mentioned having identified 216 peer-reviewed papers on the topic of abortion and mental health and then noted selection of a sample of studies that "exemplify common errors in research methodology" as well as "major articles that attempt to correct the flaws." Numerous methodologically sound studies that yielded results counter to the authors' politically driven conclusion are entirely ignored with no rationale offered (a partial list is provided below). In a valid scientific review, criteria for selection (e.g., sample size, representativeness, type of comparison group, how well controlled it

is, etc.) are specified at the outset and then the results of each study meeting the criteria are examined to identify general trends. This review lacks a systematic methodology for selection of studies to evaluate.

Coleman, P. K. (2006). Resolution of unwanted pregnancy during adolescence through abortion versus childbirth: Individual and family predictors and psychological consequences. *The Journal of Youth and Adolescence, 35,* 903-911.

Coleman, P. K. et al. (2009), Induced Abortion and Anxiety, Mood, and Substance Abuse Disorders: Isolating the Effects of Abortion in the National Comorbidity Survey. *Journal of Psychiatric Research, 43, 770-776.*

Coleman, P.K., & Nelson, E.S. (1998). The quality of abortion decisions and college students' reports of post-abortion emotional sequelae and abortion attitudes. *Journal of Social and Clinical Psychology, 17,* 425-442.

Coleman, P. K., Reardon, D. C., & Cougle, J. (2005). Substance use among pregnant women in the context of previous reproductive loss and desire for current pregnancy. *British Journal of Health Psychology, 10, 255-268.*

Dingle, K., et al. (2008). Pregnancy loss and psychiatric disorders in young women: An Australian birth cohort study. *The British Journal of Psychiatry, 193,* 455-460.

Fayote, F.O., Adeyemi, A.B., Oladimeji, B.Y. (2004**).** Emotional distress and its correlates. *Journal of Obstetrics and Gynecology, 5,* 504-509.

Fergusson, D.M. et al. (2008). Abortion and mental health disorders: Evidence from a 30-year longitudinal study, *The British Journal of Psychiatry, 193,* 444-451.

Hope, T. L., Wilder, E. I., & Watt, T. T. (2003). The relationships among adolescent pregnancy, pregnancy resolution, and juvenile delinquency, The Sociological Quarterly, 44, 555-576.

Miller, W. B., Pasta, D. J., & Dean, C. L. (1998). Testing a model of the psychological consequences of abortion. In L. J. Beckman and S. M. Harvey (eds). The new civil war: The psychology, culture, and politics of abortion. Washington, DC: American Psychological Association.

Pedersen W. (2008). Abortion and depression: A population-based longitudinal study of young women. *Scandinavian Journal of Public Health, 36* (4):424-8.

Pedersen, W. (2007). Addiction. Childbirth, abortion and subsequent substance use in young women: a population-based longitudinal study, 102 (12), 1971-78.

Pope, L. M. et al. (2001). Post-abortion psychological adjustment: Are minors at increased risk? *Journal of Adolescent Health, 29*, 2-11.

Reardon, D. C., Coleman, P. K., & Cougle, J. (2004) Substance use associated with prior history of abortion and unintended birth: A national cross sectional cohort study.

Am. Journal of Drug and Alcohol Abuse, 26, 369-383.

Reardon D.C., Ney, P.G. (2002) Abortion and subsequent substance abuse. American Journal of Drug and Alcohol Abuse, 26, 61-75.

Rees, D. I. & Sabia, J. J. (2007) The relationship between abortion and depression: New evidence from the Fragile Families and Child Wellbeing Study. *Medical Science Monitor, 13(*10), 430-36.

Sivuha, S. Predictors of Posttraumatic Stress Disorder Following Abortion in a Former Soviet Union Country. *Journal of Prenatal & Perinatal Psych & Health,17,* 41-61 (2002).

Slade, P., Heke, S., Fletcher, J., & Stewart, P. (1998). A comparison of medical and surgical methods of termination of pregnancy: Choice, psychological consequences, and satisfaction with care**.** *British Journal of Obstetrics and Gynecology,105*,1288-95.

Söderberg et al. (1998). Emotional distress following induced abortion. A study of its incidence and determinants among abortees in Malmö, Sweden. European *Journal of Obstetrics and Gynecology and Reproductive Biology 79*, 173-8.

Suliman et al. (2007) Comparison of pain, cortisol levels, and psychological distress in women undergoing surgical termination of pregnancy under local anaesthesia vs. intravenous sedation. BMC Psychiatry, 7 (24), p.1-9.

Suri, R, Altshuler, L., Hendrick, V. et al. (2004). The impact of depression and fluoxetine treatment on obstetrical outcome. *Archives of Women's Mental Health, 7,* 193-200.

2) Another major problem with the review is the use of very dated sources to make sweeping claims. For example, on the first page, the risk of death from abortion in the U.S. is reported as 1:160,000 with reference to a single 1992 citation. A brief sampling of problems with this statistic is offered below.

a. The International Classification of Diseases (ICD-9) defines maternal death as one that occurs during pregnancy or within 42 days of the termination of pregnancy. Pregnancy-associated deaths occurring outside this window are not captured in the data.

b. Coding rule 12 of the ICD-9 requires deaths due to medical and surgical treatments to be reported under the complication of the procedure (e.g., infection) rather than the treatment (e.g., elective abortion).

c. Most women leave abortion clinics within hours of the procedure and go to hospital emergency rooms if there are complications. The data reported by abortion clinics to state health departments and ultimately to the CDC therefore under-represents abortion morbidity and mortality.

d. Abortion reporting is not required by federal law and only 27 states report abortion complications.

e. The abortion-related mortality rates typically fail to factor in abortions beyond the first trimester, 12-13% of all abortions (Barlett et al., 2004 ; Gamble et al., 2008; Jones et al., 2008). Using national U.S. data spanning the years from 1988 to 1997, Bartlett and colleagues reported the relative risk of mortality was 14.7 per 100,000 at 13–15 weeks of gestation, 29.5 at 16-20 weeks, and 76.6 at or after 21 weeks.

f. At least 50% of women who have aborted deny the experience and therefore the medical records of many women who have aborted are not likely to contain an accurate history.

g. Suicide deaths are rarely, if ever, linked back to abortion in state reporting of death rates. Further, suicides are often not recorded on death certificates.

3) Studies pertaining to increased risk for substance abuse are omitted. Substance abuse disorders are widely accepted mental health problems and they have been implicated in anxiety and mood disorders.

4) The review seems to have been put together rather hastily as two of the studies (#23 which is Fergusson and colleagues' 2006 paper published in the Journal of Child Psychology and Psychiatry and Allied Disciplines and #39 which is a paper published in the Canadian Medical Association Journal by Reardon et al. in 2003) had findings contradicting their conclusion, yet these studies are cited among others as supporting their claims.

5) A final point to consider, the Impact Factor (IF) for the journal that published this review is considerably lower than that of most of the journals where the omitted studies were published.

Critique of Charles, Polis, Sridhara, & Blum Review (2008)

At first glance the findings of the "systematic" review by Charles et al. published in Contraception represents a sophisticated attempt to evaluate the literature. However, a careful reading indicates there are numerous shortcomings and the findings lack credibility for the four key reasons detailed below.

1) The review neglects to cover numerous studies that have linked abortion to substance abuse problems, one of the major mental health concerns of women who have aborted and for women in general. No explanation is provided for this blatant omission.

2) The ranking system employed ignores two of the most central methodological considerations when conducting reviews of prospective research designs: 1) the percent consenting to participate at baseline (information was not even provided by the authors of one study, Gilchrist et al., that this team ranked as "Very Good"); and 2) retention of subjects over time. Obviously when women are more harmed by an abortion they are less inclined to want to continue to participate. Further, women who are suffering from an abortion are likely to have less stable lives and are therefore more likely to be unavailable to be assessed. If the sample suffers from high attrition rates, then the results cannot be applied to the general population.

3) Five quality indicators were employed to derive the ratings of each study from "Excellent" to "Very Poor". These indicators were each deemed met or not met by the raters of the studies. However, no explanation is given for the extent to which evidence of the indicator had to be present in order to be marked as "met". This leaves the evaluation method open to considerable bias. There is no way that several of the studies listed as "Very Good" would have met 4 out of 5 of the quality indicators necessary for the rating if rated by an objective evaluator who was not invested in deriving a conclusion that is consonant with pro-choice ideology. Similarly, studies rated as "Fair" such as the one by Fergusson and colleagues published in 2006 would have been rated higher by an objective evaluator.

4) The review "missed" numerous high quality studies that meet their inclusion criteria. The result is an extremely biased selection. A sampling of the ignored studies is provided below.

Coleman, P. K. (2006). Resolution of Unwanted Pregnancy During Adolescence Through Abortion versus Childbirth: Individual and Family Predictors and Consequences. Journal of Youth and Adolescence, 35, 903-911.

Henshaw, R., Naji, S., Russell, I., & Templeton, A. (1994). Psychological responses following medical abortion (using mifepristone and gemeprost) and surgical vacuum aspiration: A patient-centered, partially randomized prospective study. Acta Obstetrica et Gynecologica Scandinavica, 73, 812-818.

Lauzon, P., Roger-Achim, D., Achim, A., & Boyer, R. (2000). Emotional distress among couples. involved in first trimester abortions. Canadian Family Physician, 46, 2033-2040.

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Critique of the Turnaway Study

In this section of my rebuttal, I address the significant methodological weaknesses and bias characterizing the Turnaway Study, referenced by the signatories of the retraction letter as more rigorous than my meta-analysis. Several of the signatories are Turnaway Study investigators and or authors of the published reports.

According to Martin (2016), Warren Buffett donated at least $88 million from 2001 to 2014 to UCSF, a medical research institution with a strong reproductive health infrastructure. Critically, that money went to support researchers with a well-defined, viewpoint-based agenda. Martin (2016) explained, "The research initiative dates back at least to the early 2000s and became more urgent after the high court held in 2007 that in cases of 'medical and scientific uncertainty,' legislatures could have 'wide discretion' to pass laws restricting abortion. Since then, a primary objective of abortion rights supporters has been to establish a high level of medical certainty—both about the safety of the procedure and about what happens when a woman's reproductive options are drastically curtailed or eliminated." (p. 2). Martin notes Advancing New Standards in Reproductive Health (ANSIRH) was established in 2002 as part of UCSF's Bixby Center for Global Reproductive Health and "foundation-backed researchers had already begun to churn out studies aimed at debunking some of the most common justifications for new abortion restrictions…that the psychological damage caused by grief and regret after abortions often persists for years and ruins women's lives." (p. 2). In line with this agenda, the Turnaway Study has generated dozens of journal articles using the same data set.

As stated in the Turnaway Study Operating Procedures Manual (2016) (p. 3), "This study explores the experiences and outcomes of women (including minor women) who obtain abortions, as well as women who are denied abortions because they present for care beyond the clinic's gestational limit. In order to determine how our study population compares to the universe of all women seeking abortion (the vast majority of whom have a first trimester abortion), we also include a third group of women who seek services in the first trimester of pregnancy." More specifically, the authors recruited participants with three distinct profiles: 1) women whose gestational age was one day to three weeks beyond the clinic's gestational limit for performing second-trimester abortions and were turned away without receiving a desired second trimester abortion; 2) women whose gestational age was one day to two weeks under the clinic's gestational cut-off for performing second-trimester abortions and received a second trimester abortion; and finally, 3) women who received a first trimester abortion.

The Turnaway Study investigators do not clearly articulate the sampling plan, the size of the population, or precisely how sites situated in different cities were chosen . Only very generalized information on these issues is described in the Operating Procedures Manual (2016) and the cities are not identified. In one of the study's published articles, Biggs et al. (2014) noted, "From 2008 to 2010, we recruited women seeking abortion care at 30 facilities in 21 states throughout the USA. Facilities were identified using the National Abortion Federation membership directory and by referral. Sites were selected based on their gestational age limits to perform an abortion procedure, where each facility had the latest gestational limit of any facility

within 150 miles. Gestational age limits ranged from 10 weeks to the end of the second trimester. Facilities performed over 2,000 abortions a year on average" (Biggs et al., 2014, p. 2506).

From this description, there is no way of determining if all the selected facilities engaged in recruitment during the first year and then continued efforts for the next two years of the study. In another publication, Dobkin et al. (2014, p. e116) note, "We began recruiting participants from one abortion facility and gradually expanded to the 30 total facilities over the next 3 years." Although the number of facilities that engaged in recruitment across the full 3 years is not stated, the excerpt does seem to suggest at least some of the facilities were retained following the initial year. The Turnaway Study Operating Procedures Manual (pp. 6-7), provided some more information, "Early in the project, Sandy Stonesifer, the Program Manager at the time, or PI Diana Foster conducted on-site orientation visits to twenty-three of the clinics. They met and trained the point people for the remaining clinics at the annual NAF meeting during the spring of 2008. Over time, additional clinical recruitment sites were added. In April 2010, we had 29 clinics participating in the study. In early 2010, Project Directors Rana Barar, Heather Gould, and other staff members visited all participating clinics, either to train them in participant recruitment (if they were new sites) or to motivate them to continue recruitment, and to share lessons learned from other successful recruitment sites." This seems to suggest 23 sites were retained in the first year and then by the third year there were 29 facilities actively recruiting. With such ambiguity, one does not know how many facilities recruited for 3 years, 2 years, 1 year, or less. Assuming continuous recruitment across the 3 years for the 23 clinics identified in the first year, the potential participants would include 138,000 for this segment based on 23 sites x 2000 average annual abortions x 3 years. Further, if the remaining 6 sites were added in the beginning of year 2 and recruited for the full remaining 2 years, the maximum potential participant pool from this segment would be 24,000 reflecting 6 sites x 2000 average annual abortions x 2 years. When the two segments are combined, the upper limit of the population would be 162,000. Unfortunately, without more information there is no way of knowing the minimum number of women who comprised the population.

According to Dobkin and colleagues (2014), of those screened in (n=7,486), only 3,045 were approached to participate across the three groups. This is approximately 41%. No explanation is provided for why so many were not approached. This is potentially problematic, because those not approached could have been systematically different from those approached relative to background characteristics, situational factors and/or how they handled the abortion experience.

Further, based on data offered by Dobkin et al. (2014), the percentages of women approached varied dramatically based on the study groups. The Turnaway Group was 83.2%, the Near Limits Group was 58%, and the 1st trimester Group was 22%. Such disparate rates are not addressed and are potentially problematic in terms of representativeness, particularly since the authors do not explain why some women were approached and others were not.

Agreement-to-participate rates derived from the percentage of women who were approached were 41% in the Turnaway Group, 42.2% in the Near Limits Group, and 33.8% in the 1st Trimester Group. The total number of women who agreed to participate across groups

was 1199/3045 = 39.37%, resulting in a sample that was unlikely to accurately represent those approached.

The final sample of 516 participants amounts to a miniscule .32% of the total abortions performed at the 29 facilities over 3 years if the high end 162,000 figure for the population is used. At 50% (81,000), the percentage only jumps to .64%, and at 10% (16,200), the percentage is 3.18%. The Turnaway Study researchers attempted to make generalized claims about women seeking abortion when the study itself likely did not even consider over 95% of women receiving abortions at the facilities included in the study. Given the extremely small percentage of women from the population represented in the sample, generalizations are precluded.

Any generalizations from this sample to women undergoing abortions under similar situations are, therefore, precluded. There are many reasons women may have chosen not to participate. For example, they may have been too upset, or they may have been concerned about privacy issues, since a longitudinal design requires repeated contact with participants. There are other ways the non-participants may have been systematically different from the participants, creating a biased sample. For example, they may have been busier with children, working more, experienced more instability or unrelated trauma in their lives, or they may have simply been less interested in giving up personal time.

Biggs and colleagues (2016) explained that 210 women in the Turnaway Group (21.9% of women who were screened-in and 26.3% of approached), 413 in the Near Limits Group (18.05% of women who were screened-in and 31.1% of approached), and 254 in the 1st Trimester Group (6.1% of women who were screened-in and 25.9% of those approached) completed the baseline measures. Overall, 877/3045 or 28.8% of eligible women approached competed the baseline measures.

The total percentage of women who finished the 5-year study from among those approached was 516/3045, a mere 16.9%, or if the women deemed ineligible after a phone call following consent to participate are eliminated from the denominator (65 women), the figure is 17.3% (516/2980). The study results are, therefore, based on a very small fraction of women eligible for inclusion.

The bottom line is the results are in no way generalizable to women beyond the study and have virtually no broader utility. Experts in various academic fields have identified low response rates leading to non-response bias as a potentially fatal flaw (Amico, 2009; Draugalis et al., 2008; Fowler, 1995). Non-response bias refers to the estimation of population characteristics based on a sample of survey data wherein certain types of respondents are under-represented. More specifically, this bias exists when respondents to a survey are different from those who do not respond relative to demographic, situational, behavioral, personality, psychological, and/or social factors. As a result, the sample is not representative of the target population and the conclusions drawn are likely invalid. Non-response bias may occur based on two conditions:

a. When a significant percentage of eligible potential participants do not consent to participate at the outset of a study.

b. When a significant percentage of participants drop out from a study (termed "attrition").

Draugalis et al. (2008) cites several experts, noting response rates need to be between 50% and 75% to be acceptable. He concludes by stating that to be credible, survey research must meet acceptable levels of scientific rigor with regard to response rate transparency and the representativeness or generalizability of the study's results. Fowler (1995) commented that one occasionally sees reports of mail surveys with a 5% to 20% response rate, noting that in such instances, the final sample has little relationship to the population of interest. Those responding are self-selected, and unlikely to yield any credible information about the characteristics of the broader group from which they originated. Finally, Amico (2009) notes that loss of potential participants or of those who initially consent to participate and subsequently leave the study of greater than 30% or 40% is indicative of a ''fatal'' design flaw, in effect negating results. Even if we consider only the percentage of participants who consented and completed the Turnaway Study, 17.3%, we must conclude that the results are of no scientific value and are not generalizable to all women who have obtained abortions.

When non-response bias is operative, differences between those who are willing to be in the study and those who are not are likely to be highly pronounced, resulting in samples that do not adequately mirror the populations from which they were drawn. For example, women whose voices are not included are likely those who had the most negative post-abortion psychological complications, because they are less likely to want to discuss a difficult experience and revisit the trauma (Söderberg, et al., 1998).

Another serious problem with the Turnaway Study is that the group of women who secured abortions near gestational limits included women for whom the legal cut off ranged from 10 to 27 weeks, ignoring the fact that women's reasons for choosing abortion and their emotional responses to the procedure differ greatly at varying points of pregnancy. Women aborting at such widely disparate gestational ages should, therefore, not be combined, particularly when the data would have permitted useful segregation by gestational ages.

Further, many of the primary outcome measures are simplistic, with two variables (anxiety and depression) containing only six items and two additional variables assessed with a single item (self-esteem and life satisfaction). This is inexcusable given the many psychometrically sound multiple-item surveys available in the professional literature. Consensus among researchers is that multiple-item measures typically offer far more reliable and valid assessments of multi-faceted psychological constructs, because they are able to capture all components of the constructs in a nuanced and thorough manner (Fisher et al., 2016). Complex human cognitions and emotions should never be measured in superficial ways, particularly when more sophisticated, thoroughly developed and extensive measures are available for the variables of interest. For the extremely common variables in the Turnaway Study, dozens of well-designed measures could have been accessed. For example, back in 2012, authors Therrien and Hunsley (2012) identified 91 different scales to measure anxiety in the published literature. Dozens of the anxiety scales they described are far superior in terms of basic coverage of the construct and in

terms of scientifically derived indicators of reliability and validity compared to the limited measures used in the Turnaway Study.

Critique of the American Psychological Association Task Force Report

The APA now has over a 50-year history of taking a political stance on abortion, advocating for its recognition as a civil right since 1969; therefore, basic precautions should have been followed to assure the work of the Task Force was done in an objective, scientifically defensible manner. The Task Force had no call for nominations and the final make-up was comprised of individuals who have been public advocates of the pro-choice view. For example, Nancy Russo and Linda Beckman responded to APA member Robert Gallagher who questioned the appropriateness of the APA taking "a very clearly political stance by explicitly associating itself with the Pro-Choice Forum" by stating: "Gallagher naïvely assumes findings with implications for women's lives can be 'apolitical'. Science always reflects the values of scientists--the difference here is that we state our values up front and do not pretend scientific methods make findings value-free… A pro-choice position means that we believe abortion is the woman's choice, that women should be given accurate information and informed consent in making their reproductive choices, and that they be supported in their decisions." (http://www.apa.org/monitor/apr03/letters.aspx). Researchers whose work has indicated abortion increases risk for harmful outcomes to women could have easily balanced the team; yet there was no effort to appoint diverse researchers to the panel. Below is list of additional problems with the conduct of the APA review.

1. There was a claim that three literature reviews (Coleman et al., 2005; Coleman, 2006; Thorp, Hartmann & Shadigian, 2003) were incorporated into the APA report; however, the conclusions of these reviews are entirely ignored, and no explanation is provided. For example, Thorp et al. (2003) concluded that induced abortion increased the risk for "mood disorders substantial enough to provoke attempts of self-harm"; this is not alluded to whatsoever in the APA Task Force Report.

2. The APA Task Force did not perform a meta-analysis; therefore, the strength of abortion-mental health associations across studies was not quantified in the 2008 report. From the authors' perspective, there were too few studies to quantify effects yet a sweeping definitive statement indicating an absence of ill-effects was considered justified.

3. According to the APA report, the Task Force "evaluated all empirical studies published in English in peer-reviewed journals post-1989 that compared the mental health of women who had an induced abortion to the mental health of comparison groups of women (N=50) or that examined factors that predict mental health among women who have had an elective abortion in the United States (N=23)." Note the second type of study is restricted to the U.S., resulting in elimination of at least 40 studies, many of which revealed statistically significant associations between abortion experience and mental health problems.

4. The APA Task Force did not select studies based on methodological criteria. Sample size, characteristics, representativeness, type of design, and employment of control

techniques should have been the minimum foundation for selecting studies to include in the review.

5. In the APA Task Force Report, there are shifting standards of evaluation based on congruence with a pro-choice agenda. There are numerous examples of studies with results suggesting no negative association between abortion and mental health being reviewed less extensively and stringently than studies indicating adverse relationships between abortion and mental health. Positive features of the studies suggesting abortion is a benign experience for most women are highlighted, while the positive features of the studies revealing negative outcomes are downplayed or ignored. All the studies showing adverse outcomes associated with abortion were published in peer-reviewed outlets, many in very prestigious journals with low acceptance rates. A few examples of this bias are detailed below.

a. The Medi-Cal studies (Coleman, Reardon, Rue, & Cougle, 2002; Reardon et al., 2003) are sharply criticized for insufficient controls; however, with the use of a large socio-demographically homogeneous sample many differences are likely distributed equally across the groups. Moreover, the strengths of the study include use of actual claims data (diagnostic codes assigned by trained professionals), which eliminate the problems of simplistic measurement, concealment, recruitment, and retention, which all are serious shortcomings of many post-abortion studies. The authors of the Medi-Cal studies also removed all cases with previous psychological claims and analyzed data using an extended time-frame, with repeated measurements enabling more confidence in the causal question.

b. Fergusson and colleagues' 2006 study had numerous positive methodological features, yet it was denounced by the APA as flawed. Among the positive features of this study are the following:

1) longitudinal design, tracking women over 25 years;

2) comprehensive mental health assessments employing standardized diagnostic criteria of DSM III-R disorders;

3) considerably lower estimated abortion concealment rates than found in previously published studies;

4) the sample represented between 80% and 83% of the original cohort of 630 females;

5) the study used extensive controls.

6. Sample attrition as a methodological weakness is downplayed in the APA report. The studies with the highest attrition rates, conducted by Majors and colleagues provided little evidence of negative effects; these studies are embraced as high-quality investigations despite attrition rates as high as 60%. Common sense suggests that those who are most

adversely affected are the least likely to want to think about the experience and respond to a questionnaire. Research indicates that women who decline to participate or neglect to provide follow-up data are more likely to be negatively impacted by an abortion than women who continue participating (Soderberg, Anderson, Janzon, & Sjoberg, 1998).

Perhaps most egregious is the fact that the final conclusion in the APA Task Force Report did not follow from the literature reviewed, and it inappropriately rested on one study by Gilchrist et al. (1995) published in the U.K. that has a number of ignored methodological flaws. Reliance on one study to draw a definitive conclusion stands in direct contrast to accepted scientific protocol as described by Wilkinson and the Task Force on Statistical Inference affiliated with the APA Board of Scientific Affairs. Wilkinson and colleagues (1999) specifically stated in the American Psychologist, "Do not interpret a single study's results as having importance independent of the effects reported elsewhere in the relevant literature. The thinking presented in a single study may turn the movement of the literature, but the results in a single study are important primarily as one contribution to a mosaic of study effects" (p. 602). Several flaws of the Gilchrist study were overlooked by the APA Task Force; the most serious are detailed below.

1. The authors report retaining only 34.4% of the termination group and only 43.4% of the group that did not request a termination at the end of the study. The attrition rate is highly problematic as are the differential rates of attrition across the comparison groups. Logically, those traumatized are less likely to continue in a study.

2. No standardized measures for mental health diagnoses were employed and evaluation of the psychological state of patients was reported by general practitioners, not psychiatrists. The GPs were volunteers and no attempt was made to control for selection bias.

3. The response rate was not provided, meaning it is impossible to know if the sample was representative of women in the U.K or not.

Within weeks of the release of the APA Task Force Report, Dr. David Fergusson, a New Zealand researcher with an extensive publication record (over 500 peer-reviewed articles), and I wrote a petition letter to Dr. Alan Kazdin, President of the APA (see Appendix D). The interest in writing a petition letter originated with Dr. Fergusson, who served as an official reviewer for the Task Force Report. As noted earlier, I too served as a reviewer of the Task Force Report. We were both concerned that the Task Force ignored our feedback and that of other reviewers. Together Dr. Fergusson and I drafted the letter, solicited support from other well-published researchers, and compiled an extensive list of articles authored by the signatories. The letter was submitted to Dr. Kazdin on September 1, 2008, and the key points we raised are summarized below. At the end of our letter, we requested that the APA revisit this issue and seriously consider a retraction or revision; however, no action occurred.

1. Wholesale dismissal of most of the evidence in the field was unacceptable.

2. In no other area of public health research has a highly contested issue been resolved on the basis of a single out-of-date research study in the way that occurred in the APA Task Force report.

3. The APA Task Force report was not an impartial assessment of the mental health risks of abortion and its conclusions were unduly colored by the views of its authors.

For decades the APA has aligned itself with major organizations with pro-choice social agendas, including the American Civil Liberties Union Reproductive Freedom Project, National Abortion Federation, National Abortion Rights Action League, Guttmacher Institute, and Planned Parenthood among others, frequently submitting amicus briefs and providing congressional testimony. Martel (2009) discussed the APA's position on abortion, among other issues, noting that the organization'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 organization in the world, politically motivated efforts to publish null findings to support and legitimize their position is logical.

National Collaborating Centre for Mental Health (NCCMH) Royal College of Psychiatrists Review

A few years after the APA report was published, the NCCMH Royal College of Psychiatrists, published a literature review. The NCCMH incorporated four types of studies:

1. Reviews of the literature.

2. Empirical studies addressing the prevalence of post-abortion mental health problems.

3. Empirical studies identifying risk factors for post-abortion mental health problems.

4. Empirical studies comparing mental health outcomes between women who choose abortion and delivery.

In each category, there were studies that were ignored and large numbers of studies that were entirely dismissed for vague and/or inappropriate reasons. With regard to the first type of study, only three reports were considered (APA Task Force Report, 2008; Charles et al., 2008; Coleman, 2011). The authors of the NCCMH report "missed" 19 reviews of the literature published between 1990 and 2011. Moreover, no criteria were identified for selection of particular reviews. In relation to the third type of study (addressing risk factors for post-abortion psychological problems), only 27 studies were included in the NCCMH report. At least 20 relevant and unmentioned articles published in highly respected peer reviewed journals were ignored and were not listed in Appendix 7 of the NCCMH report, which contained all included and excluded studies.

The NCCMH authors stated that "Because the review aimed to assess mental health problems and substance use and not transient reactions to a stressful event, negative reactions

and assessments of mental state confined to less than 90 days following the abortion were excluded from the review." This is highly problematic for the following reasons:

1. Elimination of studies that only measured women's mental health up to 90 days, does not effectively remove cases of transient reactions. Just because the authors of these dozens of studies did not follow the women long-term, it does not mean that the women were not still suffering quite significantly beyond the early assessment.

2. When investigating the mental health implications of an event, it is logical to measure outcomes soon after the event has occurred as opposed to waiting months or years to gather data. As more time elapses between the stressor and the outcome(s), healing may naturally occur, there may be events that moderate the effects, and more confounding variables may be introduced.

3. Finally, focusing only on mental health events that occur later in time effectively misses the serious and more acute episodes that are effectively treated soon after exposure. Many of the studies removed from the analyses due to the abbreviated length of follow-up, had incorporated controls for prior psychological history and other study strengths. As a result, the samples of studies included in each section of the NCCMH review were not representative of the best available evidence and many of the eliminated effects coincidentally revealed adverse post- abortion consequences. In the category wherein the authors sought to derive prevalence estimates, only 34 studies were retained, including a majority without controls for previous mental health. As mentioned earlier, in contrast, in my meta- analytic review, 12 out of the 22 included studies had controls for psychological history.

The NCCMH review was pitched as methodologically superior to all previously conducted reviews, largely because of the criteria employed to critique individual studies and to rate the overall quality of evidence. However, the quality scales employed to rate each individual study are not well-validated and require a significant level of subjective interpretation, opening the results to considerable bias.

The main problems with the quality scales employed by the NCCMH to rate the individual studies are as follows: 1) the categories used are missing key methodological features, including initial consent to participate rates and retention of participants across the study period; 2) the relative importance assigned to the included criteria was arbitrary, as opposed to being based on consensus in the scientific community; 3) the specific requirements for assigning a "+" or "-" within the various categories were not provided; 4) the authors fail to explain (as their predecessors, Charles et al. 2008 did) how combinations of pluses and minuses in the distinct categories add up to an overall rating ranging from "Very Poor" to "Very Good." Incredulously, the highly flawed Gilchrist et al. (1995) study (described above) received an overall rating of "Good", with a mark of "+" (thorough) for confounder control, a "+" for representativeness, and a "+" for validated tools.

Similarly, when it came to evaluating the quality of evidence associated with specific outcomes, such as anxiety, depression, suicide ideation, drug or alcohol abuse, psychiatric treatment, etc. with regard to the comparative studies, the GRADE (Grading of Recommendations, Assessment, Development, and Evaluation), was inappropriately employed by the NCCMH. The GRADE system was not designed for use with individual studies, but for analysis of systematic reviews (Burford, Rehfuess, & Schünemann, et al., 2012). The anchors on this scale are vague and oftentimes only one reason is identified by the NCCMH as the basis for a "Very Low" rating. For example, in the category of "Any Psychiatric Treatment," which actually only included the Munk-Olsen et al. study, the basis for the "Very Low" (very uncertain about the estimate) rating was for not having controlled for pregnancy intention. When the study was again evaluated later in the report, it was rated as "Good" in the comparison category. There are loose, poorly conceived rationales with inconsistencies like this throughout the report.

In the second paragraph of the first page of the letter requesting a retraction, the signatories state, "Coleman's analysis involved numerous egregious methodological flaws, resulting in misleading conclusions which meet the standard for retraction, as per COPE guidelines" They cite to a 2009 version. Nowhere in the 2019 version of the guidelines is there language regarding "egregious methodological flaws resulting in misleading conclusions". Reasons editors should consider retraction according to the current guidelines https://publicationethics.org/files/retraction-guidelines-cope.pdf) are reproduced below and there is absolutely no evidence that any of the conditions for retraction are relevant to my article. Perhaps they were appealing to number 1, "clear evidence that the findings are unreliable." However, as explained in detail above, this is firmly not the case and critics would be hard pressed to prove otherwise if they moved beyond broad claims and attempted to offer a detailed analysis of precisely how my methodology fell short of convention and if they relied on more scholarly sources for substantiation.

1. They have clear evidence that the findings are unreliable, either as a result of major error (eg, miscalculation or experimental error), or as a result of fabrication (eg, of data) or falsification (eg, image manipulation).

2. It constitutes plagiarism.

3. The findings have previously been published elsewhere without proper attribution to previous sources or disclosure to the editor, permission to republish, or justification (ie, cases of redundant publication).

4. It contains material or data without authorisation for use.

5. Copyright has been infringed or there is some other serious legal issue (eg, libel, privacy)

6. It reports unethical research.

7. It has been published solely on the basis of a compromised or manipulated peer review process.

8. The author(s) failed to disclose a major competing interest (a.k.a. conflict of interest) that, in the view of the editor, would have unduly affected interpretations of the work or recommendations by editors and peer reviewers.

III. Section 3: Reproductive Rights, Pro-Choice Bias of Signatories and Attack on Researchers Whose Results Conflict with the Dominant Narrative

A. Conflicts of Interest and Biases of the Signatories

As indicated at the outset of this rebuttal, the signatories of the retraction request letter have affiliations and views aligned with reproductive rights and pro-choice initiatives and organizations. Information suggestive of conflicts of interest and bias for 16 out of 17 of the authors is summarized below.

Chelsea B. Polis

- Former principal research scientist at the Guttmacher Institute https://chelseapolis.com/index.html
- Worked with Ibis Reproductive Health and Bixby Center for Global Reproductive Health
- Tweet acknowledging that she wants Dr. Coleman's work retracted so lawyers can't use it: https://twitter.com/cbpolis/status/1560981340081856519
- Describes ordering "advance provision" abortion pills: https://twitter.com/cbpolis/status/1554294040878845952
- Coauthored papers with researchers from the Abortion Access Project: https://www.sciencedirect.com/science/article/pii/S0010782403002245?via%3Dihub

M. Antonia Biggs (Turnaway)

- One of the primary Turnaway Study researchers: https://bixbycenter.ucsf.edu/m-antonia-biggs-phd
- Associate researcher in UCSF's Department of Obstetrics, Gynecology, & Reproductive Sciences/Bixby Center for Global Reproductive Health (notably pro-abortion research institution) and Senior Researcher at ANSIRH, specific research h interest in mental health outcomes of women having abortions: https://bixbycenter.ucsf.edu/m-antonia-biggs-phd
- Reckless and dangerous op-ed authored by Biggs titled "With abortion clinic restrictions tightening, women want more access at home.": https://www.salon.com/2018/11/28/with-abortion-clinic-restrictions-tightening-women-want-more-access-at-home/ (one of about 100 pro-abortion publications/co-publications)
- Frequently writes and publishes pro-abortion articles (scroll down to see comprehensive list): https://bixbycenter.ucsf.edu/m-antonia-biggs-phd

Nada Logan Stotland

- Former president of APA
- Former board member of Physicians for Reproductive Health https://prh.org/board-of-directors/
- Lots of writing on abortion
- "Advocate for women's reproductive rights" per Ms. Magazine https://msmagazine.com/author/nadastotland/
- Author of *Abortion: Facts and Feelings—A Handbook for Women and the People Who Care About Them*

- Reaction to Dobbs decision: https://psychnews.psychiatryonline.org/doi/10.1176/appi.pn.2022.08.8.52

Ushma P. Upadhyay (Turnaway)

- One of the primary Turnaway Study researchers and Director of Research of the University of California Global Health Institute's Center of Expertise in Women's Health, Gender, and Empowerment: https://www.ansirh.org/about/staff/ushma-upadhyay-phd-mph#:~:text=Ushma%20Upadhyay%2C%20PhD%2C%20MPH%20is,Health%2C%20Gender%2C%20and%20Empowerment.
- Associate Professor at USCF Department of Obstetrics, Gynecology & Reproductive Services and ANSIRH/Bixby: https://bixbycenter.ucsf.edu/ushma-upadhyay-phd-mph
- Proponent of online chemical abortion clinics, op-ed: https://www.usatoday.com/story/opinion/2021/04/12/medication-abortion-rights-protected-online-clinics-column/7106777002/
- Current research focus is on understanding the impact of state-level abortion restrictions on women's lives (specifically chemical abortion and ultrasound requirements): https://bixbycenter.ucsf.edu/ushma-upadhyay-phd-mph

Trine Munk-Olsen

- Paper showing no connection between abortion and poor mental health, which the letter uses to criticize Dr. Coleman's paper, was funded by pro-abortion Susan Thompson Buffet Foundation: https://www.nejm.org/doi/10.1056/NEJMoa0905882?url_ver=Z39.88-2003&rfr_id=ori:rid:crossref.org&rfr_dat=cr_pub%20%200www.ncbi.nlm.nih.gov

Robert Wm. Blum

- Chair of the Department of Population, Family and Reproductive Health at John Hopkins from 2004-2018 https://publichealth.jhu.edu/faculty/1441/robert-w-blum
- Past chair of Guttmacher Institute Board of Directors
- One of the commissioners of the Guttmacher-Lancet Commission on Sexual and Reproductive Health and Rights (2018) https://www.guttmacher.org/guttmacher-lancet-commission/about/commissioners

Gail Erlick Robinson

- Criticized informed consent laws: https://my3.my.umbc.edu/groups/archive/discussions/11167
- Pro-abortion letters to the editor: https://nationalpost.com/opinion/letters/todays-letters-readers-dare-to-have-the-debate-our-mps-avoided
- https://nationalpost.com/opinion/paul-russell-with-contentious-issues-the-right-word-is-crucial
- Member of the pro-abortion IAWMH: https://iawmh.org/wp-content/uploads/2019/01/IAWMHNewsletter_-_Sept_2015.pdf

Louise M. Howard

- Member of the pro-abortion IAWMH: https://iawmh.org/wp-content/uploads/2019/01/IAWMHNewsletter_-_Sept_2015.pdf

Julia H. Littell

- Littell does not publish on abortion: https://mashable.com/article/abortion-mental-health-science
- Retweeted a post saying the Democrats must be elected because reproductive choice is on the line: Original post: https://twitter.com/AllIn_PA/status/1548363070489014272?cxt=HHwWgMCl_cmA8vwqAAAA Post on Littell's feed: https://twitter.com/jlittell?lang=en - Retweets multiple posts on reproductive rights from All In (PA Democratic Party) and other Democrats
- Retweeted Robert Reich's post on "forced birth" in the U.S.: original post: https://twitter.com/rbreich/status/1540392087320858624?lang=en Found on Littell's feed: https://twitter.com/jlittell?lang=en
- Retweeted Congresswoman Scanlon's post on passing the Women's Protection Act: Original post: https://mobile.twitter.com/RepMGS/status/1521301871524204548 Found on Littell's feed: https://twitter.com/jlittell?lang=en

Diana Greene Foster (Turnaway study lead researcher)

- Director of Research for the Turnaway study: https://www.ansirh.org/about/staff/diana-greene-foster-phd
- Frequently publishes articles promoting abortion (New York Times, LA Times, Salon)
- Researcher with Bixby/ANSIRH
- Received award from Population Association of America

Leah N. Torres

- Medical director of West Alabama Women's Center (an abortion center)
- Her Alabama license was temporarily suspended due to fraud and unprofessional conduct: https://www.cbs42.com/alabama-news/doctor-at-tuscaloosa-abortion-clinic-facing-over-100k-in-legal-fees-1-year-after-license-mistakenly-revoked/

Julia R. Steinberg (Turnaway)

- According to her CV, she is currently collaborating with a Planned Parenthood lawyer on a rebuttal report on abortion and mental health, pushing back on an SD law: https://www.popcenter.umd.edu/mprc-associates/jrsteinb/cv/
- Studies the "intersection of mental and reproductive health" https://sph.umd.edu/people/julia-r-steinberg
- Researcher with pro-abortion Maryland Population Research Center https://www.popcenter.umd.edu/mprc-associates/jrsteinb
- Researcher with Bixby https://intranet.bixbycenter.ucsf.edu/fs/bios/steinberg-juliar.html
- Grants from Society for Family Planning, Ibis, etc.
- Consultant for PP who fights against pro-life laws

Corinne Rocca (Turnaway)

- Turnaway Study author: https://www.ansirh.org/research/ongoing/turnaway-study
- Collaborated on research with Planned Parenthood: https://www.plannedparenthood.org/about-us/newsroom/press-releases/public-funding-for-contraception-improves-access-to-long-acting-reversible-contraception-larc

- More research with Planned Parenthood: https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5997454/
- Affiliated with ANSIRH/Bixby Center for Global Reproductive Health

Brenda Major

- Interview in which Major describes her 1st research on abortion & mental health in which she worked closely with a Buffalo abortion clinic through the connection of a grad student who worked there: https://journey2psychology.com/2019/03/14/the-empowered-dr-brenda-major/

Donna E. Stewart

- The drafting of the preamble and recommendations of the International Women's Mental Health Consensus Statement was chaired by Donna E. Stewart and International Association of Women's Mental Health (IAWMH) https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1472251/
    - Recommendation includes support of "reproductive choices"
    - International Association of Women's Mental Health is a nonprofit organization who's purposes include: "Control by women of their personal fertility" https://iawmh.org/about-iawmh/, Indications of pro-abortion stance: https://iawmh.org/wp-content/uploads/2019/06/IAWMH-program-2019_web-1.pdf and https://iawmh.org/iawmh-and-dublin-declaration-at-2017-congress-aided-reproductive-rights-law-passage/
- President of IAWMH, 2004-2008 https://iawmh.org/wp-content/uploads/2019/01/IAWMHNewsletter_-_Sept_2015.pdf

Vignetta Charles

- CEO of ETR, which was founded out of Planned Parenthood (find good source for this) https://www.stopp.org/article.php?id=15466
- PP Action Fund lists her as a "Defender": https://www.plannedparenthoodaction.org/blog/announcing-99-dream-keepers-defenders

  As one of the authors of the William and Flora Hewlett Foundation's 2004 Population Program Strategic Plan, she recommended funneling funds to international abortion groups like Marie Stopes International and International Planned Parenthood Federation. She also recommended that the Foundation promote "safe abortion-related tools like manual vacuum aspiration equipment or medical abortion". https://hewlett.org/wp-content/uploads/2016/08/PopulationProgramStrategicPlan.pdf

B. Other Credible Researchers' Work Has Also Been Misrepresented When Results Counter the Dominant Narrative

Other researchers whose studies have yielded results that do not align well with a pro-choice agenda, have had their work mischaracterized as poor when in fact there are numerous methodological strengths. For example, the reaction of the APA Task Force authors to the work of renown researcher, David Fergusson from New Zealand, who described himself as a pro-choice, rational atheist demonstrates they will dismiss the work of anyone who conducts research revealing an association between abortion and increased rates of mental health problems.

Dr. Fergusson's obituary describes his commitment to scientific integrity, disciplined adherence to the methods of science, and the enormous impact he had.

https://www.stuff.co.nz/the-press/news/107556388/life-story-groundbreaking-health-researcher-david-fergusson-was-dedicated-to-helping-the-underprivileged

Relevant excerpts:

"Long-time colleague and close friend Dr Joe Boden said Fergusson was one of New Zealand's best scientists. His work in social science was groundbreaking and his impact on his colleagues and the field was immeasurable. In over 25 years of doing research I have known many other scientists, but none with more integrity and devotion to the scientific process than David."

"As one of the most prominent social scientists in New Zealand, Fergusson received several accolades over the course of his career. He was an elected Fellow of the Royal Society of New Zealand. He was honoured by being named an honorary Fellow of the Royal Australian and New Zealand College of Physicians and the New Zealand Psychological Society."

"He also received the distinguished Research Medal, the University of Otago's highest research award, in 2010."

This link is to a story describing how Fergusson spoke out about the bias permeating efforts to publish on the topic of abortion and mental health. https://writer-sarahterzo.medium.com/scientist-journals-wouldnt-publish-my-research-on-abortion-5d6546c54270

"Research on abortion led to unexpected findings. David Fergusson is a pro-choice researcher whose 2006 research on abortion found a higher rate of depression and other mental health problems among post-abortive women. He admits he was surprised by the results of the research. The study found that 42% of women who had undergone abortions within the previous four years suffered from depression. This was more than double the rate of women who hadn't had abortions and higher than the rate of those who had given birth. The study also found higher rates of substance abuse, anxiety, and suicidal behavior in women who had undergone abortions."

"Medical journals refused to publish his study. However, it seems that abortion advocates are attempting to hide the truth. According to Fergusson, many of the medical journals he approached refused to publish the study. He explained, "We went to four journals, which is very unusual for us — we normally get accepted the first time." This makes it clear that the scientific and medical community is biased against research that shows the risks of abortion."

"One pro-choice organization, the Abortion Supervisory Committee, tried to pressure Fergusson not to publish the study. They said that publishing the results in an "unclarified state" would cause it to become "a political football." Fergusson said it would be "scientifically irresponsible" not to publish the study and compared it to a study that found an adverse reaction to a medication. He said at the time, "It verges on scandalous that a surgical procedure that is performed on over one in 10 women [in New Zealand] has been so poorly researched and evaluated, given the debates about the psychological consequences of abortion."

As described earlier, Dr. Fergusson wrote an affirming, positive letter to the editor of the British Journal of Psychiatry in response to the publication of my meta-analysis. He was also an external reviewer for my promotion to Associate Professor with tenure and to Full Professor at BGSU. Both letters were very supportive of my scholarship.

C. General Impact of Scholarship

My career spans three decades with publications in highly reputable academic journals. Most of my publications have been in journals with Impact Factors exceeding the average for psychology (1.39) and medicine (2.90) (Althouse et al., 2009). Eleven of the journals I have published in are in the top 20% of journals across all disciplines according to Journal Citation Reports (impact factor at or above 3.0). On Google Scholar, there are 7080 peer-reviewed citations to my scholarship. Google Scholar also reports the h-index, or Hirsch index, which measures the impact of a scientist based on the total number of publications and citations to publications. Hirsch (2005) estimated that after 20 years a "successful scientist" would have a score of 20; my current h-index is 33. There is evidence that scores tend to be lower in the social and behavioral sciences, even in top ranking programs. For example, Barner et al. (2015) reported the average h-index of faculty affiliated with 25 highly ranked psychology programs was 15.67; my score is over twice as high. Finally, I am a member of Research Gate, a global community of scholars and my current score is 31.03, exceeding those of 90% of researchers from the many disciplines represented. According to Research Gate, my journal articles have been read 69,284 times, and my "Research Interest" score is 2,301, higher than 97% of affiliated scholars.

D. Success as an Expert Witness Motivates Attacks Against my Work

My research has been deemed reliable and utilized in courts across the U.S. as a basis for informed consent, waiting period, and mandatory counseling laws. The many victories in recent years, exemplified by the two cases below have motivated the signatories of the letter and others to heighten their attempts to discredit me.

1) In Planned Parenthood Minn., N.D., S.D. v. Rounds, 686 F.3d 889 (8th Cir. 2012), the U.S. Court of Appeals for the Eighth Circuit concluded that the district court erred in granting a permanent injunction enjoining a provision of the South Dakota statute requiring disclosure to abortion-seeking patients of an increased risk of suicide ideation and suicide. The suicide advisory was ruled non-misleading and relevant to women's decisions regarding abortion, as not placing an undue burden on abortion rights, and as not a violation of physicians' free speech rights. I served as an expert for the Intervenors in Planned Parenthood v. Rounds for 6 years, beginning in 2006 and I submitted numerous expert reports, a few of which were referenced in the 8Th Circuit decision. The 8th Circuit judges also noted the quality of studies in the record, "With regard to whether the required disclosure is truthful, the State submitted into the record numerous studies published in peer-reviewed medical journals that demonstrate a statistically significant correlation between abortion and suicide. The studies were published in respected, peer-reviewed journals such as the Obstetrical and Gynecological Survey, the British Medical Journal, the Journal of Child Psychology and Psychiatry, the Southern Medical Journal, and the

European Journal of Public Health, and there is no indication that the peer-review process was compromised for the studies at issue." Rounds, 530 F.3d at 735.

2) The First District Court of Appeals for the State of Florida (No. 1D18-623), cited favorably to my declaration as an expert witness, assisting the State in defending a 24-hour waiting period bill. On page 8 of the opinion, the following statement is made "The physicians' declarations are supported by other mental health-related declarations filed by the State. Dr. Coleman stated, for example, that "waiting periods in other states are associated with improved mental health among females as evidenced by a significant drop in suicide rates." She cited studies that women who have abortions in the absence of a deliberative period are more likely to suffer depression, anxiety, post-traumatic stress, substance abuse, and suicidal behavior." More recently Circuit Judge Angela Dempsey ruled in favor of the 24-hour waiting period on summary judgment. I was the only academic expert witness retained by the state whereas the Plaintiffs retained eight.

E. Increasingly Bold and Unethical Efforts to Discredit

For 30 years, despite actively publishing on the highly politicized topic of the psychology of abortion in strong journals, I have withstood virtually relentless personal and professional attacks via adherence to professional ethics and scientific standards and the ability to defend my work. Earlier in my career there was not the expansive literature documenting the psychological risks of the procedure available today, presumably because many academics shied away from such a politically hot topic and editors of journals were reluctant to publish on the topic. The activists/researchers had more power to dismiss my work prior to the proliferation of studies by numerous research groups. Now in 2022, they are apparently desperate with so much evidence that runs counter to their mission of "proving" abortion is a safe and benign procedure for all women. The signatories have largely been proponents of unrestricted access to abortion and with the fall of Roe v. Wade and state battles raging, a few of which I am currently assisting with, the signatories are obviously motivated to remove me from the legal challenges and discredit my work by whatever means possible. The desperate, poorly composed retraction request is a breach of professional behavior and an abuse of science.

IV. Conclusion

As an actively recruited expert witness, my time is valuable. I do not take kindly having to spend days developing this document for an article that has been in the public domain for over a decade and likely scrutinized far more than most articles published in the BJP and elsewhere. However, I am committed to doing whatever it takes to preserve my reputation, prevent future malicious attacks from key players, and continue to be a voice for truth in science to enable women to make healthy reproductive choices. I fully recognize that this is a highly controversial area of academic study due to the many applied implications; however, I seriously hope the time invested in this case will result in policy and actions that prevent future occurrences to advance to this point or beyond. Efforts like the current one arriving at this level go far beyond me and my career, they are a serious threat to scientific progress and public trust. When journals acquiesce to individuals and groups promulgating political or ideological agendas, they are

directly responsible for suppression of knowledge, sacrificing everything they supposedly stand for to enable agendas to advance. Accountability and consequences need to finally occur for individuals who so unabashedly fabricate, distort, and seek to manipulate science.

Respectfully Submitted,

Priscilla K. Coleman, Ph.D.

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