

Dear Dr. Throckmorton,

May 2, 2007

Thank you for your response to our rebuttals. I appreciate your willingness to dialogue over these matters and to post the discussion on your blogsite. I am sorry that you are still fundamentally skeptical of our research and that your lack of confidence in us has not changed. Perhaps I can offer some reasons for you to rethink your position.

Concerning our poster presentation at the EPA convention and the paper we wrote, it certainly would have raised fewer questions after the fact had we revised our submitted abstract to reflect our findings on estimated longevity. Like many researchers, we typically have concurrent streams of work ongoing at any given time. I was revising for submittal a much longer article on the lifespan when we realized that those results dovetailed well with the apparent decrease in homosexual prevalence at older ages. That was the reason for its inclusion in the EPA poster and paper and not some sinister plot to 'sneak' something by the EPA (after all, as my father noted, he — either singly or jointly with me — has presented scientific results on homosexuality several times to that body, including results from our very first gay obituary study).

Related to this, our separate article on the homosexual lifespan is currently under scholarly review, so it would be inappropriate at this time to send you our Danish and Norwegian data. When the article has been accepted for publication, we may be able to oblige your request. But in answer to your question, we did not purchase the Danish and Norwegian datasets solely for the EPA presentation. Also, Dr. Frisch is correct that neither Statistics Denmark or Statistics Norway publishes this information on their websites. We had to make and pay for a specific research request to obtain it.

While I can't send you our data, I can tell you exactly what they consist of. We requested and received from Statistics Denmark and Statistics Norway a series of Excel files structured almost identically and containing the following tables: for each available year, a count of the total number of deaths that occurred during that calendar year, cross-classified by sex, age at death (in one-year increments), and marital status at death (including categories for registered partners, dissolved [i.e., 'divorced'] partners, and surviving [i.e., 'widowed'] partners). It was from these tables that we constructed estimates of longevity using standard life table techniques. Altogether, we utilized more than half a million deaths in our analysis (obviously, the vast majority of these were of individuals who did not have a registered partnership status at time of death).

As a side note, I believe you may have misconstrued our qualification concerning the marital status cohorts. You summarize by saying: "You essentially say, we lumped people from different cohorts together and had no way of determining their actual marital status." Actually, we know precisely what marital status each individual had at time of death (at least to the extent that the Danish and Norwegian population registries are accurate). What we don't know is how many of the homosexually partnered at time of death had previously been married at an earlier point in life. That is rather different from your apparent interpretation.

An Overview

I will address your specific concerns about our methodology, but an overview is appropriate first. You are appreciative of the fact that we noted several uncertainties regarding our data and conclusions in our EPA paper, as any scientist is obliged to do, yet you wonder why we even proceeded with the analysis at all! And your skepticism is strengthened, it seems, by the fact that we put out multiple news releases on the results even though our methods were, in your opinion, so questionable and uncertain (in your words “fatal... limitations”).

As I will explain, you have apparently misread or misunderstood aspects of our methodology. Further, the ‘whole story’ about our research is not fully contained in the EPA paper, but rather in a series of separate, but related articles, each addressing a slightly different topic. Be that as it may, I do find it a bit of a double standard that you would implicitly criticize our use of the media and internet as a forum for dissemination of new information, when your blogsite is not, as far as I can tell, subject to any scholarly oversight (beside your own). As you know (perhaps even from personal experience), getting research published in psychological and social science journals that is critical of homosexual practice is extremely difficult, no matter how well done and no matter how scholarly the work. Political correctness rules with no more an ‘iron fist’ than in this particular arena. Yet we are convinced that our research must be disseminated, one way or another. You may not agree with our position or with the conclusions we have derived from our research, but I would hope you would agree that debate on this topic should be encouraged, not stifled, as it clearly has been in our case (there are even internet posts from gay activists who claim they have tried to lobby specific journal editors not to publish our material).

We continue to submit scholarly work to a variety of journals. We also post a variety of materials on our website (www.familyresearchinst.org). And we occasionally attempt to get media attention to our findings. We welcome legitimate debate about our findings, our methodology, or anything else of an empirical nature.

Life Tables and Obituaries

As to your specific concerns, why did we bother to present these findings at all? In a nutshell, limitations and uncertainty do not equal falsehood. All our data was fairly and impartially gathered or obtained. Our use of that data has been clearly documented and the assumptions laid out. Yes, our estimates of homosexual longevity are preliminary and may change with additional data. But are they necessarily false or unreliable? No.

Consider these facts. First, you quote our statement about the “state of flux” since the adoption of homosexual partnerships in Denmark and Norway and cite our caveat about the small number of deaths among homosexual partners in any given year. This indeed would be a fatal limitation if we were trying to construct a *current* life table, a type of table built from the data of a single year. However, it was for that reason that we amalgamated the deaths over several years and constructed a *cohort* life table, in order to harness the tremendous power of statistical averaging.

A fascinating aspect of statistics is the ability to make important and fairly accurate statements even with relatively small samples. Case in point: many polls (e.g., Gallup, Harris, etc.) often get an accurate read on national opinions through the sampling of perhaps 1,500 individuals out of a population of more than 200 million adults. Over the years, of course, emphasis has been placed on choosing those individuals in the right way, through the use of random sampling, etc. What's even more interesting is that sometimes the 'correct answer' is obtained even with a less than ideal sample. The proof is always in the 'empirical pudding,' and not strictly on what one surmises about a particular methodology.

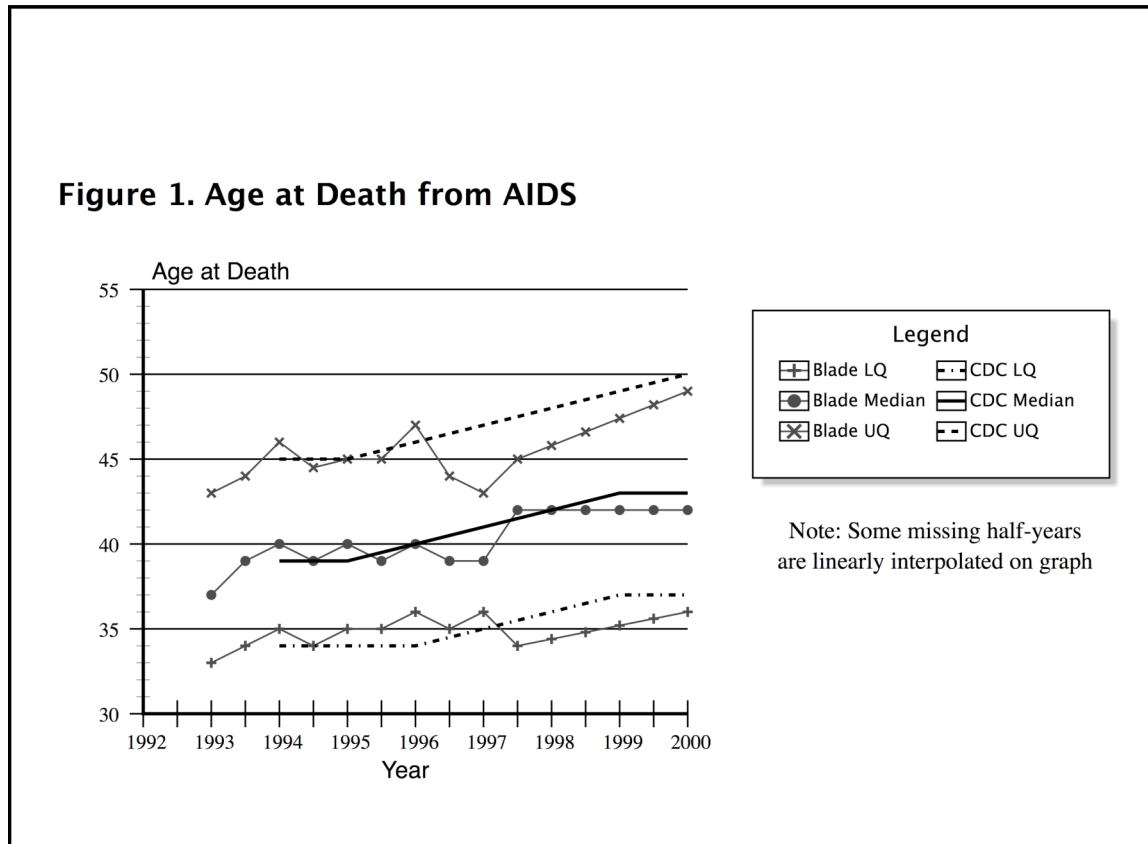
Such is true in this case. We did not simply claim that our estimates of longevity were reliable because the cohort life table methodology was developed and published by prominent statisticians and demographers (which it was). Because our particular use of it was non-standard, we did empirical tests of its accuracy. Obviously, no benchmark of the homosexual lifespan was readily available. But other benchmarks were, specifically, the official life tables of Denmark, Norway, and the U.S. Against these life tables, the cohort method proved remarkably accurate. Further, in specific response to your concern about the trustworthiness of a life table based on only a few hundred deaths, we noted that the cohort life table based on a few hundred consecutive obituaries from the *Washington Post* over a several year period matched to within 1-3 years the officially published U.S. life tables for both men and women.

We also noted how remarkable is this last result, given that *Post* obituaries are, in fact: 1) newspaper-reported obituaries and not deaths from the National Center for Health Statistics; 2) only representative, if at all, of the Washington, DC area and not the nation as a whole; and 3) only represent at best a fraction of the deaths that occurred in the DC area during the time period of collection. Given the results of these empirical tests, it was neither nonsensical nor imprudent for us to assert that the same method might generate reasonable estimates for the set of deaths in homosexual partnerships. Indeed, in my experience, this is a clear example of the power of statistical averaging at work.

And not an isolated example, either. You claim that "I am skeptical because you continue to defend the integrity of your obituary study of gay life expectancy." And "About the obituary sampling, however, it stretches the imagination to think that obituaries published in any news outlet could be considered a random sample. It is hard to imagine a more skewed sampling approach." I would understand your skepticism were it obvious that estimates of vital statistics compiled from the *Washington Blade* 'missed the mark.' But apparently you have not seen our empirical test of the obituary estimates, "Gay obituaries closely track officially reported deaths from AIDS" (Cameron and Cameron, 2005, *Psychological Reports*, 96: 693-697).

Against the benchmark of nationwide reports of AIDS deaths among MSM compiled by the CDC from 1994 to 2000, obituary estimates from the *Washington Blade* regarding median age of death and the lower and upper quartiles of this distribution were generally within 1-2 years of the CDC figures. Again, given the tremendous criticism we have received by those asserting that obituaries are so skewed as to be 'useless' in estimating homosexual longevity, this is a rather remarkable result. In fact, we also showed that the CDC-documented rise in longevity among those dying of AIDS, presumably due to new

drug treatments and/or lower rates of HIV infection, was also paralleled by the obituaries, even though yearly Ns from the *Blade* ranged from a meager 81 to 277. (see figure below)



We concluded that article in this fashion:

“If obituaries in the gay press, at least those carried by the *Washington Blade*, so closely track what is known about deaths due to AIDS among MSM, it may strengthen the case that such obituaries also track deaths among MSM from other causes. However, no publicly accessible evidence is available for an empirical test of this notion. Regardless, for males who have sex with males with AIDS, the overall finding of previous research utilizing obituaries and other indirect lines of evidence — of a 20- to 30-yr. decrement in the average lifespan of homosexuals compared with nonhomosexuals — appears to be at least partially confirmed.”

The Great Unknown

We don't take empirical data lightly, nor do we handle it carelessly. Whether you find our research methods unconventional or perhaps not what you were taught, the proof is — at the risk of repeating myself — in the empirical pudding and not what ‘theoretically makes sense.’ This same idea relates to your criticism of our conclusions about the

Canadian study data on homosexual prevalence. You noted, as we did in our paper, the uptick with age in the fraction of those who either did not answer the question on sexual orientation or said “don’t know.” You ask “isn’t it negligence to avoid an explanation for the striking shift in the Unknown column?”

Indeed it would be negligence if we had not, in fact, discussed that very issue on pages 12-13 of our EPA manuscript. There we cited the possibility that the estimates on homosexual prevalence could easily be different from those reported either by us or Statistics Canada if in fact a substantial fraction of the ‘unknowns’ were intentionally concealing their homosexual interests. However, we also offered a plausible alternative to explain the ‘unknown’ fraction, one based on empirical data and the fact that the question used by Statistics Canada forced respondents to choose only among ‘homosexual,’ ‘bisexual,’ and ‘heterosexual.’ In our U.S. sex survey from the 1980s, we offered an additional response not proffered by Statistics Canada: ‘asexual, not really sexually interested.’ A large minority of older adults chose this answer, much more so than did the younger respondents (see Table 3 from our EPA paper reproduced below).

Table 3. Sexual Desires in U.S. Urban Areas: 1983-84 (in %)

| Age | N | Homo/Bi (Male) | Asexual (M) | Homo/Bi (Female) | Asexual (F) |
|-------|-------|----------------|-------------|------------------|-------------|
| 18-29 | 1,809 | 8.0 | 1.3 | 2.9 | 1.4 |
| 30-39 | 1,276 | 9.0 | 1.1 | 2.0 | 1.7 |
| 40-49 | 652 | 7.5 | 0.8 | 1.7 | 4.4 |
| 50-59 | 513 | 3.0 | 1.3 | 0.7 | 12.8 |
| 60-69 | 412 | 1.8 | 7.1 | 0.4 | 30.6 |
| 70-79 | 154 | 2.6 | 15.0 | 1.4 | 40.5 |
| 80+ | 29 | — | 30.0 | — | 57.9 |
| All | 4,845 | 6.9 | 2.5 | 2.0 | 6.8 |

Does this ‘prove’ one way or the other that older adults were not trying to ‘hide’ their sexual proclivities from interviewers? No, but from both our experience and that of other research teams (many of them government-funded), it seems likelier to explain the results. Frankly, I agree with your speculation that someone trying to hide their homosexual leanings would be more likely to choose the socially safer response of ‘heterosexual,’ rather than to refuse to answer the question altogether or to say ‘don’t know’ when that would leave a possible suspicion. However, it is clear that those homosexually-inclined are more, not less apt, to volunteer for sex questionnaires. We also certainly agree that sexual orientation seems to be a ‘fluid’ phenomenon at least for some over the course of one’s life. In fact, we were the first researchers to note and highlight the data on changes in self-reported Kinsey scale estimates that were ‘buried’ in the *Statistical Appendix* volume of the 1970 Kinsey Institute report and never discussed by any of the original authors.

Unfortunately, your criticism of our work also ignores the fact that if the Canadian study — by far the largest study ever to include questions on sex — is unreliable because of refusals, lying, or unknowns, so is every other sex survey ever conducted. Statistics Canada, in the tabulation it prepared for us, did not compute estimates for the unknown column. We documented that facet of the study results and determined from the

codebook what responses were counted as ‘unknown.’ Plus, there is the issue of non-respondents. For the Canadian study this was relatively low — around 20% — but clearly still large enough to dramatically change the prevalence estimates were non-response correlated with a concealed homosexual orientation. This did not prevent Statistics Canada from asserting publicly that only 1.7% of the Canadian population was bisexual or homosexual. Were they professionally negligent in doing so? And what about the research teams from Great Britain, France, and the U.S. that have also reported low estimates of homosexual prevalence despite even larger refusal rates? Are you also criticizing them in the same vein, or is it only us in whom you have no confidence?

Ad Hominem Logic

My overriding concern here is that because you disagree with our public statements summarizing our findings, since in your view they ‘overstate’ our case and are not adequately tempered with qualifications, that our results or methodology really can’t be trusted. Needless to say, Dr. Frisch agrees with your assessment, seeing as he quotes from the mission statement on our website (www.familyresearchinst.org) to argue that “any report on human sexuality originating from this institution will by definition be devoid of objectivity and of questionable scientific value.” And yet, the truth of the matter is that when I was in high school, my (naïve) opinion was that gay rights was merely the next wave of civil rights. Blacks had been unfairly and prejudicially treated, and so, I thought, had homosexuals. It was only when I began to examine the empirical evidence in detail that I came to see the large number of correlations between homosexual practice and unhealthy and/or dangerous behavior. There was an empirical, scientific case to be made for why homosexuality should not be encouraged or endorsed by our culture. That is the reason for our mission statement — not because we desire to fit the data to our preconceived beliefs, but instead because that is the conclusion to which the data have so far led.

Furthermore, despite Dr. Frisch’s assertions to the contrary, I have yet to meet any researcher in any field with any length of experience who is merely a ‘disinterested observer.’ Humans simply don’t study things about which they hold no opinions or in which they have no specific interests or objectives (see Press and Tanur [2001] *The Subjectivity of Scientists and the Bayesian Approach*, for instance, to get a fascinating glimpse at several well-known historic scientific figures). Some scientists are publicly more quiet about their beliefs than others, but that doesn’t make them ‘objective.’ Nor is it a simple dichotomy of scientists on one side and activists on the other. All of us have to weigh our own expectations about how a study or experiment will turn out against the actual empirical results. In my view, a ‘reasonable’ scientist is one who is willing to consider the data and arguments put forth by those opposed to him or her without having to resort to name calling or attacks on their character. An ‘objective’ scientist is one that is willing to report data contrary to his or her notions of the ‘way things are’ and to alter their conclusions if need be in order to accurately describe the empirical reality. We have done so in the past (e.g., on the lack of measurable health consequences of abortion) and will continue to do so in the future.

I also note that despite Dr. Frisch’s protestations of his ‘lack of an agenda’ regarding gay rights, his first review explicitly noted his concern that our work would further

‘stigmatize gays and lesbians.’ You had no criticism of this obvious statement of belief on his part. Nor have you criticized Dr. Frisch for excerpting our organization’s mission statement in his critique of our research methods (see below). Are we the only scientists with stated beliefs? The *Journal of Homosexuality* is described in Wikipedia as a “highly respected forum for research into same-sex desire” and yet among its stated aims are:

“In addition to being a vehicle to bring together scholarly research on homosexuality and *to support the growing number of lesbian and gay studies programs*, the journal aims *‘to confront homophobia through the encouragement of scholarly inquiry and the dissemination of sound research.’* The contributors are professionals with *an open and positive outlook* toward sexual variations.”

Are all studies from this journal therefore inherently biased and methodologically flawed? Using your and Dr. Frisch’s logic, the answer would seem to be yes. Indeed, with rare exceptions, only authors who are openly gay publish there, so we are talking about an ‘advocacy journal’ — not quite a ‘scientific journal.’ What about the letter to the editor you excerpted approvingly from Hogg, et al. (2001, *International Journal of Epidemiology*, 30: 1499)? Why didn’t you note their stated belief that “These homophobic groups appear more interested in restricting the human rights of gay and bisexuals rather than promoting their health and well-being”? Or “we do not condone the use of our research in a manner that restricts the political or human rights of gay and bisexual men or any other group.” Do these statements not arouse your suspicion of a possible agenda when they assert that “life expectancy... cannot be attributed solely to their sexual orientation or any other ethnic or social factor...”? Are you not troubled by their assertion that “If estimates of an individual gay and bisexual man’s risk of death is truly needed for legal or other purposes, then people making these estimates should use the same actuarial tables that are used for all other males in that population,” when 1) they have provided no specific data to support this claim, and 2) their earlier article specifically and explicitly *assumes* in its methodology (without buttressing) that the only difference in mortality risk between homosexuals and non-homosexuals is due to HIV/AIDS?

You seem much like the kettle calling the pot black. Perhaps your agreement with Dr. Frisch and Hogg, et al. justifies your giving them a ‘pass’ but not us. For the record, our professional view is different: every study stands or falls on its own merits, no matter who the researcher or what their ideological stance. It does not appear that you have taken the same tack.

You are clearly correct that homosexual practitioners as a group are not ‘monolithic.’ But neither are smokers, drug users, prostitutes, drunk drivers, etc. Our society does not base public policy on individual differences, but rather on identifiable and consistent statistical tendencies associated with particular behaviors. It is an interesting fact that perhaps 10% of all smokers seem to accrue health and longevity benefits *because of* their smoking habit. Yet should we cease to discriminate against smoking because of that minority? I, and most others, would say the dangers to the majority of smokers outweigh the benefits to the few. The same logic applies to regulation of homosexual behavior.

Dr. Frisch Redux

As to Dr. Frisch's response from April 24th, I find it interesting but unfortunate that his first line of defense is an attempt at character assassination: because we have expressed a belief that homosexuality appears to be injurious to its participants and to society, we therefore by default cannot be 'objective' or 'scientific.' I'm afraid I simply disagree. The heart of his response is more interesting, because he acknowledges the possibility that a longevity differential associated with sexual preference might be a legitimate scientific question. Also, his argument that we don't yet have enough data or experience with homosexual partnerships to say one way or another is a reasonable question, one that we have tried to answer in our full write-up of the longevity study.

Nevertheless, Dr. Frisch's argument is somewhat lacking in logic. He repeats his contention that, to paraphrase you, the 'data stream' on homosexual partnerships is too 'immature' to be of any use in estimating life expectancy. And he sets up a hypothetical scenario to suggest how we could have found lower life expectancy estimates for newlyweds as compared to newly-partnered individuals, all to demonstrate why our research is a "humorous example of agenda-driven, pseudo-scientific gobbledegook."

What Dr. Frisch ignores or does not grasp is the following:

1) he does not dispute the fact that individuals who register homosexual partnerships tend to be significantly older than heterosexual newlyweds; in fact, he uses that bit of evidence to set up his hypothetical. In turn, however, what this means is that the homosexual 'data stream' is not so 'immature' after all. Indeed, as of 2006, 10% of all the registered male partners in Denmark were aged 65+ (oldest = 92) and 10% of all the registered female partners were aged 60+ (oldest = 92). This despite the fact that the partnership registry was only begun in 1989. It is therefore incorrect, and somewhat ingenuous, to say that all the deaths we observed among the partnered were necessarily young. Or that it is obviously the case that the current age distribution of those in partnerships is far younger than the 'true' (i.e., stable, long-term) age distribution.

2) he criticizes me specifically for failing to understand "the inferential problems that prevail when comparing the average age at death in two study groups with vastly different age distributions. Elementary textbooks in epidemiology warn against such undue comparisons because they lead to apparently common-sense, but overtly wrong, conclusions." But suppose, strictly for the sake of argument, that homosexuals do tend to die about 20 years sooner than non-homosexuals. What then would the 'elementary textbooks' say? By Dr. Frisch's logic, the age distributions of homosexuals vs. non-homosexuals would never become comparable — due to the higher proportion of early deaths among the former — and thus one would *never* be justified in reporting a differential in life expectancy!

My point is that we are not dealing here with an 'elementary' kind of comparison, nor does our analysis fit within the 'standard' epidemiological framework. Only time will tell, of course, whether the age distribution of partnered homosexuals 'catches up' with that of the ever-married or with males and females in general in Denmark and Norway. If it does, we will stand corrected. What we do know at this time is that while the total

number of registered partners has increased more than ten-fold since the inception of legal partnerships, the age distribution has shifted upward only modestly since the early 1990s and by about the same amount as the aging of the ever-married. Further, surveys over the past 60 years from across the Western world — both random and non-random — have found a similar paucity of older homosexuals, and quite independently of the sympathies of the researchers.

As we have stated in another submitted article specifically geared to the Canadian study:

“the apparent drop in homosexual prevalence with age is suggestive of three possible mechanisms: 1) an increased propensity for older individuals to ‘hide’ their non-heterosexual impulses from researchers; 2) a decrease in the relative proportion of non-heterosexuals among older adults, due either to a) a shorter lifespan, and/or b) changes in sexual preference away from homosexuality and bisexuality.”

While the first possibility may seem the ‘obvious’ answer to some, no systematic empirical evidence has yet been put forward to support it. By contrast, we have assembled evidence supportive of both the latter mechanisms. Both may indeed be at play. Time will tell. Perhaps at that point our research will not seem so ‘amusing’ to future epidemiology students after all.

Sincerely,

Kirk Cameron, Ph.D.
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Family Research Institute