

The ten deadly sins of Jones, Paton and Kheriaty on 'suicide contagion'

Neil Francis

19th November 2017



Rocking the vote for law reform

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Executive summary

In 2015, Dr David Jones & Prof. David Paton published an article in the *Southern Medical Journal* titled "How does legalization of physician-assisted suicide affect rates of suicide?" This study examines the article, as well as an enthusiastic editorial of it by Dr Aaron Kheriaty in the same journal issue, both of which portray "suicide contagion" from Oregon and Washington's death with dignity acts (DWDA).

However, while contagion from general suicides is a well-established phenomenon, there are multiple sound reasons to reject contagion theory in relation to assisted deaths, including:

- Most healthcare professionals readily acknowledge key differences in the characteristics of assisted deaths: for example, a fully informed, tested and rational decision with shared decision-making.
- Those using Oregon and Washington's DWDA's are, by qualifying for it, already actively dying. Thus, they are choosing between two ways of dying rather than between living and dying.
- Most of those using the DWDA discuss it with their families (expected, peaceful death), whereas most general suicides occur in isolation and without discussion (unexpected, often violent death).
- Multiple studies show that while families of general suicide experience complicated bereavement, families of assisted dying cope at least as well as, and in some cases *better than*, the general population or those who considered but did not pursue assisted death.

Even if "suicide contagion from assisted dying" theory were sound, direct evidence from official government sources shows that the number of potential suicides in Oregon in 2014 would have been fewer than 2 in 855 cases: undetectable by general modelling methods.

Jones & Paton's article title conveys an air of skilled and scientific neutrality. However, close examination of the article, and Kheriaty's editorialisation of it, reveals least ten serious flaws or 'scientific sins.'

The authors demonstrated little understanding of the complex issues surrounding suicide, willingness to unjustifiably equate assisted dying with general suicide, contentment with failing to search for, consider or include contrary evidence *including from sources they cite to argue their case*, unreasonable trust in a model that couldn't hope to legitimately resolve their premises, satisfaction with executing their model amateurishly, a disposition to overstate confidence of causation in the absence of meaningful statistical

correlations, and an inclination for emphasising results in accordance with their theories while de-emphasising or ignoring others.

Any of these flaws was serious enough to invalidate Jones & Paton's article and Kheriaty's conclusions of it, yet there is not one deadly flaw: there are at least *ten*.

Their claim of a supposed 6.3% suicide contagion rate from assisted dying in Oregon and Washington is a conceptual and mathematical farce.

The *Southern Medical Journal* is a peer-reviewed journal. However, it is difficult to reconcile the rigorous standards and sound reputation that peer review is intended to maintain, with the numerous, egregious flaws in this study and its dissemination.

Rather than inform the ongoing conversation about lawful assisted dying, the Jones & Paton and Kheriaty articles misinform and inflame it.

Given the numerous egregious flaws, both articles ought to be retracted.

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Introduction

In 2015, Dr David Jones and Prof. David Paton published an article titled “How does legalization of physician-assisted suicide affect rates of suicide?” in the *Southern Medical Journal*.¹ The article purported to establish suicide contagion from Oregon and Washington Death With Dignity Act (DWDA) deaths to “total suicides.” It also purported to establish no decrease in general suicide rates, which Jones & Paton argued should occur by substitution of assisted death for some general suicides. (Notice how these two ‘expected’ results — an anticipated rise *and* an anticipated fall in suicide rates — are in at odds principle, a fundamental point that Jones & Paton fail to expressly make up front.)

At the time of completing this study of Jones & Paton’s article (late 2017), it had been cited only three times, against assisted dying, in the professional literature. One was a glowing editorial of the study by psychiatrist Aaron Kheriaty, appearing in the same journal edition.² The second was a vague citation in an opinion piece by psychiatrist Laura Dunn,³ suggesting that Jones & Paton’s article said something it didn’t,* and the third was a mention in the American College of Physicians position paper on assisted dying.⁴

Nevertheless, the ‘results’ of Jones & Paton’s study have been quoted widely in public discourse, by both medical and lay opponents of assisted dying.

In this report, Jones & Paton’s study and Kheriaty’s editorial are critically appraised, finding that, rather like Dr José Pereira’s “slippery slope” arguments,⁵ the authors in reality produce “smoke and mirrors.”⁶

All studies have limitations

In fairness, it’s important to note that *all* scientific research has limitations. This study does not seek to criticize reasonable limitations, particularly where data was not available and where due note of a limitation had been made. Rather, while Jones & Paton’s article, and Kheriaty’s editorial of it, conveyed the impression of scientific rigour, this examination highlights multiple instances where their research deviated substantially from acceptable scientific standards.

Terms used

For simplicity and clarity in this report, deliberately self-hastened deaths *without* lawful assistance are referred to as ‘general suicide’ or merely ‘suicide,’ while deliberately self-hastened deaths *with* lawful assistance are referred to as ‘assisted deaths.’

The expression “physician-assisted suicide” (PAS) is also used where it appears as a direct quote of Jones, Paton and Kheriaty’s articles.

* Dunn cited Jones & Paton in relation to her claim that “there can be no limit [to suicide], save individual will.” Jones & Paton’s article didn’t expressly make this point.

Jones & Paton’s ‘study’ has been quoted widely in lay circles as an argument against lawful assisted dying.

Ten deadly 'sins'

A recent scholarly report has criticised Jones, Paton and Kheriaty's articles,⁷ identifying a number of concerns. The numerous offences against scientific rigour committed by Jones, Paton and Kheriaty — perhaps from ignorance but nevertheless egregious — are examined in greater detail in this report, under ten deadly 'sins.'

- Sin 1 Premise bias:** simplistically equating DWDA deaths with suicides and "suicide contagion" despite relevant circumstances and empirical evidence to the contrary;
- Sin 2 Omission bias:** failing to mention pertinent facts which contradicted their "suicide contagion" premise, in sources they cite to argue the premise;
- Sin 3 Premise bias:** arguing, using invalid metrics, that there ought to be a substantial proportion of 'substitution' — people switching from general suicide to assisted death, while ignoring quantitative contrary evidence contained in a source they cited;
- Sin 4 Premise bias:** arguing invalidly and without considering appropriate metrics, that there ought to be substantial "general suicide death delay" from assisted dying;
- Sin 5 Theory-versus-method bias:** selecting a general econometric modelling approach that couldn't hope to answer their conflicting research questions, when direct evidence — unsupportive of their "suicide contagion" premise — was readily available; and introducing their primary "general social contagion" theory only *after* reporting and trying to explain their unhelpful results;
- Sin 6 Selection bias:** selecting 'test' assisted dying jurisdictions whose suicide rates may suggest "contagion," while omitting other jurisdictions whose suicide rates contradict it;
- Sin 7 Selection bias:** selecting 'control' jurisdictions in a way likely to increase differences between test and control, and failing to compare like with like;
- Sin 8 Confounding factor control bias:** failing to adequately research, consider or control for most significant suicide rate confounding factors, while 'controlling' for several non-significant factors, in their econometric model;
- Sin 9 Interpretation bias:** failing to join the research dots from their primary result to their primary theory, and overconfidently highlighting results that seemed to support their theory while downplaying interpretations that contradicted it; and
- Sin 10 Spreading misinformation:** invalidly claiming causation and disseminating that conclusion to a wide and non-expert audience.

Sin 1: Premise bias — “suicide contagion”

Jones & Paton underwrite their entire study with the premise that assisted deaths in Oregon and Washington are equivalent — or at least similar in substantial ways including “contagion” — to general suicides. To support this claim, they speculate (only in the later discussion and not up front) that “it may be that legalising PAS also provides positive role models who help normalise suicide more generally,” citing as evidence only the equally speculative opinion of a single social worker, published in 1994.⁸

General community perceptions of suicide are that acting to end one’s life is irrational, ill-informed, impulsive, and the result of mental illness with or without concomitant substance abuse. In such cases negative perceptions, and efforts to reduce suicide, are justified.

However, lawful assisted dying cases in USA Oregon and Washington states — the test jurisdictions of Jones and Paton’s article — are fundamentally different: they are fully informed, considered and tested wishes to hasten death in the face of intolerable and unrelievable suffering at end of life.

Yet Jones & Paton fail to cite any of the many published scholarly sources at variance with their assumed suicide equivalence.

Rational versus irrational decision making

Medical presumption of mental impairment in suicide is diminishing,⁹ with two types of suicide — rational and irrational — now widely accepted.¹⁰ In the face of refractory and intolerable symptoms at the end of life, most Australian doctors believe a request for hastened death can be rational, including 96% of NSW doctors,¹¹ 93% of Victorian doctors¹² and 89% of South Australian doctors.¹³ Amongst UK psychiatrists, 86% agree,¹⁴ as do 81% of USA psychotherapists,¹⁵ and 85% of USA mental health counsellors.¹⁶

It has been suggested by mental health experts that there are three components of a rational assisted death: (1) the presence of an unremittingly hopeless condition, (2) a decision made as a free choice, and (3) the presence of an informed decision making process.¹⁷

Stances opposed to the *rational* pursuit of a peaceful death over-pathologise suffering⁹ and exhibit medical paternalism.¹⁸

Anticipated versus unexpected death

In general suicide, most don’t communicate their intention to end their own lives, and so the death is often an unexpected shock.¹⁹

In contrast, those using the DWDA are already actively dying, so death is anticipated. Further, 93% in Oregon²⁰ and 91% in Washington²¹ inform their families of their decision to use the DWDA, so the *form* of death is mostly anticipated as well.

Jones & Paton fail to cite any of the many published scholarly sources at variance with their assumed suicide equivalence.

Less rather than more likely complicated grief

The bereaved in general suicide can experience complicated grief, including guilt, confusion, rejection, shame, anger and stigma, contributing to depression and potential further suicide.²²

However, in Oregon and Washington, since the individual must have a terminal illness in order to qualify, he or she was already on a direct and foreseeable trajectory to death. This means that deaths under the DWDA are a choice between two different ways of dying — unlike general suicide where the choice is between living and dying — significantly diminishing the likelihood of complicated grief, at least in relation to *differences* between ‘natural’ and assisted death.

Indeed, an Oregon study found that the bereaved from assisted deaths appreciate the opportunity to say goodbye, to know that the choice was the deceased’s wish, that the deceased avoided prolonged suffering, that the choice was legal, and they were able to plan and prepare for the death.²³

Another Oregon study found that the mental health outcomes of bereaved from assisted deaths were no different from the bereaved from natural deaths.²⁴ Bereaved from assisted deaths were more likely to believe that the dying person’s wishes had been honoured and were less likely to have regrets about the death.

A Swiss study found the rate of complicated grief after assisted death was comparable to the general Swiss population,^{25†} while a Dutch study found bereavement coping in cancer was *better* after assisted death than after non-assisted death.²⁶

Unlikely ‘copycat’ method

Jones & Paton also overlook the well-established principle that many of those whose suicide is influenced by another’s suicide often do so by copying the suicide *method* (“copycat” suicides or the “Werther effect”²⁷), for example railway suicides in Austria²⁸ and Germany;^{29,30} falls from high places in Switzerland;³¹ carbon monoxide poisoning in the UK,³² Hong Kong,³³ and Korea;³⁴ hydrogen sulphide poisoning in Japan;³⁵ drug overdose in the UK,³⁶ and hanging/suffocation in UK prisons.³⁷ But how do individuals *make* themselves terminally ill in order to qualify for a DWDA death? (For a fuller discussion of suicide risk and protective factors, see Appendix A.)

Unsurprisingly, given all these profound differences, it is important that “suicide prevention and therapy should not be projected onto assisted suicide without critical reflection.”³⁸

*How do individuals
‘make’ themselves
terminally ill so as to
qualify for a DWDA
death?*

† Slightly elevated levels of PTSD were found amongst the bereaved (compared to the general population), but it was not established whether this would have been different from the trauma of experiencing continued suffering and deterioration.

DWDA deaths not suicide

For these many reasons, and showing respect for those who rationally choose to hasten death in the face of intolerable and unrelievable suffering, the Oregon and Washington DWDAs expressly state that deaths under their provisions are *not* suicides.

These views of Jones, Paton and Kheriaty are consistent with the Catholic Church's:³⁹ that lawful assisted deaths are equivalent in all meaningful ways to general suicide and therefore ought to be condemned and opposed (see *Appendix C: Who are Jones, Paton and Kheriaty*).

Jones & Paton don't discuss the many significant differences between assisted dying and general suicide in their article, but carry on regardless to prosecute the case for 'suicide contagion' from assisted deaths. That's a substantial premise bias.

Sin 2: Omission bias — content of cited sources

Jones & Paton omit important information which runs *counter* to their suicide contagion premise, from sources they cite to support their case. Here are three examples.

No mention of DWDA; actual suicide contagion rate given

Firstly, Jones & Paton cited some data from the Oregon government team that forensically analyses every single suicide according to strict national guidelines (their citation 18, Shen & Millet 2012).⁴⁰ Crucially, they failed to mention that the DWDA is not mentioned once by this independent and expert team as a potential source of “suicide contagion.”

They also failed to mention that in table 5 on page 14 of the cited report, rounded to the nearest percent, is the *actual* rate of suicide contagion in Oregon at that time: “Suicide of family member or friend within past five years.” They had no reason to surmise, guess or wonder: the quantitative figure is in the report, just 1%. This alone should have signalled that their broad econometric model wasn’t up to the task of detecting contagion from the small number of assisted dying cases, even accepting for a moment the premise that such deaths cause general suicide contagion.

Greater risk factor omitted

Secondly, while Jones & Paton readily quote statistics from Shen and Millet regarding the rate of physical illness contribution to suicide in Oregon (25%), they fail to mention a more significant finding — intimate partner problem contribution: 29% for males and 27% for females. Since intimate partner problems contribute to suicide more than does physical illness (at least in Oregon), it is critical to control for this confounding factor in order to generate meaningful model results. However, as discussed later, Jones & Paton do *not* include a control measure for intimate partner problems, even though one is readily available: divorce rates.

But there’s worse.

Contrary data omitted

Thirdly, Jones & Paton also cite a Switzerland Government Statistical Office report (their citation 17)⁴¹ to populate their storyline.

Switzerland has the world’s oldest assisted suicide law, in effect since 1942, and which has *none* of the safeguards and processes of the Oregon/Washington DWDAs. If there were any important jurisdiction to test for “suicide substitution from assisted dying law,” Switzerland would be the *first* port of call. So, it’s curious that Jones & Paton plump for Oregon (legalised in 1997) and Washington (2008).

On page 2 of 4 (i.e. not hard to find) in the Swiss document Jones & Paton cited, is a figure, Chart G7, reproduced below (Figure 1).

Jones & Paton cherry-pick a single suicide factor from a report, while ignoring a more significant factor, and the actual, quantitative answer to their “suicide contagion rate” question: just 1%.

Jones & Paton fail to mention critical evidence from a Swiss source they cite.

A mere cursory glance at this chart is all that is necessary to discern that there may be a clear and direct substitution of suicides with assisted deaths. (It remains unproven on only this evidence, but it is consistent with substitution theory.)

Suicide and assisted suicide 1995–2009

G 7

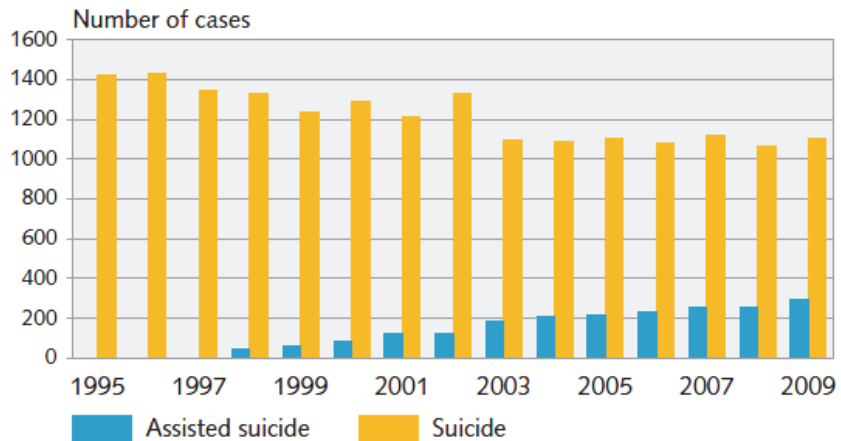


Figure 1: Chart G7, from the Swiss government’s statistical office, in a document cited by Jones & Paton, but which they fail to mention
Source: Federal Statistical Office [Switzerland]⁴¹

Jones & Paton utterly fail to mention this critical data in a document from which they’ve cited *other* content to argue their case about suicide substitution, but say they didn’t find.

Jones & Paton include a variable in their model which one of their cited sources expressly said was a poor predictor of suicide rates.

Jones & Paton also cite a study to naively argue that legalisation of marijuana for medical and recreational purposes is a protective factor against suicide,⁴² while neglecting to mention that one third of USA emergency department admissions for drug abuse are in relation to marijuana.⁴³

More strikingly, Jones & Paton ignored the clear statement in this cited study that 0.08% blood alcohol drink-driving laws were a *poor* predictor of suicide rates. Jones & Paton went ahead anyhow and included 0.08% alcohol law data in their suicide model.

That Jones & Paton omit highly significant information, unresponsive of their assumptions or conclusions and from sources they cite to argue their case, is a most egregious bias indeed. Their overlooking the quantitative answer to their primary research question contained in a source they cite is astonishing and at the very least smacks of profound carelessness. Do the multiple instances of omission possibly indicate that they were intent on reaching particular conclusions regardless of contrary existing evidence?

Sin 3: Premise bias — substitution sensitivity

Jones & Paton cite supporters of assisted dying to say that legalising it ought to decrease the number of people suiciding violently in relation to chronic and terminal illnesses, by substituting assisted death. However, Jones & Paton use invalid means to grossly overestimate the likely proportion of substitutions.

Gross ‘potential substitution’ in Switzerland and Oregon

They report statistics from Switzerland⁴¹ to say that “in the 20% of [general] suicides that involved physical illness, ‘the range of physical illnesses reported with suicide is similar to that reported with assisted suicide.’”

They also report that around 25% of individuals suiciding in Oregon “were found to have physical health problems.”

Jones & Paton use these proportions to claim that “the legalization of PAS could provide an alternative to [general] suicide for some people with **chronic or terminal illnesses**,” (emphasis added) and would therefore reduce the general suicide rate by substituting assisted death in some cases. By reporting figures of 20–25% and referring to chronic as well as terminal illnesses, they imply a large and significant potential substitution of assisted dying for general suicide.

However, the Swiss comparison is *invalid* because there are *no* qualifying criteria for access to assisted death in Switzerland, while in Oregon and Washington a terminal illness with death expected within 6 months is mandatory. Therefore, any underlying similarities between general suicides and assisted deaths in Switzerland is uninformative to the Oregon/Washington context.

The Oregon comparison is also *invalid* because “people with *chronic illnesses*” do *not*, as Jones & Paton wrongly imply, qualify for assisted dying in either Oregon or Washington. Nor is access granted merely on the basis of “terminal illness”: only in terminal illness *where death is expected within six months*.

Indeed, Jones & Paton don’t mention the six-month prognosis anywhere in their article. That’s an egregious omission of a critical fact in the development of their substitution hypothesis.

Tiny real rate of substitution

What do published studies tell us about *realistic* substitution rates?

Demos research in the UK⁴⁴ found 10.6% suicides involved a significant chronic physical illness. The state coroner in Victoria, Australia, found 8.5% of suicides in respect of serious physical illness,⁴⁵ and one in ten suicides in the USA occurs in the absence of a mental disorder.⁴⁶

These figures largely cover chronic physical illnesses, which don’t qualify under Oregon or Washington DWDAs. So what proportion of general

Jones & Paton used invalid comparisons to try and suggest a vastly higher substitution rate (of assisted death for general suicide) than could be the case in Oregon & Washington.

suicides occur directly in relation to terminal illness? The Demos study found that just 2.1% of suicides involved a terminal illness.

For the sake of simplicity, let's say that *half* of all those with terminal illnesses would be expected to die within six months (DWDA qualification criterion). Assuming, as the *chronic* physical illness suicide data suggests, that the UK and USA are also similar in regard to *terminal* illness suicide rates, that's just 1% of all suicides related to physical illnesses that would qualify under Oregon and Washington's DWDA's.

Also for the sake of argument, let's assume that *all* those individuals in Oregon and Washington found a doctor willing to consider and honour their request for an assisted death, though this may not be the case. That's still a 1% maximum *possible* drop in the general suicide rate, while the natural statistical variation in suicides in Oregon is on average 5% from year to year (US Centers for Disease Control data).

Near impossibility of detecting substitution

Adding to the natural year-to-year variation are trends in the major contributors to general suicide including mental health, substance abuse, financial distress and intimate partner problems including divorce, and others. Under these conditions it would be virtually impossible for an econometric modelling study of the type Jones & Paton chose to pursue, to statistically detect a drop of just 1% in the general suicide rate.

More evidence of undetectable substitution

Further readily-available evidence confirms the proportions difference between chronic and terminal illness as potential contributors to general suicide rates, using two proxy measures.

Firstly, in severe chronic illness, poor access to appropriate medical care could be a contributing factor for general suicide.

Jones & Paton failed to find and use readily-available evidence to test their theories, and whether their modelling approach could have any hope of answering their research questions.

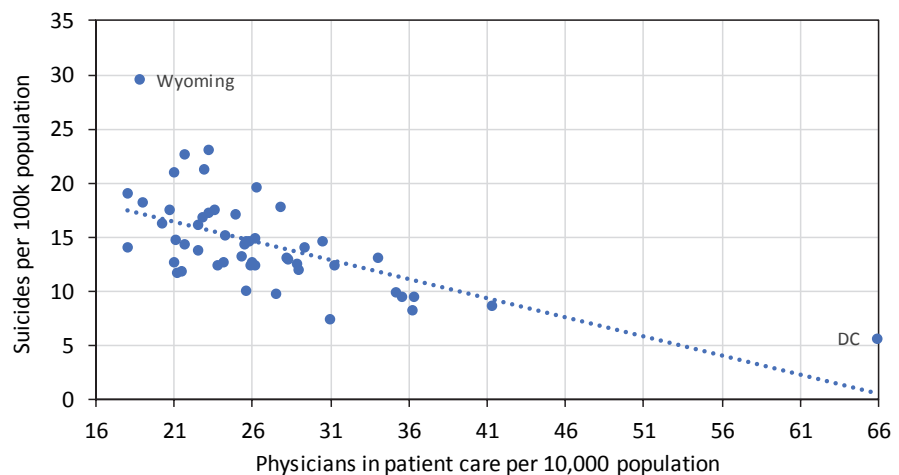


Figure 2: USA state physician density and suicide rates 2012

Sources: U.S. Department of Health and Human Services, CDC Wonder

Empirical data (Figure 2) confirms a significant state correlation between low physician-to-population density and general suicides (adjusted $r^2 = 0.36$, $p < 0.001$). Thus, there is a statistically-detectable (and strong) relationship between unaddressed chronic physical illness and suicide rates.[‡]

Secondly, a useful proxy measure for terminal illness is the rate of cancer deaths by state, since most of those who use the DWDA have advanced cancer (Figure 3).

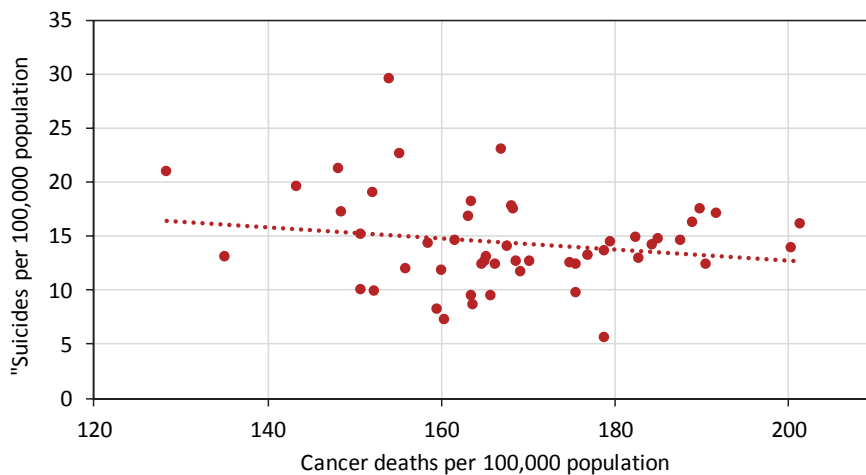


Figure 3: USA state cancer death and suicide rates 2012

Source: CDC Wonder

There is no significant correlation between cancer death rates and general suicides by state (adjusted $r^2 = 0.01$, $p = 0.21$).

Such straightforward research and analyses to test initial assumptions would have alerted Jones & Paton to not only the extremely low potential rate of, but the near statistical impossibility of their model detecting, substitution.

[‡] It is conceivable that poor physician access may also be related to untreated *mental* illness, but separate analysis shows no significant correlation between general suicide rates and unmet mental illness treatment needs (U.S. National Survey on Drug Use and Health).

Sin 4: Premise bias — delay sensitivity

Jones & Paton state that some of those accessing assisted dying are dissuaded from suiciding early, living longer than had they suicided early, violently and alone. They project this observation into a claim that such a death-delaying effect would result in the average age of *general* suicides increasing after a DWDA came into effect.

Crucial to this analysis is that Oregon and Washington DWDAs require a terminal illness with a prognosis of *death within six months*, because this qualification criterion places significant limits on the amount of time by which life may have been extended. While experience shows that a small minority of those accessing the DWDAs live longer than the anticipated six months, most don't. And only *some* of these will be life-extending.

Jones & Paton do not mention or consider the six-month requirement.

Small detection chance

For the sake of simplicity, let's say that *on average*, an assisted death in Oregon and Washington delays what would have been a general suicide by two months. Age at death is of course recorded in years, so the chance of a change of age at death is just 1 in 6 (2 months versus 12 months). That's a small chance of finding a significant change.

Miniscule detection chance

But it gets worse (part 1). Jones & Paton used CDC suicide statistical data on general suicides to calculate the 'mean age at death,' and the CDC suicide data is in *5-year age cohorts* (e.g 20–24 years).[§] That means the average possible movement from one age cohort to the next is two in sixty months, or a chance of just 1 in 30 to appear as a change in age-at-death in CDC data. Indeed, for age cohorts over 24, the CDC database ranges are in 10-year blocks, meaning for most ages it's a miniscule 1 in 60 chance.

Vanishing detection chance

But it gets worse (part 2). We previously established that the likely maximum substitution ratio of suicide to assisted death in Oregon is around 1%. So only 1 in 100 general suicides are even *candidates* for a change of age-at-death. That makes the chance of detecting a change a vanishingly small 1 in 6000 (1 in 60 x 100). No wonder Jones & Paton's general econometric model didn't return a significant result.

Jones & Paton pointed vaguely to irrelevant data to create an impression of significant substitution possibility, while ignoring direct and specific evidence to the contrary. Ultimately, it was indefensible to pursue a general econometric modelling approach to attempt to answer this hypothesis.

Jones & Paton not only failed to conduct any realistic numeric pre-assessment of whether their tests could detect their postulated delay effect, they completely failed to mention a critical fact about qualification for Oregon and Washington DWDA: prognosis of less than six months to death.

[§] Confirmed via private correspondence with author David Paton.

Sin 5: Theory-versus-method bias

In attempting to answer the question posed by their premise of suicide contagion from assisted dying, Jones & Paton chose an econometric modelling method. They incorporated a range of variables into various mathematical equations to try and approximate a real-world answer. Coincidentally, David Paton is a Professor of industrial economics.

Conflicting anticipated results

Jones & Paton begin their study by introducing several **conflicting** possible results. In their Abstract and Key Points, they argue for a reduction in total suicides.

Abstract: “It has been suggested that legalisation [of PAS] could lead to a **reduction in total suicides** and to a delay in those suicides that do occur.”

Key points: “It has been claimed that the legalization of PAS is likely to lead to a **reduction** in other suicides and in the **total number of suicides** (including PAS).”

However, in the introduction section, they argue the opposite, for an *increase* in total suicides:

Introduction: “The legalisation of PAS could provide an alternative to nonassisted suicide for people with chronic or terminal illnesses.** If so, the direct effect of legalising PAS would be for the **total number** of intentional self-inflicted deaths (including assisted suicides) to **increase** but for deaths by nonassisted suicide to decrease.”

In their *discussion* section, Jones & Paton acknowledge that their observed results of no significant change could be the result of opposite forces balancing each other out:

Discussion: “It should be noted that the rise in overall [that is ‘total’] rates of death by suicide and the absence of a fall in rates of nonassisted suicide are both **net effects** and do not necessarily mean that legalising PAS has no suicide-inhibiting effects... Rather, the results suggest that if such inhibitory mechanisms exist, they are **counteracted by equal or larger opposite effects**.”

Establishing a specific theory and mechanism

A foundational principle of scientific research is that you don’t just run a gaggle of ad hoc analyses and then pick and choose the results that seem to support your pet theory. Rather, you must clearly establish a coherent and specific theory *first*, including precise mechanisms by which you anticipate effects are generated from particular causes. You *then* design experiments to specifically test *that theory* and *those mechanisms*.

Jones & Paton make conflicting statements about their expected outcomes, and then adopt a research method that couldn’t hope to answer their research questions, particularly about causation.

** Jones & Paton wrongly asserted here that the *chronically* ill, and *all* terminally ill, qualify for assisted death in Oregon and Washington, when they don’t.

Jones & Paton's primary mechanism

Even more telling than their conflicting anticipated outcomes is that rather than introduce their specific "suicide contagion" mechanism up front before the design of the research, Jones & Paton only introduce it at the *end of the discussion section*, that is, as they try and justify and explain the results they actually obtained.

And what is their proposed, specific model of suicide contagion?

"Persons socialised in nations with relatively high rates of suicide are more likely to be exposed to suicidal role models, which provide positive definitions of suicide," which "increase[s] the level of individual approval of suicide and therefore reinforce[s] the high rate of suicide within the culture."

Primary result not connected to primary mechanism

Given that Jones & Paton's *primary theory* was 'general cultural changes in attitudes toward suicide,' potential causation would only be established if they found that the *general* suicide rate increased. After all, there are vastly more general suicides than assisted deaths in Oregon and Washington, so if their theory were true the effect should be easy to detect.

But their results found *no general increase*. They then failed to connect their primary result with their primary theory, and instead went on in detail to explain away the various non-significant results they obtained.

Thus, from the outset, Jones & Paton offer conflicting theories about expected effects, and then later failed to connect their primary (non-supporting) result to their primary stated mechanism. That's a serious scientific offence.

Direct evidence overlooked

Also pertinent is the question as to why Jones & Paton chose a general econometric model relying on complex, error-prone (and poorly executed) mathematical approximations, when more direct data was readily available to help answer the research questions.

Contrary direct evidence: actual suicide contagion rate

Oregon participates in a dedicated multi-state, government-sponsored study which forensically examines all suicides using carefully set standards, and collates and publishes detailed statistics: the National Violent Deaths Reporting System (NVDRS).^{47††}

Jones & Paton expressly ignored direct quantitative evidence in an NVDRS source they actually cited, that Oregon's suicide contagion rate was just 1% in 2009. Between 2003 and 2014, the rate of suicide contagion was 1.62%. That is, in 1.62% of all suicides, the suicide of a relative or friend in the past five years

Jones & Paton fail to make the appropriate connection between their primary theoretical mechanism (general social approval of suicide) and their primary research result (no increase in general suicides): that is, that their theory was not supported by their results.

†† Washington state is not an NVDRS participant, so these statistics are unavailable for that state.

was a *contributing* factor (not necessarily the only factor). The maths is straightforward: in 2014, 105 Oregonians used the DWDA to hasten their death in response to a terminal illness. Therefore, even *if* we entertain for a moment the premise that assisted deaths are equivalent to general suicides — when we established in “Sin 1” that they aren’t — a total of 1.7 suicide cases (1.62% of 105) in 2014 might be attributable to the DWDA. And of course, even less in earlier years.

Further simple calculations are informative. In 2014, there were 750 general suicides in Oregon, and therefore around 12 cases of actual suicide contagion. Thus, according to Jones & Paton’s general contagion theory there would be, in 2014, fewer than 2 “assisted-suicide contagions” in 855 “total suicides” (general suicides plus equating DWDA deaths with general suicides as Jones & Paton do). Oregon’s general suicide count naturally varies on average by about 5% a year: equivalent to around 38 cases in 2014.

Therefore, even entertaining Jones & Paton’s “suicide contagion” theory from assisted deaths, it would be impossible for even a well-run general econometric study to detect such a small potential change.

Contrary direct evidence: state rates and rankings

Other much more direct evidence is available too, from the state suicide data that Jones & Paton retrieved from the USA Centers for Disease Control (CDC) online Wonder database. With this data, it is easily possible to determine each state’s *ranking* for suicide rates. If DWDAAs were responsible for general suicide contagion as Jones & Paton theorise, then we would expect to see Oregon and Washington’s general suicide rankings among the USA states *worsen* after their DWDAAs.

In fact, the opposite is true: both Oregon and Washington’s state suicide rate rankings improved significantly after their DWDAAs came into effect (see “Sin 7: Comparing like with like rankings” on page 26.)

*Jones & Paton fail to introduce, at the **start** of their study, a precise mechanism by which they argue that contagion may occur from assisted deaths to general suicides. They invoked a wobbly econometric approach without checking whether it was up to the task of detecting the necessary associations, while overlooking direct and readily-available evidence to the contrary of their theory, including in a source they cited.*

*They then failed to join the research dots from their primary result (no relative increase in general suicide rates) to their primary theoretical mechanism (generally more positive attitudes toward suicide), instead explaining the detail of selected unhelpful results away with more complex theories **after** they ran their models, while overlooking readily-available and more concrete data to the contrary of their theory; and the obvious answer of “no correlation/causation.”*

Jones & Paton resort to a general econometric modelling approach while ignoring direct, concrete and readily-available “suicide contagion” data.

Sin 6: Selection bias — test cohort

Jones & Paton chose USA states Oregon and Washington as their *test* jurisdictions for their “assisted dying contagion theory.” The general suicide rates in both states have risen in recent years.

However, there are multiple jurisdictions where assisted dying is legal (including what Jones & Paton refer to as “assisted suicide”), with readily-available data on assisted deaths and general suicide rates. But Jones & Paton don’t mention them.

Why not test Switzerland?

Extensive longitudinal Swiss data is compelling in regard to the effects of lawful assisted dying on general suicide rates and “contagion” (Figure 4).

Jones & Paton select Oregon and Washington as their test cohort: their suicide rate data looked like they might support their “suicide contagion” theory. However, Jones & Paton comprehensively failed to mention other major assisted dying jurisdictions whose data already strongly contradicted the theory.

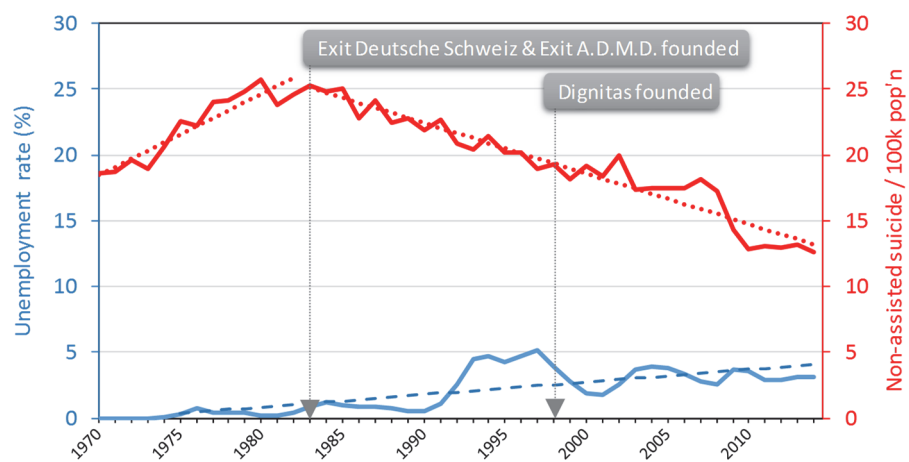


Figure 4: General suicide rate trends in Switzerland since 1970
Source: Federal Statistical Office (Switzerland)

The rate of Swiss general suicides rose from at least 1970 to the early 1980s. Then, in 1983, two associations were established to provide Swiss residents with an accompanied death or ‘assisted suicide’ under the law: Exit Deutche Schweiz for German-speaking Swiss residents, and Exit ADMD for French-speaking Swiss residents. Neither association provides membership or accompaniment for non-residents.

In 1998, Dignitas was founded. It provides membership and accompaniment for mostly non-residents, that is, foreigners.

From 1983 onwards, since the two domestic societies were established, the general (non-assisted) suicide rate began, and has continued, a long-term downward trend. The rate continued its downward trend when Dignitas began accompanying foreigner deaths. And this occurred consistently *in the face of a rising unemployment rate* (a well-established risk factor for suicide) and increasing public discussion of assisted deaths.

Switzerland is an *inconvenient truth* to Jones & Paton's suggestion that they found no statistical evidence of reduced suicide rates in the face of rising use of assisted dying (see Figure 1); evidence of substitution.

Why not test the Netherlands?

Jones & Paton overlook the Netherlands, too, as their preferred test cohort, despite it having lawful assisted dying for much longer than Oregon and Washington. Figure 5 shows a history of assisted dying and general suicides in the Netherlands.

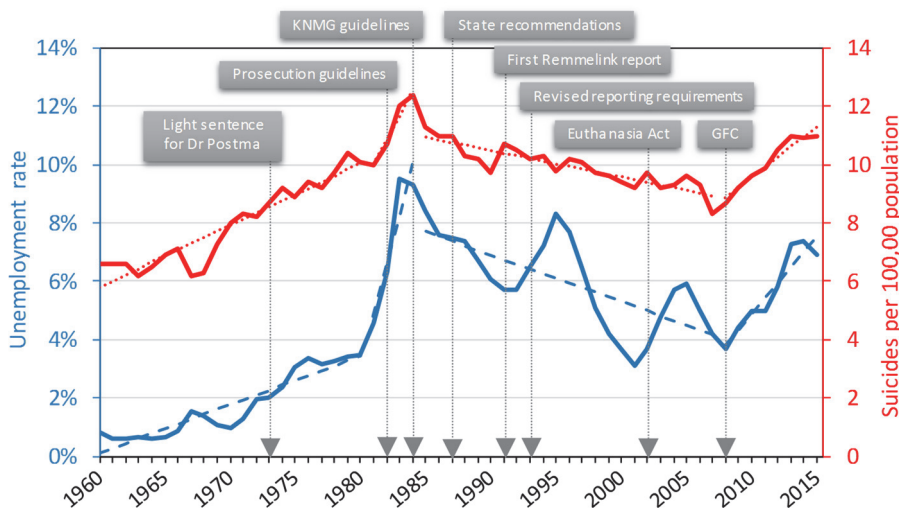


Figure 5: Dutch assisted dying and suicide rates

Sources: CBS Statistics Netherlands

Trends in Dutch suicide rates closely follow the unemployment rate. Unemployment alone explains 80% of the variation in Dutch suicide rates between 1960 and 2015. And from the year that the Royal Dutch Medical Association (KNMG) first issued structured guidelines for doctors to participate in assisted dying, 1984, there's been a long-term downward trend in the suicide rate. As discussed in more detail elsewhere,⁴⁸ the rate only increased from 2008 onwards when the Netherlands was particularly hard-hit by the global financial crisis (GFC).

This too is an *inconvenient truth* against Jones & Paton's suicide contagion theory.

Since Jones & Paton's study, anti-assisted dying lobbyists such as Prof. Margaret Somerville continue to claim that the rise in Dutch suicide rates since 2008 represent 'contagion' from the increase in assisted deaths since then. However, the theory is contradicted again by concrete and specific evidence.⁴⁹

In the Netherlands, there are five major regions each with their own Euthanasia Commission, and each Commission collects and reports its own data. This permits rates to be compared amongst regions.

Figure 6 shows the assisted dying rate and the general suicide rate by region for 2014. Region 3 (North-West) includes Amsterdam and surrounding urban areas.

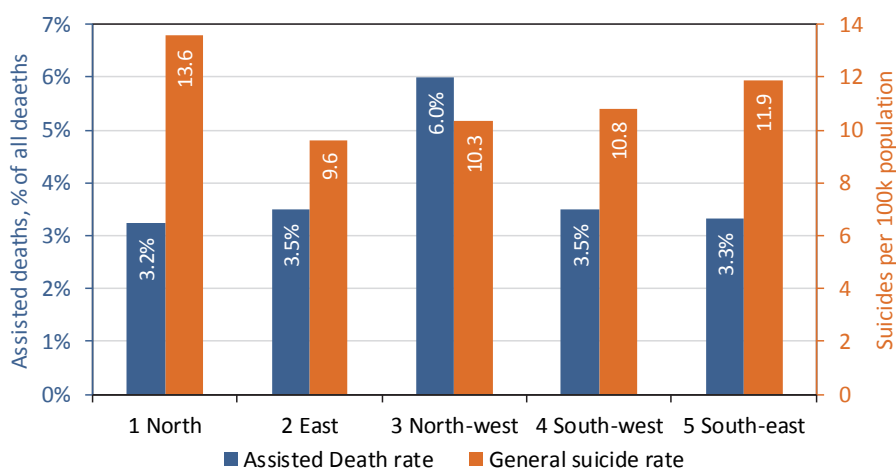


Figure 6: Assisted dying and general suicide rates of the five Dutch regions, 2014
Sources: Euthanasia Commission Reports, Dutch Government statistics, Koopman & Putter⁴⁹

Anti-assisted-dying lobbyist claims of suicide contagion in the Netherlands since 2008 are contradicted by the regional evidence.

If Jones & Paton's suicide contagion theory were correct, the region with the highest assisted death rate would also have the highest general suicide rate. But North-West, with by far the highest assisted death rate, has the second-lowest suicide rate; and North, with the lowest assisted death rate, has by far the highest suicide rate.

This data from the Netherlands, including the regional analysis published after Jones & Paton's study, is also an *inconvenient truth* to their suicide contagion theory, and in part suggests (though doesn't prove) assisted death substitution from general suicide.

Why not test Belgium?

Unlike the Netherlands, assisted dying was completely illegal in Belgium prior to its Euthanasia Act, which came into effect in late September 2002.⁵⁰ All the online, publicly available government data for unemployment and general suicide in Belgium are presented in Figure 7.

The unemployment rate shows that labour in Belgium, unlike the Netherlands, was unaffected during the GFC, and that the general suicide rate has substantially dropped since Belgium's Euthanasia Act came into effect.

Indeed, the data indicates that the general suicide rate after the Act is decreasing both *faster* than it did before the Act, and *faster* than the fall in unemployment rate since. Comparing the pre- with the post- data, the trends in unemployment and general suicide are statistically similar prior to the Euthanasia Act (1987–2001) ($p < 0.05$), but different after it (2003–2013). For means, the post-Act unemployment rate is not significantly different from the pre-Act unemployment rate ($p = 0.60$), while the post-Act mean suicide rate is significantly lower than the pre-Act rate ($p < 0.01$).

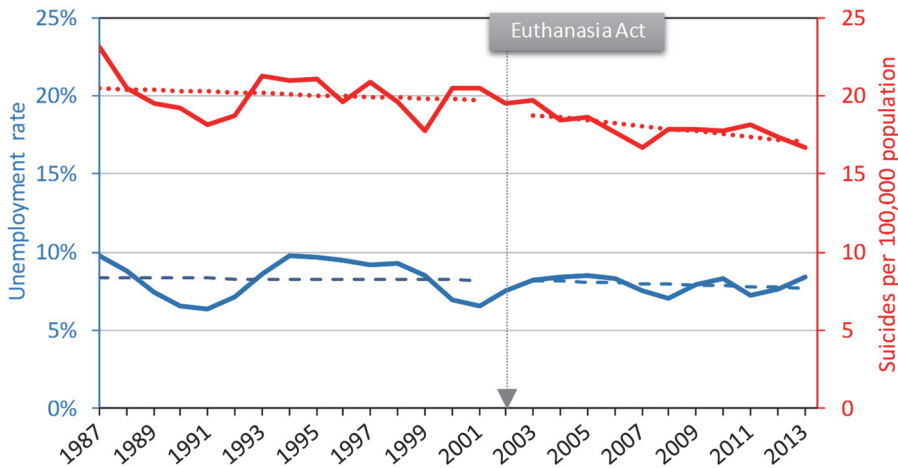


Figure 7: Belgium unemployment and general suicide rates

Sources: OECD Data; Eurostat

Belgium, too, represents an *inconvenient truth* to Jones & Paton's 'suicide contagion' theory.

All support "substitution" theory

Thus, of all three major jurisdictions with assisted dying laws, the empirical evidence contradicts Jones & Paton's premise of "suicide contagion" and their conclusion that assisted dying does not result in substitution from general suicide.

It's important to note that the assisted dying laws in Switzerland, the Netherlands and Belgium are all much broader, permitting assisted death in relation to severe *chronic* illness, while Oregon and Washington's laws permit assisted death only in relation to a terminal illness with less than six months to live. Thus, Oregon and Washington were the jurisdictions least likely to provide evidence for the "substitution" theory.

Jones & Paton not only failed to use any of these jurisdictions — all with readily-available data — as their test cohort, but didn't mention, while arguing the case for "general suicide substitution," that data from these jurisdictions all supported their premise while it was almost impossible for Oregon and Washington to do so.

Jones & Paton would have been hard-pressed to select a 'test' cohort (Oregon and Washington) less likely to be able to confirm "substitution" theory.

Sin 7: Selection bias — control cohort

Jones & Paton’s handling of the *control* cohort (the group against which the test cohort is compared) is also deeply flawed, through simply averaging all the other states and making a single comparison.

The average ‘coverup’

While comparing a test cohort with an “all others” control cohort is often conducted as a first step in testing for differences, alone it is insufficient for asserting causation. That’s because a multi-member *average* control will smooth out any significant differences amongst control members (in this case USA states other than Oregon and Washington), some of which may mirror or exceed the observations of the test group (Oregon and Washington).

Jones & Paton constructed their ‘control’ cohort in a manner whose most likely consequence would be to conceal significant and relevant variances amongst other, similar states.

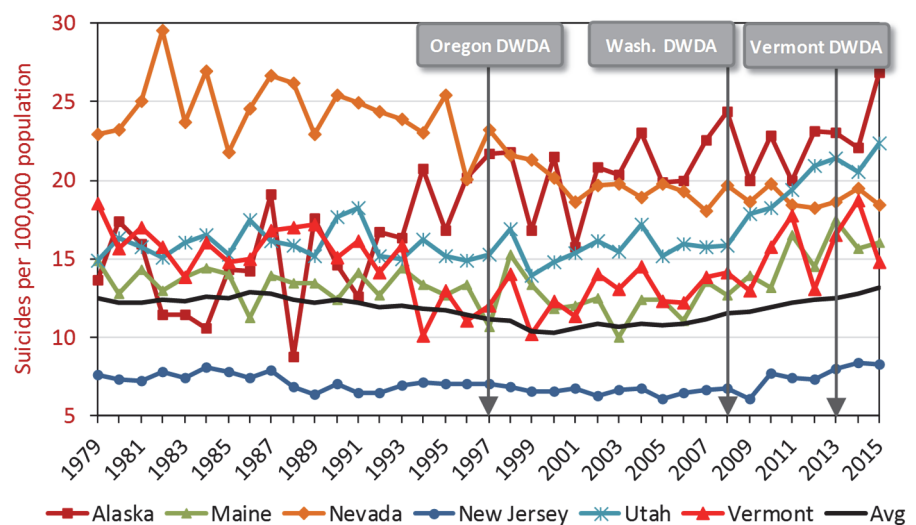


Figure 8: Suicide rate trends in various USA states

Source: CDC Wonder. Note: all series are *unsmoothed*; Avg = all states excluding Oregon, Washington

Figure 8 illustrates the trends in suicide rates of several states. It’s obvious that there are very large differences between them, such as Nevada’s high but falling rate, Alaska’s high and rising rate, and New Jersey’s low and mostly level rate.

The following are claims about *individual* state changes that could be made by statistical “association”:

- Vermont’s general suicide rate was falling prior to but increased significantly after *Oregon’s* DWDA came into effect;
- Maine, Utah and New Jersey’s general suicide rates all increased significantly after *Washington’s* DWDA came into effect; and
- Alaska’s general suicide rate has increased to a record level since *Vermont’s* DWDA came into effect.

Obviously, it would be nonsense to argue causation in these unrelated cases. Other significant factors must account for these spurious inter-state “associations,” and may equally explain any observed intra-state associations.

Also obvious is that the average of all states excluding Oregon and Washington ('Avg' in Figure 8) considerably smooths out and hides very substantial differences amongst the individual 'control' states.

Comparing like with like rates

One form of crucial analysis in testing for causation is to compare like with like — for example to compare Oregon and its DWDA with another individual state very like it, but which doesn't have a DWDA.

Oklahoma fits that bill very well. It's very similar to Oregon in many important respects, such as an almost identical population size, similar capital-city/rural population split, similar average levels of education, similar average household incomes, and a similar mix of primary, secondary and government industry sectors.

Oklahoma's unemployment rate is significantly lower than Oregon's and the Oklahoma population is significantly more religious, which should result in lower suicide rates. On the other side of the ledger with known risk factors for suicide, Oklahoma's divorce rate is somewhat higher than Oregon's, and a slightly higher proportion of Oklahoma households own firearms.

Oklahoma has never had an assisted dying law, and yet, comparing 18 years of its post-Oregon DWDA data with 18 years of pre- data, its suicide rate increase is *larger* than Oregon's, (Figure 9).

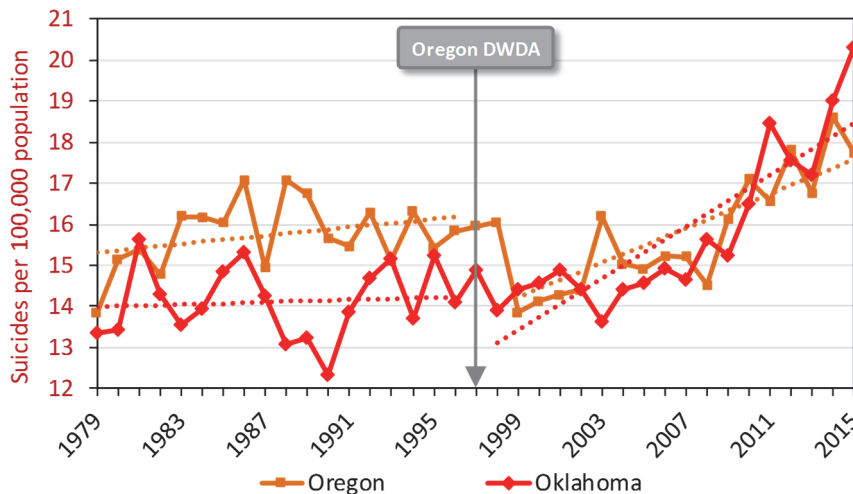


Figure 9: Oregon and Oklahoma suicide rates 1979–2015

Source: CDC Wonder. Note: The year of Oregon's DWDA (1997) is excluded from both pre- and post-data analysis.

While Oklahoma's average suicide rate prior to Oregon's DWDA was significantly lower than Oregon's (14.1 versus 15.7, $p < 0.0001$), since Oregon's DWDA, Oklahoma's rate has risen dramatically (mean 15.8). Oregon's average suicide rate, however, didn't change significantly (from 15.7 to 15.8, $p = 0.44$).

Jones & Paton failed to conduct any 'like versus like' tests, one of the foundations of establishing causation. Oklahoma is very like Oregon, but with no DWDA. Oklahoma's suicide rate has deteriorated significantly while Oregon's raking has improved.

Both Oregon and Oklahoma’s post-DWDA suicide rate trends are significantly different from their pre-trends (Oregon $p < 0.01$; Oklahoma $p < 0.0001$), but not significantly different from each other ($p = 0.49$).

That is, the suicide rate has risen in both states at similar rates since Oregon’s DWDA, but in Oklahoma it has risen from a much lower pre-DWDA base.

Comparing like with like rankings

A further useful test is to compare the longitudinal *state ranking* of Oregon and Oklahoma suicide rates amongst all states, since this too compares all states individually instead of ‘test’ versus an ‘all-others average control’ (Figure 10).

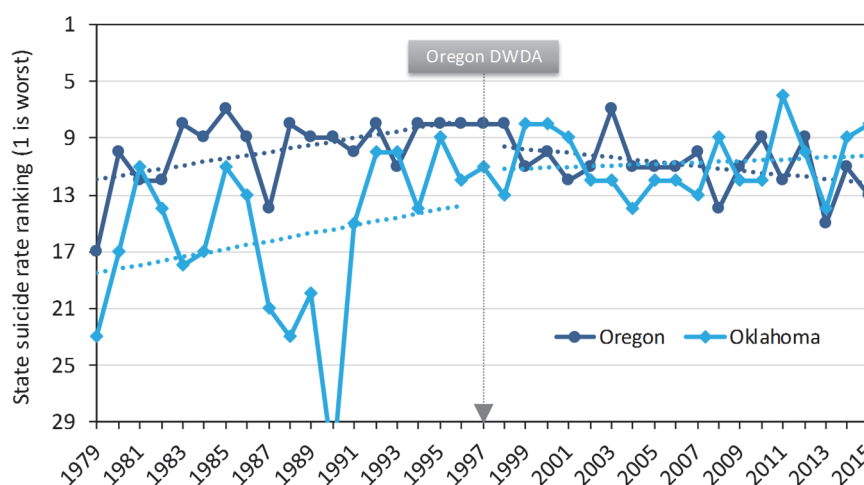


Figure 10: Oregon and Oklahoma state suicide rate ranking 1979–2015
 Source: CDC Wonder. Note: Ranking 1 is the *highest/worst* suicide rate. The year of Oregon’s DWDA (1997) is excluded from both pre- and post-data analysis.

Oklahoma’s mean state ranking was significantly better (16.1) prior to the year of Oregon’s DWDA than afterwards (10.7, $p < 0.001$), whereas Oregon’s pre- and post- means were not significantly different. Further, While Oklahoma’s post-trend is not significantly different from its pre-trend, Oregon’s pre- and post-trends are significantly different ($p < 0.01$).

Thus, while Oklahoma’s average suicide ranking has significantly worsened since Oregon’s DWDA, Oregon’s average ranking hasn’t changed, and its ranking trend has reversed from deteriorating to improving.

In simple counts, Oregon was in the top 10 suicide rankings for 13 of the 18 years pre-DWDA, but just 6 of the 18 post- years. Oklahoma on the other hand was in the top 10 just 3 times pre-DWDA but 8 times post-.

Washington’s ranking likewise has improved since its DWDA was passed in 2008 (Figure 11). Its average 7-year post-DWDA ranking (22.4) is better than but not significantly different from its 7-year pre- ranking (19.7), but significantly different from (better than) its 18-year pre- ranking (16.9, $p < 0.0001$). In simple counts, Washington appeared in the top 20 in 4 out of 7 pre- years, but just 1 of the 7 post- years.

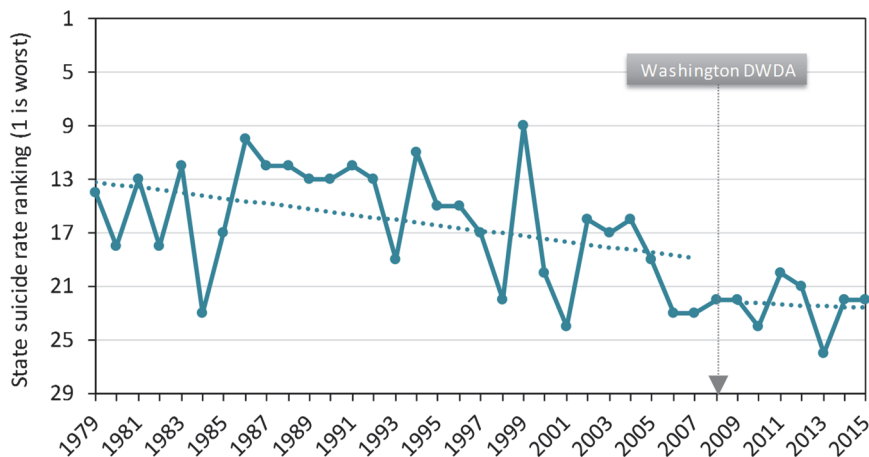


Figure 11: Washington state suicide rate ranking 1979–2015

Source: CDC Wonder. Note: Ranking 1 is the *highest/worst* suicide rate.

Relevant controls contradict “contagion” theory

This empirical “comparing like with like” data contradicts Jones & Paton’s “suicide contagion from DWDA” theory. Indeed, while it might be tempting to claim that the Oregon/Oklahoma comparison indicates some ‘protection’ from the DWDA against general suicide in Oregon, there are too many uncontrolled variables to draw such a conclusion.

Jones & Paton chose to check the test states only against a control of the average of all other states, a methodology bound to conceal similar or worse variances amongst the control states. They failed to compare like with like, an important step on the path to testing for causation rather than mere correlation.

Jones & Paton collected all the necessary data (CDC Wonder database suicide rates) to run these “like with like” tests, but didn’t conduct them (or at least didn’t report them).

Sin 8: Confounding factor control bias

To be able to claim a causative relationship between two variables that demonstrate a statistical correlation, it is necessary to control for all likely confounding factors — in this case, things other than assisted dying which may cause general suicide rates to change.

In the kind of econometric modelling that Jones & Paton undertake, it isn't possible to control for *all* confounding factors, because suitable data is not available for them all. But at the very least, it is critical to control for as many significant confounding factors as data permits, to begin building confidence in claims of correlation or not (let alone causation).

A detailed consideration of risk and protective factors for general suicide appears in Appendix A. While there are many potential confounding factors, Jones & Paton attempt to control only the following in their model:

- Unemployment;
- Per capita real income;
- Any religious affiliation;
- Proportion of Blacks and Hispanics;
- Marijuana decriminalisation; and
- Drink-driving 0.08 blood alcohol law.

Table 1 lists the state-level bivariate correlations of potential confounding factors for which data was readily available online from authoritative sources, presented in decreasing order of correlation degree and statistical significance. A more detailed discussion of each factor appears in Appendix B.

Factors well controlled

Jones & Paton controlled well for two factors: proportion of Black/African American population (22% covariance with suicide rates, $p < 0.001$), and per-capita personal income (13% covariance, $p < 0.01$).

Factors poorly controlled

Jones & Paton controlled poorly for several factors.

For alcohol consumption, they used only a binary flag for years in which each state had a drink-driving 0.08% blood alcohol law, when quantitative state alcohol consumption (via ethanol sales volumes) data was readily available (13% covariance with suicide rates, $p < 0.01$), and a source they cited expressly stating that 0.08% drink driving laws are *not* good predictors of suicide rates.⁴²

For illicit drug use, Jones & Paton used only a binary flag for state legalisation of marijuana for either medical or recreational use, which they counted as a *protective* factor, when marijuana is responsible for one third of USA drug abuse emergency admissions, and while omitting data for any 'hard' drugs. In any case, semiquantitative state marijuana use (percent of population who

Jones & Paton controlled for a handful of potential confounding factors. However, they omitted the most significant ones, included at least one that a source they cited said wasn't relevant, and made a hash of at least two.

used in the past month) was readily available, and was found to be a non-significant factor (0% covariance with state suicide rates).

Table 1: USA state suicide rate confounding factor bivariate analysis*

Factor	Expected effect	Observed effect	Year [†]	Variance explained [§]	p [^]
Low population density ¹	Risk	Risk	2010	73%	< 0.001
Household firearm ownership	Risk	Risk	2007	48%	< 0.001
Divorce	Risk	Risk	2008	45%	< 0.001
Low physician density	Risk	Risk	2012	36%	< 0.001
Education: higher degree	Protection	Protection	2011	28%	< 0.001
Fertility	Protection	Risk	2010	27%	< 0.001
Serious mental illness past year	Risk	Risk	2012	24%	0.001
Education: bachelor degree	Protection	Protection	2011	22%	< 0.001
Black/African American %	Protection	Protection	2012	22%	< 0.001
Rate of marriages ²	Protection	Risk	2010	22%	< 0.001
State spending on local welfare	Protection	Protection	2012	19%	0.001
Percent married ²	Protection	Risk	2012	15%	< 0.01
Per-capita personal income	Protection	Protection	2012	13%	< 0.01
Alcohol consumption ^{2,3}	Risk	Risk	2010	13%	< 0.01
Cocaine use in past year	Risk	Protection	2012	8%	< 0.05
Illicit analgesic use in past year	Risk	Risk	2012	8%	< 0.05
Any religious affiliation	Protection	None	2010	5%	NS
Unemployment	Risk	None	2012	4%	NS
Rate of cancer deaths	Risk	None	2012	1%	NS
Average IQ	Protection	None	2006	0%	NS
Marijuana use ⁴	Risk	None	2012	0%	NS
Hispanic/Latino %	Protection	None	2012	0%	NS

* Factors in shaded rows were included in Jones & Paton's model. † Year of data pair analysed.

§ Adjusted r^2 expressed as a percentage. Total variance explained exceeds 100% because factors are not completely independent. For example, population density explains 63% of variance in household firearm ownership, 40% of the variance in fertility, 30% of variance in Black/African American race and 23% of variance in cocaine use; holding a bachelor degree explains 23% of variance in religious affiliation, 39% of variance in firearm ownership and 28% of variance in the divorce rate. ^ NS = Not significant. 1: Log of population density. 2: Excluding significant outliers 3: Jones & Paton used a binary measure of 0.08 drink-driving law by state, while this analysis used state average alcohol consumption data (gallons/year). 4. Jones & Paton used a binary measure of whether marijuana use was legalised by state, while this analysis used state-level percentage of any use of marijuana in the past month.

Jones & Paton failed to control for numerous suicide-rate confounding factors, including eleven of the top twelve.

Jones & Paton also 'controlled' for religious affiliation, unemployment, and proportion of Hispanic/Latino population, when their *actual* covariance with suicide rates *at the state level* was low: 5%, 4% and 0% covariance respectively, none of them statistically significant. This would have reduced any potential sensitivity of their model. Jones & Paton assumed that published studies finding a correlation between a factor and suicide in local or single-site studies would apply equally to *USA state-level* studies, when they didn't.

It would have been prudent, since they had already collected the necessary data on these factors, to check state-level correlations for significance to confirm each factor was worthy of inclusion in their model.

Factors not controlled

Jones & Paton failed to control for a *wide range* of factors whose important contribution to suicide rates is well-established: for example, rural versus

urban residence, household firearm ownership, mental illness, divorce and education.

Further, while quantitative data can sometimes be hard or impossible to obtain — particularly for protective factors such as ease of access to crisis help lines — authoritative data for many crucial factors for their study were available freely online, so there was no legitimate reason to exclude them. That they neglected to include these crucial factors suggests they are at the very least inexperienced in the subject matter.

By far the greatest covariant of suicide rates found in this examination (73% of covariance, $p < 0.001$) was low population density. It is in part a proxy measure for the accessibility of social and professional support. Household firearm ownership — firearms being the most lethal suicide method — also correlated strongly (48%, $p < 0.001$) as did the divorce rate (45%, $p < 0.001$).

Other covariates were significant, too, including education (28% higher degree, 22% bachelor degree, both $p < 0.001$), fertility (27%, $p < 0.001$), serious mental illness (24%, $p = 0.001$), rate of new marriages (22%, $p < 0.001$) and marital status (15%, $p < 0.01$), and use of cocaine and non-medical use of pain relievers in the past year (each 8%, $p < 0.05$).

Contagion a small contributor

Jones & Paton ignored the specific evidence in a source they cited that clearly stated the “suicide contagion” rate to be just 1%. Other readily-available NVDRS data clearly demonstrates that different risk factors predominate at different ages, too. For example in Oregon, family stressors predominate amongst teens, intimate partner problems in the 20s and 30s, diagnosed mental illness in the 40s and 50s, and physical health problems in older age (Figure 12).

Direct and concrete evidence exists of complex interactions between risk factors across different age groups. Across all age groups, the “suicide contagion” rate in Oregon is very low.

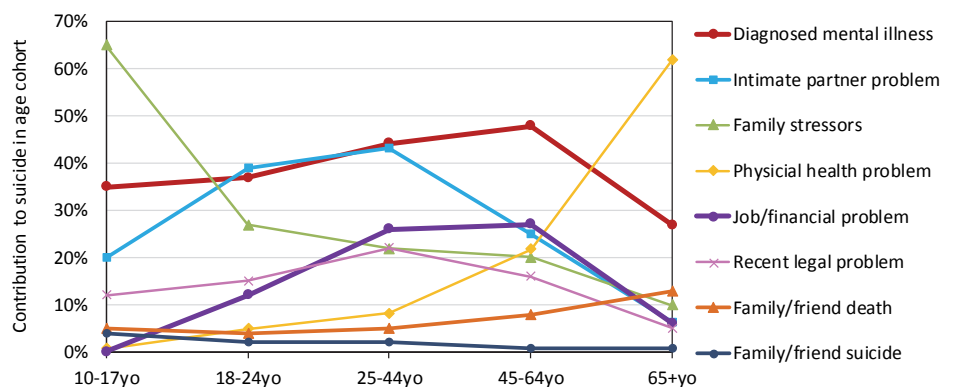


Figure 12: Oregon suicide circumstances 2003-2012
Sources: Shen & Millet (2015), personal correspondence with Oregon DHS.

However, across all age groups, the suicide of a family member or friend in the past five years is a very small contributor to suicides: suicide contagion.

Overall, given the significant interrelationships between the numerous risk and protective factors covered in this examination, a compound total of 423% of suicide rate variance was explained by well-known factors for which robust data was readily available. Yet Jones & Paton's "carefully controlled study" covered a compound total of just 35% of significant factors, or just 8% of the total explained variance, and omitting eleven of the top twelve factors.

Jones & Paton demonstrated a poor grasp of the complexity of confounding (risk and protective) factors in suicide, assuming that "contagion" was a numerically-large contributor when it wasn't, and even when a source they cited specifically gave the quantitative answer of just 1%. They also failed to search for authoritative data for confounding factors, and failed to test their assumption that their included factors actually exhibited relevant state covariance with suicide rates. These multiple failures to control for major confounding factors comprehensively invalidates their model, and their conclusions of it.

Sin 9: Interpretation bias — ‘association’

Jones & Paton, and especially Kheriaty, make unjustifiably confident statements about the meaningfulness of the study results.

Study limitations, once more

It’s important again to stress that *all* scientific studies have limitations, and the point of this examination of Jones & Paton’s article and Kheriaty’s editorial of it was not to critique limitations beyond the authors’ control, such as the omission of variables for which no data was available, even if proxy measures.

It’s worth reviewing the limitations Jones & Paton discussed in their article, and comparing them with the limitations found in this assessment. Jones & Paton properly state that “there are several limitations to the analysis in this study that suggest *our results should be treated with some caution*,” (emphasis added) identifying the following:

1. Questions of public policy are not addressed;
2. The complex issue of assisted dying cannot be resolved purely by statistical analysis;
3. Whether prevention strategies that are effective for general suicides are also effective in assisted death cases [note the inherent suicide-equivalence bias];
4. That many people may not have been eligible for assisted death;
5. Few USA states have legalised assisted dying and therefore it is difficult to generalise their results;
6. Those states (at the time of their study) with DWDAs were all in the north of the nation;
7. Limited data was available from some states that had more recently legalised assisted dying;
8. That their observations were net effects and that net effects may hide significant but opposite co-trends; and
9. There may “remain other unobservable factors affecting suicide rates and that are correlated with the legalisation of assisted suicide and that may affect our conclusions”.

Jones & Paton’s discussion of study limitations in part confirms premise and preferred outcome bias.

Signs of preferred outcome bias

Jones & Paton identify in their “limitations” discussion few of the significant ‘scientific sins’ outlined in this examination. They do identify in point 9 above that there may have been other factors that affected suicide rates that weren’t included in their study. *But note how they phrase it:*

- They say that the factors, if any, were “*unobservable*.” To the contrary, there are many, complex risk and protective factors for suicide, well-documented in the literature, and there are many quantitative and semi-quantitative measures and data for them, as outlined in this examination.
- They mentioned “unobservable” factors only “that are correlated with the legalisation of assisted suicide.” They do *not* mention

“unobservable” (or observable) factors which give rise to significant changes in suicide rates but are *unrelated* to assisted dying. Of these there are many.

Indeed, there are several further flags that suggest that they were intent of finding “suicide contagion” and “no substitution.”

Firstly, they failed to conduct a proper and balanced examination of the existing professional literature on the subject, overlooking clear evidence that suicide contagion from assisted deaths is *less* rather than more likely, instead equating the two throughout their article.

Secondly, they failed to report critical information in sources they cited, or report other existing sources (e.g. Switzerland, Belgium, Netherlands), whose empirical evidence was unhelpful or contrary to their conclusions.

Thirdly, they failed to test their premise that certain factors correlate with general suicide rates *at the state level*, even though, for some variables they collected the data to do so.

Fourthly, they failed to adequately test whether their proposed model would indeed be appropriate and sensitive enough to answer their research question, citing inappropriate figures or no figures at all to assume it could detect both significant “contagion” and “substitution.”

And fifthly, they batted away the possible problem of methodological bias or statistical doubt by saying that “total suicides” (i.e. including assisted deaths) increasing while there was no decrease in general suicides, “provide some reassurance of the robustness of our results.”

The bias rot spreads

This last flag is worthy of extra attention. Note that, having been unable to find a statistical correlation between assisted dying and general suicide rates (their primary premise), Jones & Paton add assisted death figures to the general suicide figures (confirming their equivalence bias), to say that “total suicides” have increased with the introduction of Oregon and Washington’s DWDA.

That’s not a scientific finding — it’s a circular argument. What Jones & Paton in effect state is that since assisted dying was legalised in Oregon and Washington:

“the increase in assisted deaths correlates significantly with ... the increase in assisted deaths.”

Clearly, Jones & Paton are unfamiliar with, or don’t much care for, the important distinction between statistical clusters and epidemiological contagion (see Appendix A). In Oregon in 2014, 34,151 people died. Just 105 of those deaths were assisted, and it is exceedingly unlikely that many or any of those persons knew each other. The assisted deaths are a statistical observation (a cluster), not established causative vector (contagion).

Jones & Paton’s primary ‘result’, “increase in total suicides” is not in fact a scientific finding. It’s a circular argument: that the rate of assisted deaths correlates with the rate of assisted deaths.

Jones & Paton's conclusion that when a particular choice is legalised, any increased exercise of the choice is 'contagion,' is a moral opinion and not an epidemiological conclusion.

Then, despite having said earlier that "*our results should be treated with some caution,*" Jones & Paton sum up the circular 'proof' of increasing assisted deaths, plus the lack of finding a drop in general suicide rates (which their study couldn't hope to find) to say that this "*provides some reassurance of the robustness of our results.*"

That's self-justification bias: arguing that 'robustness' is 'reassured' because two statistical answers appeared to be consistent with the premise. Robustness is *not* conveyed by how well results agree (or disagree) with premises: it's based on how well the research was conducted. Jones & Paton's study was conducted very poorly indeed.

Selective weighting of 'evidence'

Jones & Paton state that "Our results provide *strong evidence* that the legalization of PAS is associated with increases in the rate of suicide, if assisted suicides are included", and "the formal regression analysis uncovered *clear evidence* that PAS has been associated with an increase in the overall rate of death by suicide (including assisted suicides)" [emphasis added].

Having already covered the circular nature of this argument, and its biased premise that assisted deaths are the same as general suicides in all relevant ways, the firm claims of "strong evidence" and "clear evidence" are worthy of further examination.

In Jones & Paton's discussion, they argue that legalised assisted dying may furnish general "suicide role models" and "normalise suicide more generally." That is, their stated epidemiological vector for "suicide contagion" from assisted dying was a very general one. If their claimed vector were true, it would be supported by "strong" and "clear" statistical evidence of an increase in *general* suicide rates, and those would be the *primary* results.

However, Jones & Paton found the general suicide rate results "equivocal," that is, significant or not significant depending on which variables they included in the analysis.[‡] They didn't give much weight or attention to this central and major anomaly between their theory and results, choosing instead to cast assisted deaths as equivalent to general suicides and promote that 'result,' for which their econometric model seemed to return a significant statistic (but which in fact was a circular argument).

[‡] Given how poorly Jones & Paton's econometric model was designed and executed, it is hardly surprising that the statistical outcomes for general suicide rates were "equivocal." Nor do they explain why they reported three different sets of results for it, when the "state + year effects + covariates + state time trends" was the appropriate set, and which returned a non-significant result for general suicides.

Jones & Paton turn their badly run study's equivocal results into seeming certainties: "strong evidence" and "clear evidence". These are biased and unscientific conclusions that would have been wholly unjustified by the study even if it were well-run.

In plain language, Jones & Paton argued the logical fallacy that A causes B because their 'experimental results' confirmed it, but only by *adding A to B*.

Overconfidence of interpretation

A general econometric model, even if very well conducted and returning consistently significant results, is unable to prove causation. It's simply not the type of research that can do so. Other more sophisticated and specialised methods are necessary.

Therefore, the most that can be claimed of such an exercise is that it is "interesting." Further research to try and establish causation would be necessary.

Yet despite all this, including Jones & Paton acknowledging that "our results should be treated with some caution," and that the main result (increase in general suicides) did not support their thesis (general suicide contagion), their conclusion, in the Abstract, says, without qualification:

"Legalizing PAS has been associated with an increased rate of total suicides relative to other states and no decrease in non-assisted suicides. This suggests either that PAS does not inhibit (nor acts as an alternative to) nonassisted suicide, or that it acts this way in some individuals but is associated with an increased inclination to suicide in other individuals."

Three specific observations of the conclusion are warranted.

Firstly, "has been associated with" implies a generality of causation unwarranted by the methodology. It would have been better to say "our study found a correlation...";

Secondly, the expression "total suicides" directly equates assisted deaths in all relevant ways with general suicides, when they are substantively different.

Thirdly, Jones & Paton frame the results only in favour of their causative premise while avoiding the obvious alternative explanation against it. Their stated causative epidemiological vector was increased general societal approval of suicide where assisted dying is legal. But they did *not* find a consistent statistical increase of suicides in Oregon and Washington.

The four-barrelled conclusion (with two barrels missing)

And therein lies the four-barrelled bias. Instead, Jones & Paton cleverly reframed the "no rise in general suicide rates" (from the contagion effect) as a "no drop in general suicide rates" (from the substitution effect).

In other words, having found **no reliable statistical relationship between assisted deaths and general suicide rates, which was the primary premise of their contagion argument**, Jones & Paton constructed the following four-barrel **conclusion** (with two barrels conspicuously missing):

- a) They inappropriately added assisted deaths to general suicides to report a rise in "total suicides" (the circular argument);

Jones & Paton's ultimate conclusion is a highly selective four-barrelled affair, in which they neglect to present two of the four barrels: the ones that don't support their theories.

- b) They failed to mention their primary premise of expected *increased* general suicide rates from 'contagion' (a missing barrel);
- c) They reported instead only that there was no *reduction* in general suicide rates expected from 'substitution'; and
- d) They overlooked Occam's Razor — the most simple and obvious conclusion that there was no relationship (a missing barrel).

So, finally, after Jones & Paton claiming that their study found "clear evidence", they sum up in the conclusion of their Abstract that "legalizing PAS has been associated with an increase in total suicides relative to other states and no decrease in non-assisted suicides."

Their expression "has been associated with" grossly overgeneralises the applicability of their findings, in contravention of accepted scientific standards. Rather than conveying an exploratory correlative finding (which in their study was actually a circular argument and where their primary result did *not* support their primary theory), their statement conveys the impression that it is now well-established that "suicide contagion" occurs in jurisdictions with assisted dying laws, complete with a nonsense percentage figure.

In summary, not only did Jones & Paton attempt to test a theory with conflicting result possibilities (general suicide up from contagion theory, but down from substitution theory), they ignored robust evidence to the contrary of their conclusions including in sources they actually cited, attempted to test it using a model that couldn't possibly hope to answer the question in a causative or even quantitative way, constructed the test very badly indeed by failing to control for numerous significant and well-known confounding factors, and then interpreted the results only through the prism of their premise while ignoring the most obvious and generally-accepted interpretation — no significant relationship.

How that passed scientific peer review into publication is a mystery.

Sin 10: Spreading misinformation

Where's the "contagion"?

It's worth noting that in Jones & Paton's entire paper, whose fundamental premise was "suicide contagion from lawful assisted dying", and in which they cited published literature about "suicide contagion," they don't use the expression "suicide contagion" or indeed the word "contagion" once.

Why was that? Was it to create an impression of neutrality, was it because they didn't understand the subject matter, or something else?

Creating fear, uncertainty and doubt (FUD)

Aaron Kheriaty, on the other hand, had no such qualms and has used the term "contagion" liberally to describe Jones & Paton's 'research' results.² He described Jones & Paton's article as a "careful empirical study," demonstrating as little understanding of scientific research principles, the fundamental differences between apparent statistical clusters and actual contagion cohorts (see Appendix A), and rational assisted deaths versus irrational and isolated violent suicides, as them.

Kheriaty's deliberate "contagion" agenda is highlighted by his interpretation of Jones & Paton's results. Like them, he fails to join the primary research dots: that, if their general social suicide-acceptability contagion theory were true, they would have found highly statistically significant rises in the *general* suicide rates of Oregon and Washington: but the study did *not* find this result.

Instead of reporting a negative outcome, Kheriaty opined that "when state-specific trends were included," — which they most certainly should have been — "the power of the tests to pick up the effect as significant was reduced."

That is, when the research returned a negative result to their primary research premise (and overlooking all the other jurisdiction data that contradict it, too), what Kheriaty asserts directly here is that (a) there is an effect, (b) that it should have been picked up by the research, but that (c) the *only* reason it wasn't was that the test 'power' was insufficient.

Rarely will you see a more unjustified and biased interpretation of research results in the professional literature.

Kheriaty continued to promote his misinterpretation of the 'study' to a wide audience, including in a letter to the American Medical Association in opposition to physician-assisted suicide, 16 Feb 2017,⁵¹ in which he stated:

"Of particular interest here is a study published last year by Jones and Paton, who analyzed the suicide data from Oregon and Washington. After controlling for other factors that could account for the rising rates of suicide in these states, their analysis suggested that there is indeed a

Rarely will you see a more unjustified and biased interpretation of research results in the professional literature than Kheriaty's take on the Jones & Paton study.

causal link between the legalization of assisted suicide and an increasing in [sic] overall suicides in both states.” (original emphasis)

Kheriaty’s outrageous and unconscionable misrepresentation of the Jones & Paton study deserves vocal criticism.

That’s an outrageous and unconscionable misrepresentation of the study, which deserves vocal criticism.

Further spread of the FUD

Table 2 presents a summary of the spread of the “assisted dying suicide contagion” theory from Jones & Paton and Kheriaty’s articles, which were published in the *Southern Medical Journal* on 10 Oct 2015.

Table 2: Spread of “assisted dying suicide contagion” from the Jones & Paton and Kheriaty articles

3 rd Oct 2015	Catholic Aaron Kheriaty opinion editorial in the San Jose Mercury News. ⁵²
5 th Oct 2015	In Catholic anti-assisted dying lobby blog Euthanasia Prevention Coalition. ⁵³
10 th Oct 2015	By Michael Cook in Catholic ethics blog BioEdge. ⁵⁴
13 th Oct 2015	By Paul Russell in Catholic blog “HOPE”. ⁵⁵
1 st Nov 2015	In religious ‘Physicians for Life’ blog. ⁵⁶
20 th Nov 2015	Catholic Aaron Kheriaty opinion editorial in the Washington Post. ⁵⁷
2016	United States Conference of Catholic Bishops report saying that “every suicide is tragic”, 2016, ³⁹
29 th Apr 2016	In Catholic blog ‘Crux’, ⁵⁸
Jun 2016	By Victorian Catholic MP James Mulino in his minority report opposing findings of his Parliamentary committee on assisted dying. ⁵⁹
6 th Dec 2016	By St Mary’s Catholic Parish, Bryantown. ⁶⁰
30 th Dec 2016	By the New York State Catholic Conference in a deposition to the New York Court of Appeals. ⁶¹
2017	Catholic Archdiocese of Melbourne, Victoria, Australia, republishing Daniel Mulino’s minority report. ⁶²
16 th Feb 2017	A letter from Aaron Kheriaty to the American Medical Association in opposition to physician-assisted suicide. ⁵¹
19 th May 2017	In an opinion editorial by Catholic Prof. Margaret Somerville ⁶³
22 nd May 2017	Somerville’s editorial reproduced in Catholic blog Mercatornet. ⁶⁴
23 rd May 2017	Somerville’s editorial reproduced in Catholic lobbyist Alex Schadenberg’s blog. ⁶⁵
Sep 2017	By Catholic Oregon lobbyist William Toffler in multiple public addresses around Australia. ⁶⁶

Jones & Paton and Kheriaty’s claims have been promoted largely by Catholic anti-assisted dying lobbyists.

Conclusion

Jones & Paton began with a biased premise which they failed to adequately check in the scientific literature, ignored multiple assisted dying jurisdictions whose data conflicted with their theory, selected a methodology that couldn't hope to resolve the conflicting observations they expected, ran the study poorly by failing to control for multiple major confounding factors, and focused on results which seemed to confirm their causative 'association,' while downplaying those that didn't.

Despite Jones & Paton rightly saying that "several limitations to the analysis in this study suggest our results should be treated with some caution" (yet their 'limitations' discussion omits most of the major flaws reported here), their Conclusions statement is firm: "*Legalizing PAS has been associated with an increased rate of total suicides relative to other states and no decrease in non-assisted suicides.*"

The 'associations' in Jones and Paton's findings were then enthusiastically over-egged in Aaron Kheriaty's glowing editorial pudding of the study. Kheriaty then set about spreading the "contagion" story as though it were proven.

And from there the "contagion" misinformation spread, including by the United States Conference of Catholic Bishops, Catholic blog site Mercatornet, Catholic ethicist Margaret Somerville, and others.

As I wrote in response to Prof. Margaret Somerville's citing of the Jones & Paton study to 'prove' suicide contagion in Oregon:

"It commences with biased premises, sashays its way through deeply flawed methodology (for example, it omits significant suicide risk and protective factors), segues into conclusions which fail to establish assisted dying contagion to the general suicide rate, and then tries to establish a secondary conclusion unjustified by the methodology."⁶⁷

I stand firmly by those remarks, and provide here extensive detail of the grounds on which to do so.

Jones & Paton's article, and Kheriaty's editorial of it, should be retracted.

*Jones & Paton's, and
Kheriaty's, articles
should be retracted.*

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Appendix A: General suicide factors

Suicide risk factors

There are many complex risk factors for suicide, including mental illness,⁶⁸⁻⁷⁰ impaired decision-making particularly in diminished threat sensitivity to adverse outcomes,⁷¹⁻⁷³ potentially heritable personality traits including borderline personality disorder,⁷⁴ neuroticism⁷⁵ and impulsiveness⁷⁶⁻⁸² including impulsive/aggression,^{72,83} drug⁸⁴⁻⁸⁶ and alcohol abuse,^{87,88} domicile in rural or remote areas,⁸⁹⁻⁹² unemployment and economic hardship⁹³⁻¹⁰¹ especially when long-term¹⁰²⁻¹⁰⁶ and related to multiple debts,¹⁰⁷ sexual orientation,¹⁰⁸⁻¹¹⁰ legal problems,^{101,111,112} significant physical illness,^{70,113-115} adverse events such as relationship breakdown including divorce and family stressors,^{68,89,101,116-120} household firearm ownership¹²¹ and other possible factors such as arsenic exposure.¹²²

There can also be many complex interactions between factors, such as bullying and depression,¹²³ sexual minority and family/relationship problems,¹²⁴ bullying and drug use,¹²⁵ multiple-drug use and diminished mental wellness,¹²⁶ drug use and legal problems,¹²⁷ physical and psychiatric illness,¹²⁸ psychiatric illness and unemployment,¹²⁹ low income and ill health,¹³⁰ and declining physical health and divorce.¹³¹ Rural residents are more likely to use a firearm, which is more frequently lethal than other suicide methods.^{132,133}

Suicide risk factors are many, complex, and interrelated.

The US Surgeon General has enumerated key risk factors for suicide:

- Previous suicide attempt or family history
- Mental disorders
- Alcohol and substance abuse
- Hopelessness and isolation
- Impulsive and/or aggressive tendencies
- Barriers to accessing mental health treatment
- Relational, social, work or financial loss
- Physical illness
- Easy access to lethal means, especially firearms
- Stigma of seeking help for mental health
- Belief that suicide is noble
- Suicide contagion¹³⁴

Strong links have been found between unemployment and the suicide rate across UK and Europe,^{94,96,100} Asia^{93,96,97} and American countries.⁹⁶

Larger increases in unemployment have greater impacts on the suicide rate⁹⁴ but may be moderated by appropriate state policy responses, especially a safety net of financial assistance to help meet basic living costs.^{94,95}

Financial hardship and unemployment has a greater effect on suicide rates amongst males^{94,96} and ages younger than 65 years,⁹⁴ particularly of 14-24 year-old males in Europe, and 45-64 year-olds in American countries.⁹⁶

One trend worth noting is the change in the relationship between marriage and suicide rates. Marriage has traditionally been considered a protective factor, and statistically was so in the USA at least until the 1980s.^{135,136} A more recent study in Europe found recent change in marriage status (including getting married) to be a risk factor for suicide,¹³⁷ and being married is a suicide risk factor in Iran.¹³⁸

This study confirms marriage as a risk rather than protective factor at the state level in the USA. This may be due to less stable long-term relationships than in previous decades, since higher new-marriage rates also correlate with higher divorce rates (adjusted $r^2 = 0.31$, $p < 0.001$).

Suicide protective factors

The US Surgeon General notes important factors that help protect against suicide:

- Good clinical care for physical, mental and substance abuse disorders
- Easy access to support to seek help
- Restricted access to highly lethal suicide methods
- Family and community support
- Learned skills in problem solving and non-violent conflict resolution
- Religious or cultural beliefs that discourage suicide.¹³⁴

It can be difficult, but not impossible, to obtain useful quantitative data for some suicide protective factors.

Strong religious belief, but far less so religious attendance, correlates negatively with strong suicide tolerance.¹³⁹ (No significant association *at the state level* was found in this study, although one was found in relation to Black/African American percent of population.)

In the USA, higher educational attainment correlates negatively with suicide rates,^{140,141} as do preventative intervention strategies such as higher proportions of state budget allocations to health and welfare.¹⁴²

USA Airforce suicides significantly reduced with intervention strategies including leadership involvement, role modelling and a community approach to reducing deaths from suicide.¹⁴³

Obtaining direct metrics for protective factors is sometimes more difficult than for risk factors, for example to quantify how 'available' crisis help lines are across the jurisdiction, or how skilled the population is in problem solving.

Nevertheless, some proxy measures do serve as useful surrogates, such as population density to indicate how easy or difficult it is for individuals to access the right kind of care and interventions at the right time, and highest educational attainment as an indication of problem-solving skills.

Suicide clusters versus contagion

Suicide clustering

It can be difficult to reliably demonstrate or rule out that a particular individual's suicide was motivated by another's.¹⁴⁴ p. 188 Consequently, analysis of suicide statistics is used as a proxy measure.¹⁴⁵ Statistics are

assessed for spatial and temporal proximity of attempted or completed suicides, referred to as clusters. Some clusters may occur incidentally via factors such as presence of existing personal risk factors, proximal precipitating events (either independent, or common such as economic downturn), lack of social support, and the tendency for like-minded people to form assortative relationships.¹⁴⁶

Some studies have found no clustering, for example on the Hong Kong railway, the Golden Gate bridge, and in jail.¹⁴⁵ At least one study produced conflicting results, with statistical clustering demonstrated using one choice of analytical parameters, but not with other parameters.¹⁴⁷ Another study examined five apparent clusters of teenage suicides and found three of the clusters were consistent with an epidemic model, while two were not.¹⁴⁸

Suicide contagion

When a suicide cluster occurs, in which subsequent suiciders either personally know the precedent suicider (point clustering¹⁴⁶) or become aware of the suicide through media, online or other exposure (mass clustering¹⁴⁶), then it is possible to impute an infection-like influence mechanism: clustering can *only then* be described as contagion.¹⁴⁹

Suicide contagion is sometimes referred to as the 'Werther effect', after a spate of 18th century suicides said to be inspired by the fictional lead character of Goethe's *The Sorrows of Young Werther*, the copycats dressing similarly and suiciding by pistol at their desk (Werther on the basis of unrequited love), leading to several countries banning the book.^{150, p. 2} A contemporary analysis of *Werther* has found a handful of imitation cases, though no convincing evidence of the claimed widespread epidemic.¹⁵¹

In this copycat suicide contagion it is the *method* of suicide that is duplicated, such as railway suicides in Austria²⁸ and Germany;^{29,30} falls from high places in Switzerland;³¹ carbon monoxide poisoning in the UK,³² Hong Kong,³³ and Korea;³⁴ hydrogen sulphide poisoning in Japan;³⁵ hanging/suffocation in UK prisons;³⁷ and drug overdose in the UK.³⁶ The copycat effect is greater if the initial suicide is of an entertainment or political celebrity.¹⁵²

Suicide 'clusters' are not the same thing as 'suicide contagion.'

Appendix B: Suicide factor bivariate correlations

The following USA state-level bivariate correlation^{ss} analyses include data for matching pairs of years, of significant known potential risk and protective factors for general suicide. The year of analysis differs from pair to pair dependent on data availability, but all years used (2006–2012) include data from authoritative sources that were freely available online prior to the publication of Jones & Paton’s econometric modelling study in 2015.

Risk — low population density (73%)

Residence in rural and remote areas is a well-established risk factor for general suicide.⁸⁹⁻⁹² Low population density not only mitigates against pro-social opportunities, but against access to healthcare services for physical illness, mental health and substance abuse, and to alternative means of employment during economic downturn.

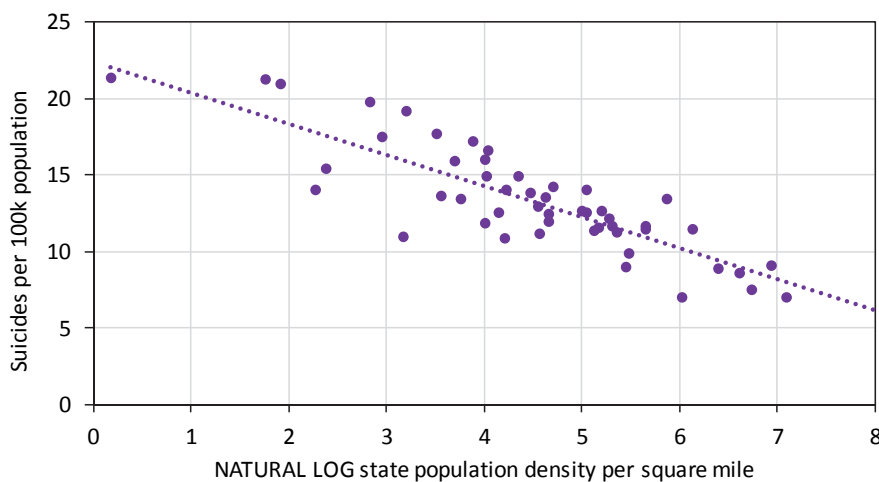


Figure B1: USA state population density (ln) by suicide rate 2010

Sources: US Government census, CDC Wonder

Of all the covariates tested, low population density was by far the most significant (adjusted $r^2 = 0.73$, $p < 0.001$) (Figure B1).

Jones & Paton did not include population density in their model.

Risk — household firearm ownership (48%)

Household firearm ownership is a long-established risk factor for general suicide in the USA.¹²¹ It is on average the most lethal method of attempting suicide: the most likely to result in death.

Of all the covariates tested, household firearm ownership was the second-most significant (adjusted $r^2 = 0.48$, $p < 0.001$) (Figure B2).

Jones & Paton did not include firearm ownership in their model.

^{ss} Multi-variate analysis was beyond the scope of this examination.

Low population density is by far the most outstanding risk factor for suicide in the USA, but Jones & Paton fail to control for it in their study.

Household firearm ownership is well-known as a highly significant risk factor for suicide in the USA, but Jones & Paton fail to consider or control for it in their study.

Higher household firearm ownership also correlates strongly with lower population density (adjusted $r^2 = 0.63$, $p < 0.001$) and with higher divorce rates (adjusted $r^2 = 0.26$, $p < 0.001$).

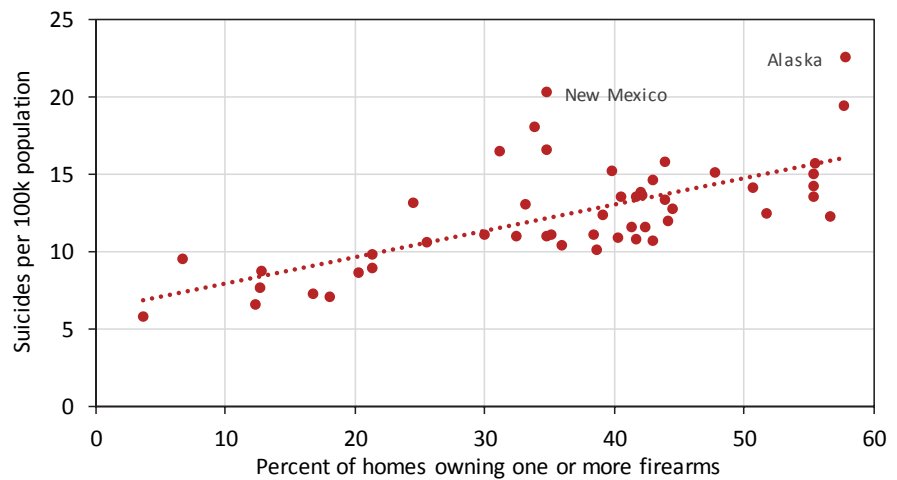


Figure B2: USA state household firearm ownership and suicide rates 2007
Sources: DemographicData.org, CDC Wonder

Risk — divorce (45%)

Divorce is a well-established risk factor for suicide.^{135,153-155}

Divorce is consistently reported throughout the worldwide professional literature as a significant risk factor for suicide, but Jones & Paton fail to consider or control for it in their study.

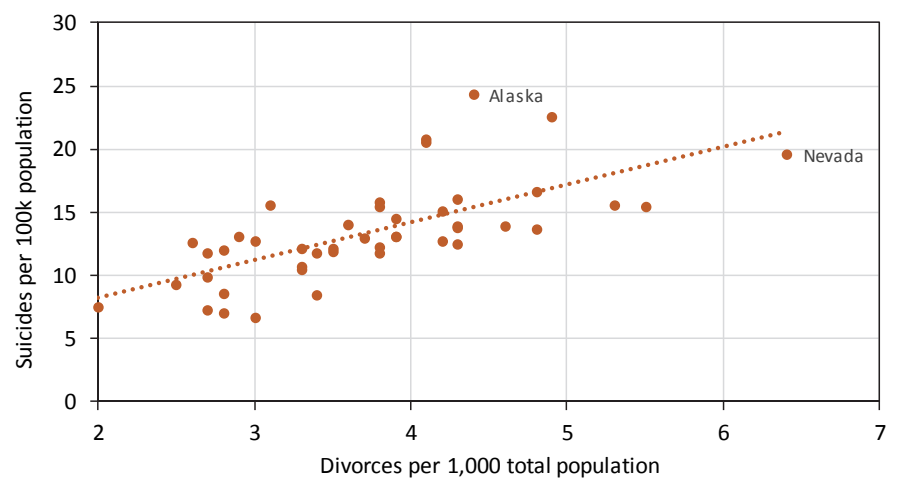


Figure B3: USA state divorce and suicide rates 2008
Sources: CDC NVSS, CDC Wonder. Note: Six states were omitted due to divorce data non-availability

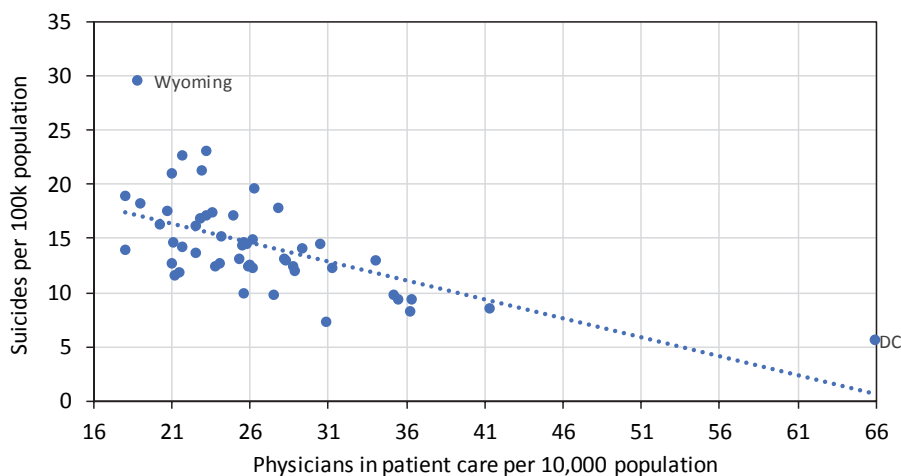
Of all the covariates tested, divorce was the third-most significant (adjusted $r^2 = 0.45$, $p < 0.001$).

Jones & Paton did not include divorce rates in their model.

Higher divorce rates also correlate significantly with higher household firearm ownership (adjusted $r^2 = 0.26$, $p < 0.001$), lower population density (adjusted $r^2 = 0.22$, $p = 0.001$), and somewhat with lower religious affiliation (adjusted $r^2 = 0.08$, $p < 0.05$).

Risk — low physician density (36%)

Serious and chronic physical illness is a well-established risk factor for suicide.^{70,113-115} Physician density, that is, the proportion of physicians per capita population, may be a useful proxy measure of how readily individuals can access appropriate medical care and interventions for these illnesses.



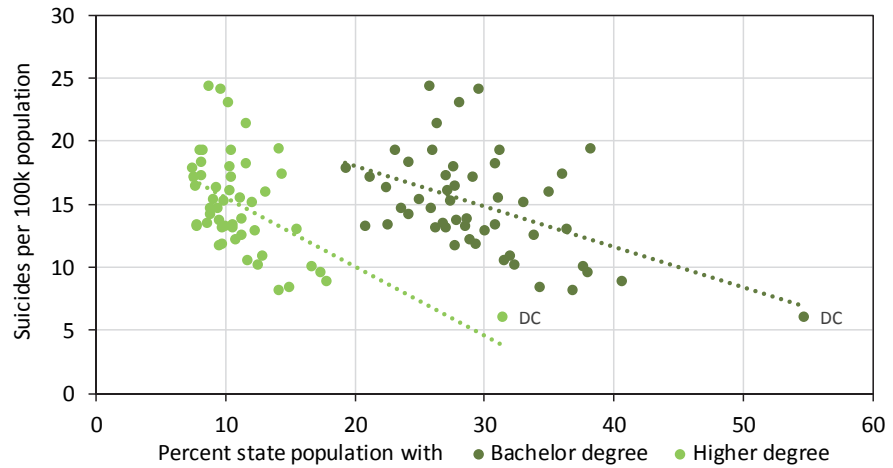


Figure B5: USA states higher education and suicide rates 2011-2015
Sources: US Census Bureau, CDC Wonder

Attainment of higher educational qualifications (results for Bachelors) correlated negatively with firearm ownership (adjusted $r^2 = 0.45$, $p < 0.001$), divorce rate (adjusted $r^2 = 0.27$, $p < 0.001$), lower population density (adjusted $r^2 = 0.27$, $p < 0.001$) and religious affiliation (adjusted $r^2 = 0.23$, $p < 0.001$).

Jones & Paton did not include educational attainment in their model.

Risk — fertility (27%)

Higher fertility rates have traditionally been considered an exemplar of social harmony and a protection against suicide.^{155,158} While in the past a protective association between fertility rates and suicide had been found in the USA,¹⁵⁹ more recently positive associations have been reported.¹⁶⁰ A positive (risk) association has also been found in France.¹⁶¹

This study confirms that at the USA state level, fertility is a risk factor for suicide (Figure B6) (adjusted $r^2 = 0.27$, $p < 0.001$).

The observation may be explained by the rate of abortion in each state (Figure B7), with lower suicide rates at higher abortion rates (adjusted $r^2 = 0.32$, $p < 0.001$). Additionally, abortion rates correlate with fertility rates at a similar level (adjusted $r^2 = 0.33$, $p < 0.001$). This suggests that termination of unwanted pregnancies lowers rates of mental, social, financial and other stresses that may lead to suicide.

Jones & Paton do not include either the fertility rate or the abortion rate in their model.

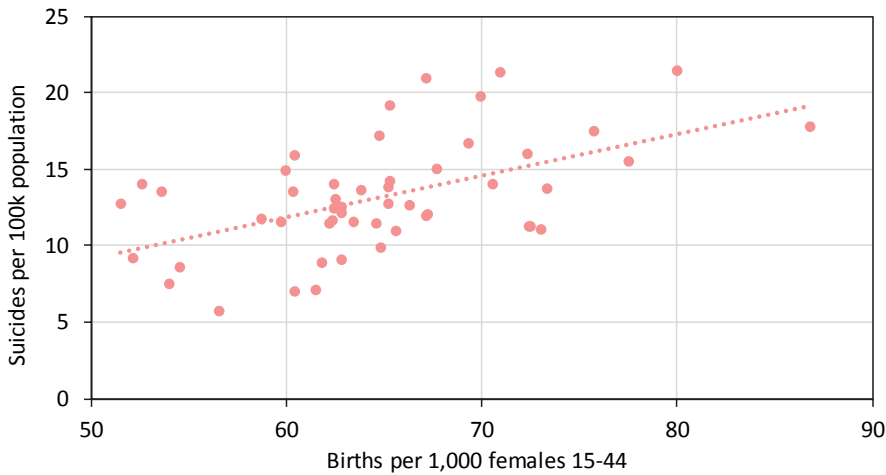


Figure B6: USA states fertility and suicide rates 2010
Sources: Martin, Hamilton *et al* 2012,¹⁶² CDC Wonder

High fertility rates have traditionally been regarded as a protective factor against suicide, though recent USA experience shows it clearly to be a risk factor (because it correlates negatively with higher abortion rates, which are protective). Jones & Paton fail to control for either in their study.

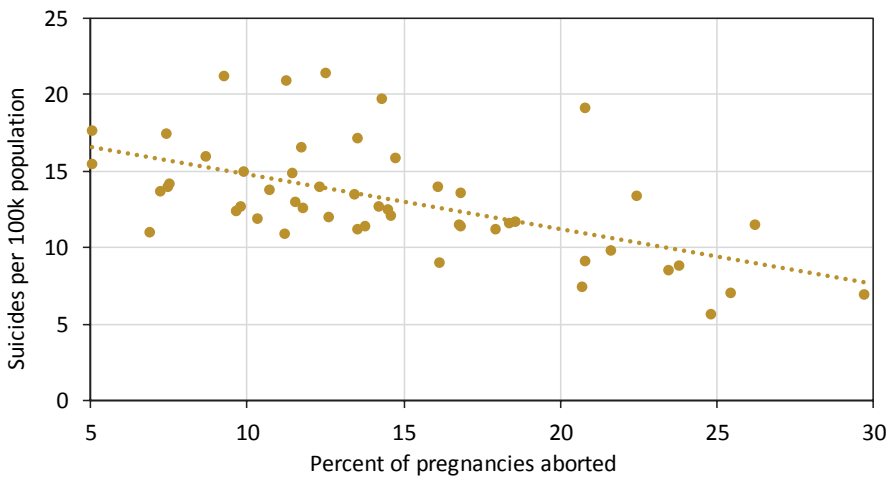


Figure B7: USA states abortion and suicide rates 2010
Sources: Kost 2015,¹⁶³ CDC Wonder

Risk — serious mental illness (24%)

Serious mental illness is a well-established risk factor for suicide.⁶⁸⁻⁷⁰

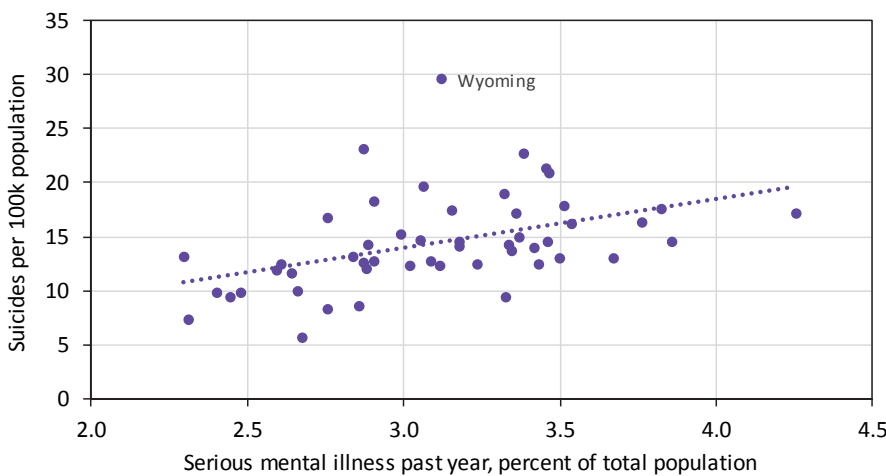


Figure B8: USA state serious mental illness and suicide rates 2010
Sources: US National Survey on Drug Use and Health, CDC Wonder

Serious mental illness is one of the best-known risk factors for suicide, yet Jones & Paton fail to control for it in their study.

A significant correlation exists between state rates of serious mental illness and suicide (adjusted $r^2 = 0.24$, $p = 0.001$) (Figure B9). Significance was slightly lower when statistical outlier Wyoming was included (adjusted $r^2 = 0.18$, $p = 0.001$).

Jones & Paton do not include serious mental illness rates in their model.

Protection/no effect — race (22%, 0%)

Black/African American race has long been documented as a protective factor against suicide, largely on the basis of religious and cultural beliefs and practices.¹⁶⁴⁻¹⁶⁷

Black/African American race is a protective factor against suicide in the USA, and Jones & Paton control for it in their study. However, they also attempt to 'control' for Hispanic/Latino race, which has no statistical correlation with suicide rates.

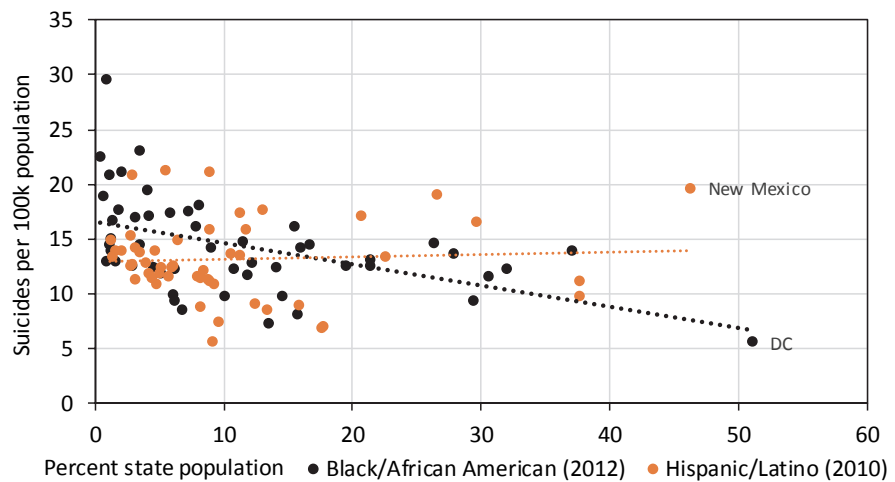


Figure B9: USA state race percentages and suicide rates

Sources: US Census Bureau, CDC Wonder

Higher percentages of Black race correlated with lower state suicide rates (adjusted $r^2 = 0.22$, $p < 0.001$) (Figure B9).

While there was no significant correlation between state proportions of Black and real income, firearm ownership, religious affiliation or divorce rates, two protective factors were significant. The first was population density — higher rate of Blacks in more densely populated states — providing stronger community and easier access to health and social support services (adjusted $r^2 = 0.30$, $p < 0.001$). The second was a stronger measure of religiosity (than religious affiliation), “religion is very important in my life,” with higher religiosity in states with higher proportions of Blacks (adjusted $r^2 = 0.28$, $p < 0.001$). This was unsurprising as Pew Center research has previously found Blacks to have more negative attitudes toward suicide, and Black protestants the most negative.¹⁶⁸

Jones & Paton also include proportions of Hispanic populations in their models, as a factor that has “been found to affect suicide rates.” They provide no citations for this claimed association, nor state whether the expected effect is risk or protective. Moral objections to suicide amongst Hispanics have been found to be similar to whites, while Blacks had significantly higher moral

objection.¹⁶⁹ Suicidal ideation and attempts are restricted to specific subgroups of the Hispanic population¹⁷⁰ and are associated with complex subgroups of religious affiliation and other factors.¹⁷¹

This study found no significant relationship between state proportions of Hispanics/Latino populations and suicide rates, so there was no reason to include this factor in Jones & Paton's model.

Jones & Paton correctly include Black/African American proportions in their model as a significant protective factor against suicide. However, they also include the proportion of Hispanic/Latino state populations in their model, but it has no predictive power.

Risk — rate of new marriages (22%)

According to Durkheim's social integration theory, marriage has traditionally been associated with prosocial protection against suicide,^{136,172} especially amongst males.¹⁷³

This study, however, found that at the *state* level, there was a significant correlation between higher rates of new marriages and higher rates of suicide (adjusted $r^2 = 0.22$, $p < 0.001$) (Figure B10).

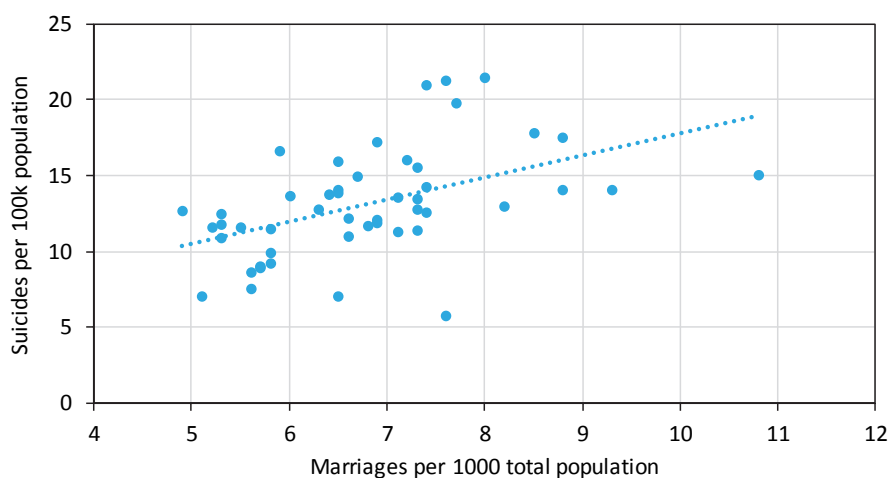


Figure B10: USA state new marriages and suicide rates 2010

Sources: US National Vital Statistics System, CDC Wonder. Note: excludes significant outlier marriage destination states Nevada and Hawaii.

The only other confounding factor found to correlate with new marriages rates was divorce rates: higher new marriage rate states also having high divorce rates (adjusted $r^2 = 0.31$, $p < 0.001$). This suggests that states with high marriage rates have less stable relationships (re-marriage),¹³⁵ with greater chance of stress and suicide.

Jones & Paton do not include marriage rates in their model.

Higher new-marriage rates are a significant risk factor for suicide in the USA, most likely related to re-marriage rather than first marriage (i.e. less stable long-term relationships). Jones & Paton fail to control for it in their study.

Protection — state spending on local welfare (19%)

Higher proportions of state budget allocations to health and welfare is a known protective factor against suicide,¹⁴² and data for state expenditures on welfare is readily available online from US Government Spending.

Higher state welfare spending correlates significantly with lower suicide rates (adjusted $r^2 = 0.16$, $p = 0.002$) (Figure B11). However, suicide rates do *not* correlate at all with *general state-wide* welfare spending (adjusted $r^2 = -0.02$, $p = 0.856$), but strongly with state *local* expenditures, that is, those expenditures targeted carefully to local communities most in need (adjusted $r^2 = 0.19$, $p = 0.001$).

State spending on local welfare programs is a significant protective factor against suicide. Jones & Paton do not control for it in their study.

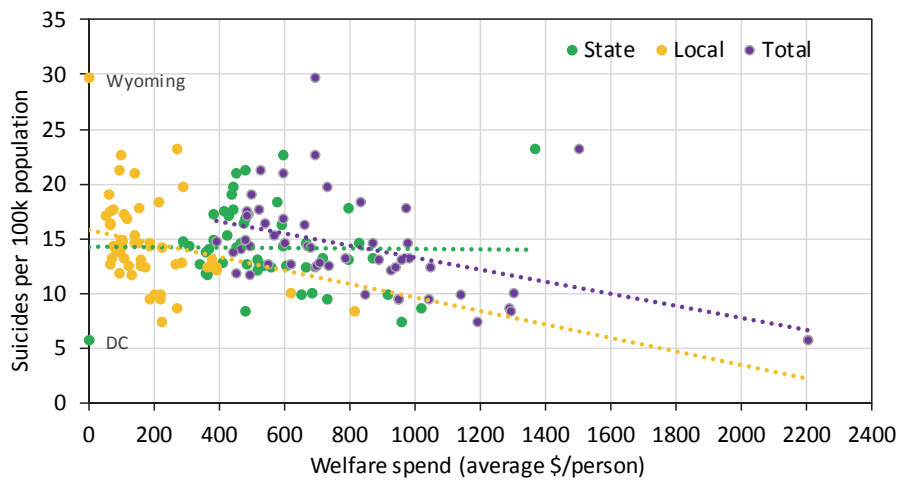


Figure B11: USA state welfare expenditure and suicide rates (2012)

Sources: www.usgovernmentspending.com, CDC Wonder

The local expenditure data also explains why Wyoming’s suicide rate is so much higher than the rest of the nation. Not only does Wyoming have the nation’s second-lowest state population density (after Alaska), the highest rate of household firearm ownership, and the second-lowest physician density — all significant risk factors for suicide — it spends none of its state welfare budget *at the local level* where it would have the most effect.

Jones & Paton do not include state welfare spending in their model.

Risk — married status (15%)

Like marriage rates, married status is generally well-established as a protective factor against suicide, though the relationship is more complex in light of first- versus re-marriage.¹³⁵ A recent Iranian study found a positive association between marital status and suicide.¹³⁸

This study found that at the *state* level, there was a significant correlation between higher rates of marriage and higher rates of suicide (adjusted $r^2 = 0.15$, $p < 0.01$) (Figure B12).

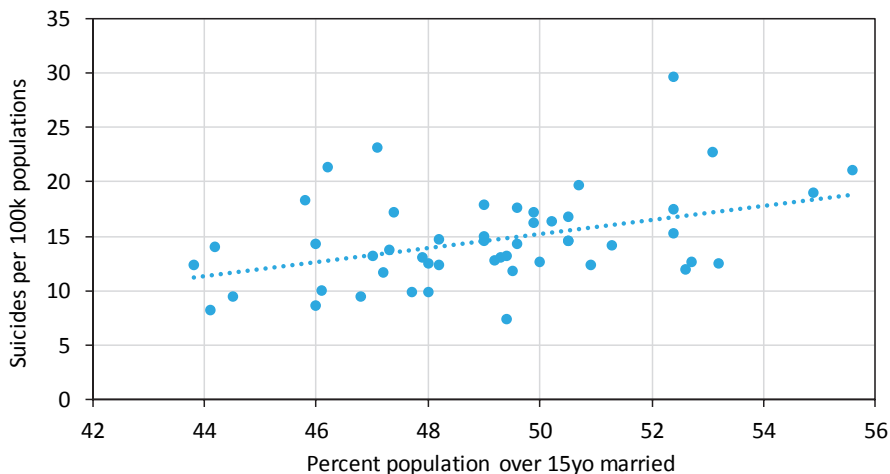


Figure B12: USA state married status and suicide rates 2012

Sources: Lamidi and Payne 2014,¹⁷⁴ CDC Wonder. Note: Excludes significant outlier District of Columbia.

Like marriages rates, married status correlates in the opposite direction from expected. Unlike marriage rates, married status does not correlate with divorce. However, marriage rates correlate negatively with state population density — higher rates of married status at lower population densities (adjusted $r^2 = 0.34$, $p < 0.001$). This suggests that married people in rural and remote areas have lower access to social support and professional help services.

Jones & Paton do not include married status rates in their model.

Protection — per capita personal income (13%)

Higher personal income is well-established as a protective factor against suicide.¹⁷⁵

This study found a modest but statistically significant protective effect of higher personal income against suicide (adjusted $r^2 = 0.13$, $p < 0.01$) (Figure B13).

Jones and Paton do include a measure of per capita personal income in their model.

Marital status is also a significant risk factor for suicides in the USA. Jones & Paton do not control for it in their study.

Higher per capita income is a known protection factor against suicide. Jones & Paton do control for it in their study.

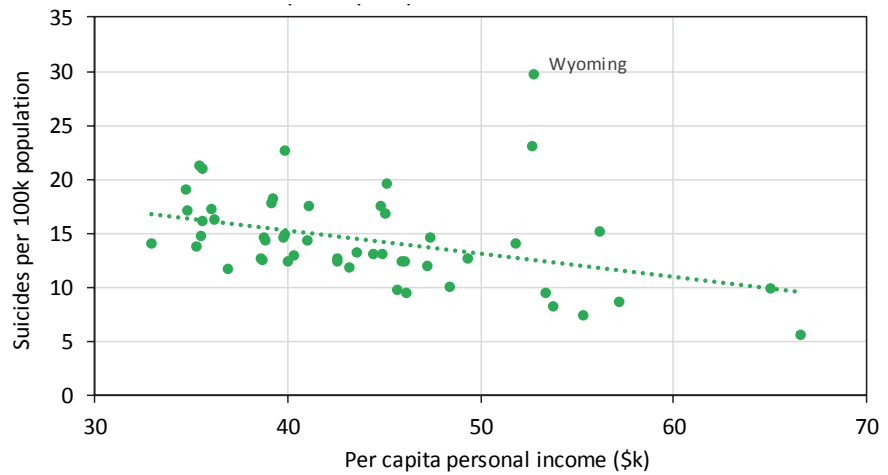


Figure B13: USA state per capita personal income and suicide rates 2012
Sources: US Bureau of Economic Analysis, CDC Wonder

Risk — alcohol consumption (13%)

Alcohol abuse is a well-known risk factor for suicide.^{87,88}

This study found increased consumption of alcohol correlated significantly with increased suicide rates (adjusted $r^2 = 0.13$, $p < 0.01$), but only if three statistical outlier states were excluded, Utah, New Hampshire and the District of Columbia (Figure B14).

Alcohol abuse is a known risk factor for suicide. Jones & Paton bizarrely include a binary measure for 0.08% blood alcohol drink-driving laws in their study, which a source they cited expressly stated was a poor predictor of suicide rates. They did not use readily-available, quantitative alcohol consumption (via purchase) data.

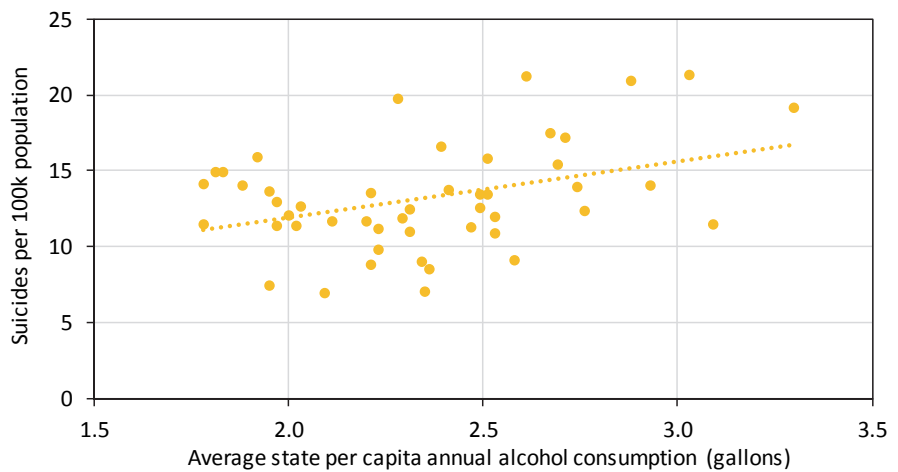


Figure B14: USA state alcohol consumption and suicide rates 2010
Sources:

Utah had by far the lowest alcohol consumption but a relatively high suicide rate. The suicide rate correlates with by far the highest religion rate. If Utah was included, the correlation was still significant but reduced (adjusted $r^2 = 0.06$, $p < 0.05$).

New Hampshire and the DC both have exceptional circumstances, because “alcohol consumption” is measured by the ethanol content of alcoholic beverage *sales* per jurisdiction. New Hampshire has a significant number of state-owned, tax-free alcohol outlets on major interstate highways, so much

in more densely populated states (adjusted $r^2 = 0.23$, $p < 0.001$), suggesting that social support, better familiarity with dosage and other factors may result in a lower lethal rate.

Indeed, the contribution of illicit drugs to suicide rates is complex, because the rate of treatment service admissions in relation to each drug — marijuana, heroin, cocaine, etc — varies widely amongst US states.¹⁷⁸

Jones & Paton do not include any hard drug use rates (including cocaine) in their model.

Risk — illicit use of analgesics (8%)

The USA is experiencing an epidemic of opioid over-prescription for pain management, with concomitant risks for illicit use and suicide. Opioid analgesic deaths now exceed motor vehicle crashes, or deaths from cocaine and heroin combined.¹⁷⁹

This study found a significant correlation between the non-medical (illicit) use of analgesics and suicide (adjusted $r^2 = 0.08$, $p < 0.05$) (Figure B16). If statistical outlier Wyoming was included, the correlation was not significant.

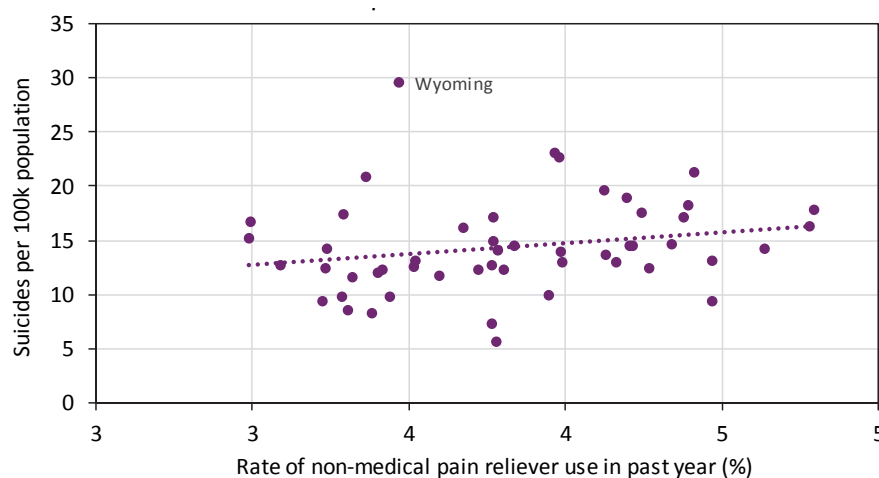


Figure B16: USA state illicit use of pain relievers and suicide 2012

Sources: US National Survey on Drug Use and Health, CDC Wonder

Jones & Paton do not include rates of illicit use of opioid analgesics in their model.

No effect — any religious affiliation (5%/NS)

Religious affiliation or attendance is a poor predictor of suicide rates, though strength of religious belief is a stronger predictor.¹³⁹

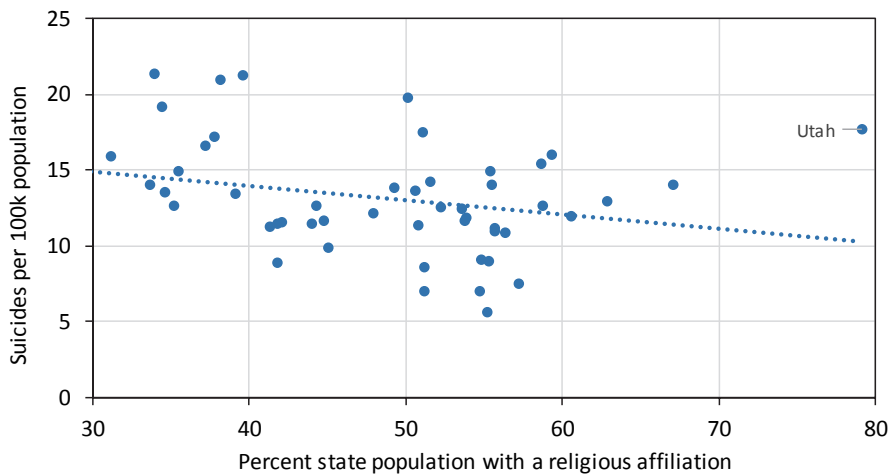


Figure B17: USA state any religious affiliation and suicide rates 2010

Sources: US Religion Census, CDC Wonder

This study found a small negative correlation between religious affiliation and suicide rates, but just beyond significance (adjusted $r^2 = 0.05$, $p = 0.057$) (Figure 17). Excluding Utah, the correlation became significant (adjusted $r^2 = 0.13$, $p < 0.01$). This suggests that at least a proportion of the very high religious affiliation in Utah — largely Latter-Day Saints — is a risk rather than protective suicide factor.

Jones & Paton do include religious affiliation in their model, though its correlation including all states is weak.

No effect — unemployment (4%/NS)

Unemployment and its economic hardship is generally regarded as a significant risk factor for suicide,⁹³⁻¹⁰¹ though the relationship can be complex depending on economic cycles,⁹⁴ with an excess credit supply acting as a protective factor,¹⁸⁰ and state welfare assistance programs. A robust examination of USA state data has previously found that unemployment rates do not have a significant impact on suicide rates.¹⁴²

This study found no statistically significant correlation between unemployment and suicide rates, except if statistical far outlier Wyoming was included, when the unemployment rate became a *protective* factor (adjusted $r^2 = 0.07$, $p < 0.05$) (Figure B18).

There was no significant correlation between unemployment and average personal income, so potential savings from higher former income was not suggested as a protective factor. However, there were two significant protective correlations: higher population density and therefore better access to social and wellbeing support (adjusted $r^2 = 0.20$, $p = 0.001$); and lower ownership of the most lethal suicide device, firearms (adjusted $r^2 = 0.11$, $p < 0.01$).

Religiosity is a known protection factor for suicide (the relationship is complex), but 'any religious affiliation' is a weak measure of it. Correlation with USA state suicide rates is marginal. Jones & Paton did 'control' for it in their study.

Jones & Paton 'control' for unemployment in their study, but unemployment does not correlate significantly with suicide rates at the USA state level.

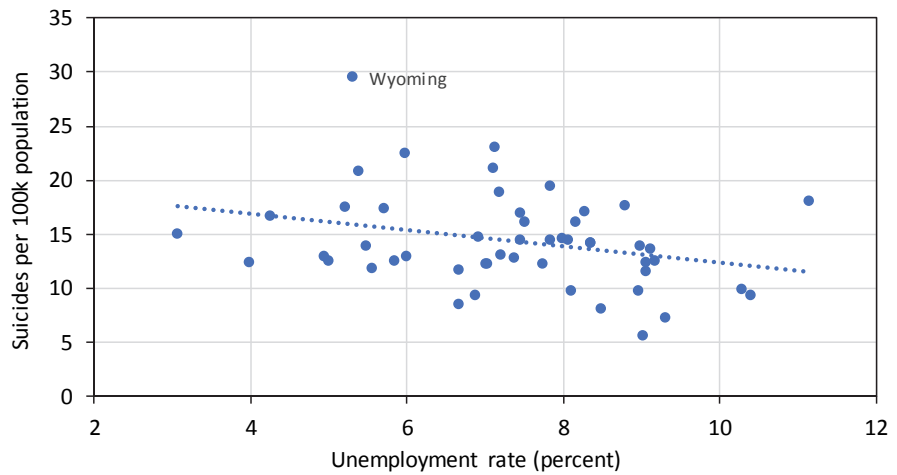


Figure B18: USA state unemployment and suicide rates 2012
Sources: US Bureau of Labor Statistics, CDC Wonder

Jones & Paton include unemployment rates in their model, though the correlation with state suicide rates is weak.

No effect — rate of cancer deaths (1%/NS)

Cancer deaths serve as a useful proxy measure of “terminal illness” in respect of Jones & Paton’s study, since most users of lawful assisted dying worldwide, including in Oregon and Washington, are individuals with advanced cancer.

Jones & Paton argue that suicide rates ought to decrease through *some* individuals substituting a lawful assisted death in place of suicide. They employ a *state-level statistical* method to try and answer the question. In this case it is helpful to establish that there is indeed a *state-level statistical* relationship between terminal illness (for which cancer deaths is a proxy measure) and suicide rates. If a statistical relationship between cancer deaths and suicide rates cannot be detected at the state level, then attempting to detect a statistical drop would be futile.

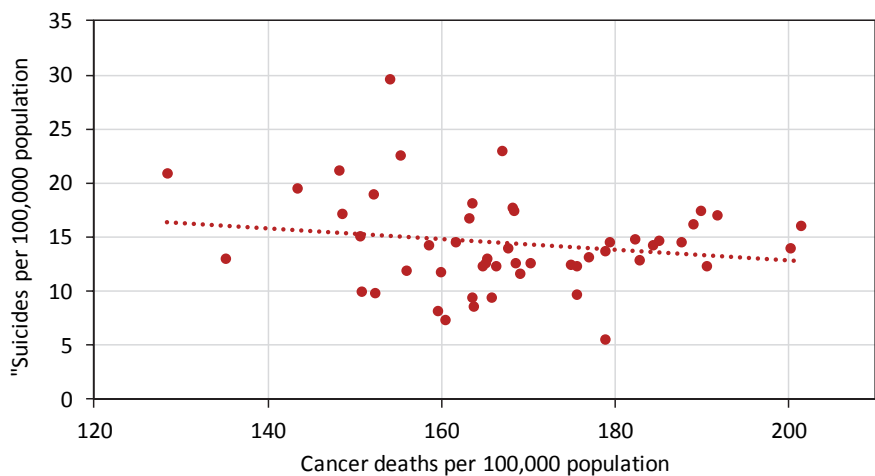


Figure B19: USA state cancer death and suicide rates 2012
Source: CDC Wonder

Jones & Paton attempted to detect a theoretical and tiny possible variance for a variable (terminal illness, proxy measure ‘cancer deaths’) that demonstrated no state-level correlation with suicide in the first place.

This study found no significant covariance between cancer death and suicide rates (adjusted $r^2 = 0.01$, $p = 0.209$) (Figure B19).

Thus, Jones & Paton attempted to detect a theoretical and tiny possible variance for a variable (terminal illness, proxy measure 'cancer deaths') that demonstrated no state-level correlation with suicide in the first place. Their broad statistical modelling therefore was an invalid method to attempt to answer the question.

No effect — average IQ (0%/NS)

It may be argued that higher IQ would be associated with higher problem-solving skills, a known protective factor against suicide.¹³⁴

The average IQ of USA states has been estimated, at least in respect of schoolchildren's standardised reading and mathematical skills.¹⁸¹ There was no significant state-level correlation between child IQ and suicide rates (adjusted $r^2 = -0.01$, $p = 0.558$) (Figure B20).

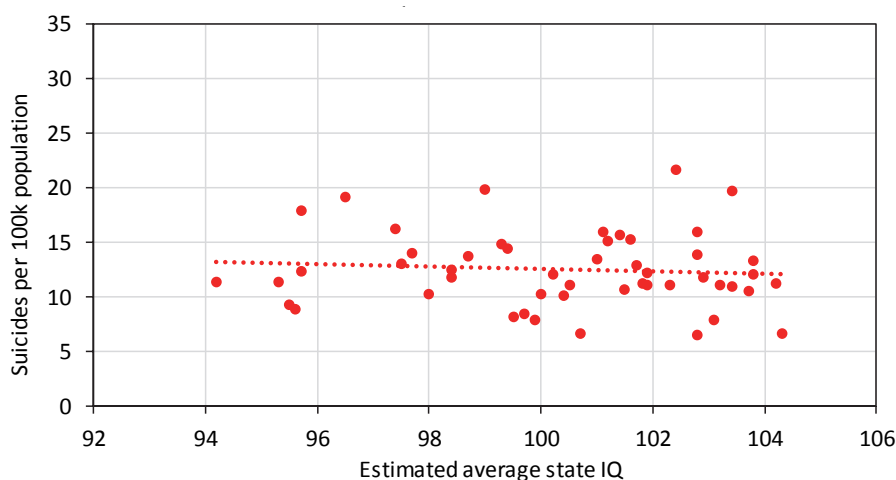


Figure B20: USA estimated child IQ and total suicide rates 2006

Sources: McDaniel 2006,¹⁸¹ CDC Wonder

Higher IQ was modestly related to gross state product (adjusted $r^2 = 0.06$, $p < 0.05$) and government efficiency (adjusted $r^2 = 0.10$, $p < 0.05$), and highly related to higher bachelor degree educational attainment (adjusted $r^2 = 0.26$, $p < 0.001$), lower rates of violent crime (adjusted $r^2 = 0.31$, $p < 0.001$), and especially to better health (adjusted $r^2 = 0.55$, $p < 0.001$).

Given that average IQ did not correlate with suicide rates, but higher educational attainment (bachelor degree) correlated significantly, this may suggest that *learned* rather than innate problem-solving and non-violent conflict resolution skills are protective against suicide, as the US Surgeon General states.¹³⁴

Average IQ might be expected to contribute to superior and non-violent problem-solving skills, but its correlation with suicides was not significant. It appears that learned skills (via higher educational attainment) may account for any superior problem-solving skills in respect of lower suicide rates.

No effect — marijuana use (0%/NS)

Jones & Paton cite a study of medical marijuana laws and general suicide rates⁴² to suggest that legalisation of marijuana for medical purposes may have led to a reduction in suicide among boys and men, justifying its inclusion in their model as a relevant protective factor against suicide. Jones & Paton’s approach was unjustified.

Firstly, at the general population level (the level Jones & Paton investigate in their model) their cited study authors state explicitly that they found no statistically significant association.

Secondly, the anticipated effect was secondary, because legalisation was only in respect of *medical* use under physician prescription, with general availability lowering street prices somewhat. (Jones & Paton also included a flag for decriminalisation of recreational use.)

Thirdly, the study authors note that an explanatory factor for small reductions in suicide rates amongst younger males may have been the result of substantial reductions in alcohol consumption and binge drinking.

And fourthly, in the USA in 2009, a third (34%) of emergency department admissions in relation to illicit drug use were for marijuana, and therefore would indicate its use as a risk rather than protective factor.⁴³

Jones & Paton overlook these important details.

Jones & Paton considered marijuana legalisation a protective factor against suicide, while overlooking its substantial contribution to emergency department admissions. They ‘controlled’ for it via a simple flag of whether marijuana was legalised in each state, rather than using readily-available semi-quantitative data of its usage rates. In any case, its use was not correlated with suicide rates at the state level.

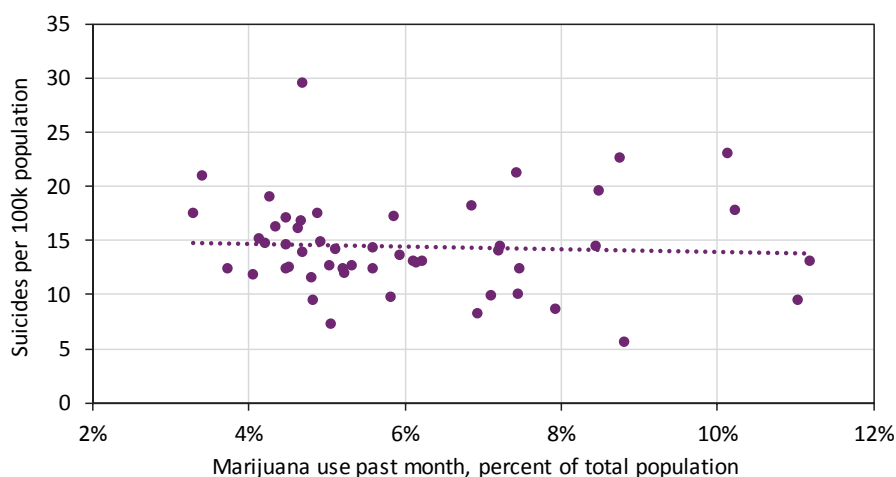


Figure B21: USA state marijuana use in the past month and suicide rates 2012
Sources: US National Survey on Drug Use and Health, CDC Wonder

No significant correlation was found between recent marijuana usage and suicide rates (Figure B21).

Jones & Paton include a binary flag of marijuana legalisation (both medical and recreational) by state and year in their model, when semi-quantitative data for marijuana use indicates no correlation.

Appendix C: Who are Jones, Paton and Kheriaty?

David A. Jones is Director of the UK [Anscombe Bioethics Centre](#) (formerly the Linacre Centre). The Centre is a self-proclaimed “Roman Catholic academic institute” whose stated purpose is to “bring to bear those questions principles of natural law, virtue ethics, and the teaching of the Catholic Church...” It is not affiliated with any higher education institution.

David Paton is [Professor of Industrial Economics](#) at Nottingham University Business School and a visiting Professor at St Mary’s [Catholic] University, Twickenham. He’s a regular contributor to the [Catholic Herald](#), as author of “[No one helps the poor more than the \[Catholic\] Church](#)” as well as pieces [opposed to sex education in schools](#), taking the Catholic Church as the authority on the matter and [suggesting Catholic Schools take extra care](#), and [railing against anti-Catholic myths](#).

Aaron Kheriaty is Associate Professor of Psychiatry at the University of California Irvine School of Medicine. He earned his degree from (Catholic) Georgetown University and is author of [A Catholic Guide to Depression, Mental Health and the \[Catholic\] Church](#), [A Catholic Guide to Mental Illness](#), [Treating depression with psychiatry and faith](#), and [The Perils of Assisted Suicide](#), amongst others.

David Jones, David Paton and Aaron Kheriaty are all deeply inured Catholics. Coincidentally, nothing appearing in their articles was at odds with the Catholic Church’s views about and entrenched opposition to assisted dying law reform.

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Rocking the vote for law reform