

Sociological Studies Show Social Factors Produce Adult SSA

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Abstract

An important path analysis study by Bell, Weinberg, and Hammersmith (1981a) is usually interpreted in the literature as proving there are no social/upbringing effects on development of adult SSA (same-sex attraction). Instead, the study said that varying social factors leading to SSA occur in different ways in various classes, such as bisexuals, blacks, and effeminate homosexuals. It correctly points out that individual factors contribute to SSA for the whole population in small and diverse ways and that any single cause will result in SSA only in small percentages of a population. The present paper shows that these social factors are collectively significant. An important follow-up study (Van Wyk & Geist, 1984) showed sexual experience factors were very important.

Bell, Weinberg, and Hammersmith (1981a) believe that adolescent SSA development is biologically preprogrammed—in other words, it is fixed in childhood and shows no further change. This is shown to be quite erroneous on several counts. For example, recent work on teenage twins with SSA (Bearman & Brueckner, 2002) shows no genetic influence and a predominant nonshared environmental component.

The conclusion regarding factors contributing to SSA is that social factors are significant, confirming the observations of clinicians, but the influence of the factors is heavily dependent on personal idiosyncrasy.

Introduction:

The Apparent Lack of Studies Showing Social Effects on SSA Origins

In her book on biological effects as the cause of SSA, Cheryl Weill (Weill, 2009) very evenhandedly says she would have included papers talking about social causes, but could not find credible material; she concluded that “there is no data.” By this she seems to primarily mean sociological survey data, since the psychological clinical data has existed for many decades.

Similarly, Savic and colleagues (Savic, Garcia-Falgueras, & Swaab, 2010) say there is no evidence at all of anything after birth affecting sexual orientation. However, sociological data providing such evidence does exist in various papers, particularly because an important decades-old study has been misinterpreted. This research is the foundational study of Bell, Weinberg, and Hammersmith (1981a, 1981b). Other important evidence is provided by the follow-up study of Van Wyk and Geist (1984).

The 1981 Path Analysis Study

After more than a decade of work that resulted in the 1981 Path Analysis Study, Bell, Weinberg, and Hammersmith produced two volumes. The first (Bell, Weinberg, & Hammersmith, 1981a), a 242-page book, describes the interpretation and implications of the large amount of data they collected. Detailed statistical tables of this data and further resulting path diagrams comprise the second volume (Bell, Weinberg, & Hammersmith, 1981b), a 322-page book.

The study examined the causes of adult homosexuality using interviews from the early 1970s, a few years before the Stonewall riot. Their White sample was comprised of 575 homosexual males, 284 heterosexual males, 229 homosexual females, and 101 heterosexual females. Block sampling techniques were used, which should have ensured a reasonably representative sample. A Black sample of adequate size was also interviewed. However, because the sample was not rigorously random in today’s terms, there does remain a real question as to whether the results may be validly extrapolated

to all homosexual people, particularly those not living in urban areas. Participants were asked to complete a 175-page questionnaire that took three to five hours to complete; the survey asked questions checking on almost every cause of SSA suggested at that time by clinicians, psychiatrists, and theoreticians. The survey was designed to directly test whether those theories were valid.

The answers from respondents were combined into areas of similarity, with constant testing to ensure that combining made a measurable statistical difference. Many causes apparently did not significantly contribute to adult homosexuality and were eliminated. Such a procedure was understandable for that time, but probably mistaken because it questionably assumed that a single unique cause for SSA would predominate and be highlighted.

After all those factors were eliminated, what ultimately remained were about fifteen factors, or variables, such as “hostile mother,” “homosexual genital activities in childhood,” and “felt different for gender reasons.” These factors were examined statistically for connections to other variables and the end point—adult homosexuality—using a technique called *path analysis*, which is supposed to give information about causes. The results were published in 1981, a decade after data was collected. This certainly cannot be considered a hasty publication.

Path analysis tries to identify the most common path or paths leading to a particular condition—in this case, homosexuality. Path analysis produces diagrams (figures 1 and 2, redrawn and altered from the 1981 study) that visually show the network of causes; it then attempts to assign a relative importance to each cause. The method works best when there are a relatively small number of causes. For that reason, it does not seem to be an ideal tool for the study of homosexuality, because even from the diagrams there seem to be a multitude of causes or paths, many of which are social/family factors. It is also doubtful that the sample size was large enough to firmly establish the large number of paths displayed. In any case, the authors thought the results for social influence were illusory, and they preferred a biological explanation.

A second, approximately confirmatory study by Van Wyk and Geist, published in 1984, used male and female data collected by Kinsey and his co-workers in the 1940s, but corrected for sampling bias. While Bell and colleagues found preexisting sexual feelings were primary as a cause of homosexuality, Van Wyk and Geist found instead that sexual experiences (social factors) were primary.

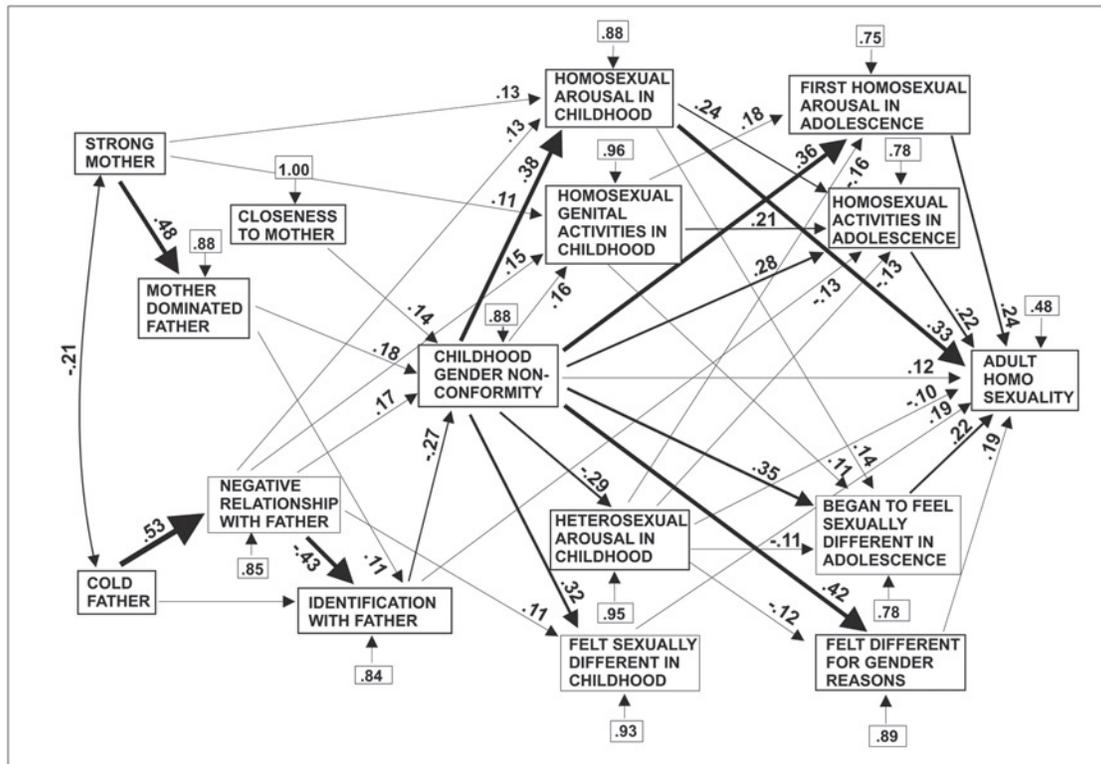


Figure 1. Path diagram for male adult homosexuality. The thickness of arrows is proportional to the strength of connection. The strength of effects is also indicated by the numbers alongside the arrows. Numbers alongside the boxes indicate the influences due to chance.

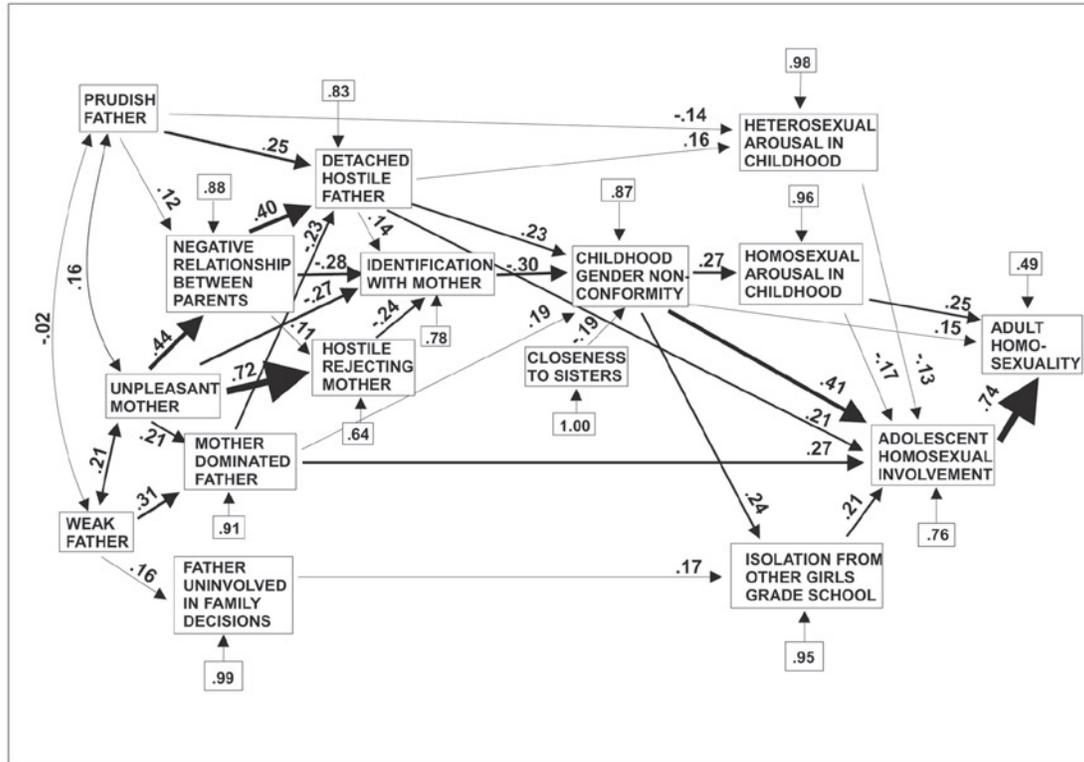


Figure 2. Path diagram for female adult homosexuality. The thickness of the arrows is proportional to the strength of connection.

Some paths in figures 1 and 2 showed up more strongly than others, but even the strongest variable was described by the authors as rather mediocre as a direct predictor. Child gender nonconformity (“sissiness”) was the strongest single variable for boys, but on a scale of 0 to 100, it measured only 12% as a direct contributor to homosexuality. This means that few “sissy” boys became homosexual as a direct consequence of their sissiness. However, when indirect paths (such as sexual arousal) were considered, the contribution was much higher (61%). This means that most paths—about 80%—were indirect and depended on other factors being present. This is an important and typical pattern in sociological research (Rutter, 2006); most factors interact with a multitude of others. However, researchers in the 1980s were still looking for single predominant factors that might cause SSA, and as a result they postponed examination of the idea that many factors might be involved.

The individual psychiatric/psychological factors popular in 1981 were shown in the study to have almost zero direct or indirect effects on adult homosexuality. This was a striking and challenging finding and seemed to remove most such theories from further consideration in the eyes of many academics. These academics thought the clinical psychologists with their vivid case studies showing the importance of individual psychological factors must simply be wrong. The clinicians, on the other hand, unable to deny their clear clinical experience, basically ignored the statistical findings.

This study had a strong effect on subsequent thought. In 2010, Google Scholar found the study was cited 680 times by other scholarly works, which by definition makes it a citation classic. To put that in perspective, most papers in most fields do not get cited by anyone—even by their authors.

Since studies showed that individual social factors did not clearly predominate in the development of SSA, critics of psychological theories of SSA then tended to emphasize and research biological causes. But in the thirty years that followed, no single biological factor was shown to have a predominant influence on SSA. This alleged choice between nature and nurture has been debated in many areas other than SSA, and a general academic consensus from the last few decades is that both are always involved and both are multifactorial. Whitehead (2011) has emphasized that chance is also an important factor that was previously considered too little in the debate.

This 1981 Path Analysis Study is particularly important because it has been consistently misinterpreted ever since its publication, probably because its specialized statistical techniques are not familiar to the average reader nor even the psychotherapeutic community, who felt unqualified to challenge them. (There appear to be no subsequent reviews of the study with informed statistical commentary.) The usual claim by several authors is that the study disproves *any* social cause for homosexuality (APA Task Force on Appropriate Therapeutic Responses to Sexual Orientation, 2009), though Bell, Weinberg, and Hammersmith had a considerably more nuanced approach.

Denial That Social Factors Were Important

Bell, Weinberg, and Hammersmith (1981a) initially found a strong link between their social factors and adult SSA. Social/upbringing factors explained an almost unprecedented 76% of the variation (variance) for both men and women, but the researchers then denied that those social factors were significant—an unusual course of action. We therefore consider in detail the appropriate passages in their book (italics are in the originals):

Our path models for describing the development of sexual orientation succeed in explaining 76% of the variation in adult sexual orientation among both the men and the women in our study ... As we have already pointed out, this value is extremely high—so high, in fact, that it forces us to consider adult homosexuality as just *a continuation of the earlier homosexual feelings and behaviors from which it can be so successfully predicted*.... we should not forget that these variables in large part simply measure the same phenomenon at different times.

What our study suggests, then, is a strong continuity between a person's childhood and adolescent sexual feelings (and to a lesser extent behaviors) and his/her adult sexual preference.... The very strong continuity between preadult homosexual patterns and an adult homosexual preference can be interpreted as reflecting an extraordinarily strong conditioning effect of some sort that "tracks" people into homosexuality. (pp.186–187)

By the time boys and girls reach adolescence, their sexual preference is likely to be already determined, even though they may not yet have become sexually very active. (p. 186)

We therefore conclude, statistically speaking, that the presence of [adolescent] homosexual feelings and homosexual activity in our model is due to statistical artifact; i.e., they are there not because they are really distinct from

adult homosexuality but simply because they occur before adulthood. In plain language, we infer that what the model is telling us is that sexual preference seems to be pretty well established early in life and Adult Homosexuality simply represents the last stage in the emergence of a deep-seated pattern of homosexual responsiveness. (pp.103–104)

The negative reaction of Bell, Weinberg, and Hammersmith to such a good explanation of homosexuality was quite reasonable. It is so rare in the social sciences to find such high variance contributions that one must certainly look for other explanations. The immediate explanation they give is “tracking”—a strong pattern of homosexual response that does not change significantly with age and that is completely responsible for the apparent importance of social factors (though, if that is true, it makes their path diagram essentially meaningless).

One can indeed believe that a very sexually active adolescent might continue this sexual activity into adulthood—a form of “tracking,” or perhaps more realistically a type of repetitive behavior. This pattern could indeed contribute in a major way to that improbably high 76% of variance.

This means their research design was confusing. If there is a very strong connection between adolescent homosexuality and adult homosexuality in a path diagram, that connection will greatly obscure the possible influence of social factors. If presenting a path diagram for the influence of social factors, the data should have been presented without this adolescent factor.

The quotes demonstrate that the study authors have extended their ideas about adolescents to children, with the conclusion that the homosexual pattern is essentially fixed and unalterable from childhood on. However, the authors do not actually calculate the degree to which their assertions are true, although they could have. This failure, at best, is sloppy interpretation. This paper will confirm that adolescent SSA is not fixed from childhood but remains very variable throughout adolescence.

Biological Explanation?

The authors also concluded: “What we seem to have identified . . . is a pattern of feeling and reactions within the child that cannot be traced back to a single social or psychological root; indeed homosexuality may arise from a biological precursor” (pp. 191–192).

We should note that there are hence two explanations given by the authors for their results. The first explanation is the idea of tracking (an artifact of research design); the implication is that tracking completely explains the path diagram. The second explanation is that biological/genetic factors may completely explain the path diagram, and although this important point is nowhere explicit, they probably intend to say that they coincide—in other words, tracking is created by prenatal biological imprinting.

When the authors were attacked in a book review by Tripp (1982) for espousing biological explanations, they coyly denied they had done so; they maintained that they had only suggested biology might be a factor (Hammersmith, Bell, & Weinberg, 1982). However, they indeed seem to have espoused biological explanations, which was also how later readers interpreted their results. In later advice to physicians, Hammersmith (1987) herself had hardened (or possibly clarified) her position to say:

Homosexuality does not develop through social learning or sexual conditioning—e.g. early pleasurable experiences with members of one’s own sex, traumatic experiences with members of the opposite sex or a same-sexed environment. . . . Homosexuality appears to emerge rather independently of the causal factors so long espoused in psychiatry, psychology, and sociology. Although this study found some correlations between homosexuality and various factors thought to influence its development, a sequential path analysis showed most of these to be either reflections of an already-established inner difference, or to have virtually no independent effect on sexual orientation. (p. 183)

I strongly suspect that one's inner sexual orientation is biologically rooted for the vast majority of both homosexuals and heterosexuals. (p. 184)

The crucial implication for psychotherapy is simply that sexual orientation appears to be a given. It is not a matter of choice, and no-one is to "blame." . . . Beyond that, I think therapeutic speculation about why a particular person became homosexual is purely metaphysical and most likely a waste of time. (p. 185)

These statements show that Hammersmith (and probably her coauthors) would prefer a simple biological explanation for SSA origin—or possibly no explanation at all. This is a fascinating but illegitimate confusion of the psychological and sociological. She says one should not speculate about individual cases, but instead should rely on the sociological analysis. A sociologist is hence saying quite illegitimately that psychological evaluation of individuals is wrong and should not be used. On the contrary, deep understanding of an individual—which is possible through extensive interview—may shine light on more general causes, and cannot and should not be overruled by whatever is the usual case in society as a whole (Whitehead, 1996).

Hammersmith's quotations show that the interpretation has moved without justification from saying "We can find no social causes" to saying "There *are* no social causes, and causes are almost certainly biological." The evidence quoted for this by Hammersmith included studies that are no longer considered such evidence by the research community (for example, supposed estrogen response in SSA males).

It is impossible to prove such a universal negative about social causes—some social cause might always be found in the future. Such is philosophically inadmissible. No such negative statement should be made, nor particularly made the basis of a factor such as public policy.

Literature Showing Tracking Is Not Predominant

While it is completely true that the 76% of variance explained arises at least in part from the preexisting adolescent SSA—in other words, the very structure of the research that included adolescent stages—it is very misleading to think that there is universal tracking from adolescence to adulthood. Looking more closely, we find that some of the very data reported in the study contradict the idea of universal tracking, and other literature concurs. A number of places in the study compare preadult (including childhood) homosexual feelings or behavior and present data on whether they continued into adulthood. For men, preadult sexual *behavior* was predominantly homosexual for only 56% of the eventually homosexual adults (p. 100). Similarly, for predominant preadult homosexual *feelings*, the corresponding figure is only 59% (p. 99). For women, the corresponding predominantly homosexual behaviors were 22% (p. 168), and the corresponding figure for feelings is only 44% (p. 167). Only about half the homosexual men and a smaller percentage of the women show “tracking.” The effect is not “extraordinarily strong,” as the authors stated, but moderate at best. So approximately half of the SSA male teenagers are no longer SSA active as adults, nor do they have homosexual feelings. This degree of change is now a well-known rule of thumb that has been documented by others (Sandfort, 1997; Whitehead & Whitehead, 1999), and because there is so much change, it is clear that tracking is not a predominant factor.

Stability of teenage attraction was the subject of a very large and detailed subsequent study by Savin-Williams and Ream (2007). They similarly concluded that teenage homosexual attractions were so variable over time that they queried whether the concept of homosexual orientation had any meaning in the adolescent context. Whitehead and Whitehead (2010) calculate from those data that midteenage homosexuality is at least twenty-five times as unstable as teenage heterosexuality. For example, overwhelmingly, a sixteen-year-old with SSA (including cases concurrent with opposite-sex attraction) will be exclusively heterosexual in attraction the following year.

This means that the idea of stability, or tracking, is true only for some SSA people. Was this known in 1981? Yes. Even at that time, it was the general professional opinion that the orientation of SSA teenagers was notoriously unstable, being well-known from the sociological data of Kinsey, Pomeroy, and Martin (1948). This opinion, also held by the psychological professions, was summarized in an incensed commentary by Barnhouse (1977):

It is impossible for me to state strongly enough that to present this model to young people, or to allow them—as often happens in the contemporary climate of open discussion—to imagine that their transitory adolescent experiments are truly indicative of a settled homosexual disposition, is not only evidence of psychiatric ignorance, but is specifically wicked as well. (pp. 153–154)

Because of this well-known contemporary psychological fact (the lack of tracking by adolescents) and the implications from their own data, it is not clear why the authors said that “extraordinarily strong” tracking takes place. That conclusion was simply not correct.

The analysis of Van Wyk and Geist (1984) clearly showed that throughout adolescence, the probability of homosexual activity continuing into adulthood increases with age. However, what is the explanation? Is it inevitable revelation of an already firmly fixed biological propensity, or is it learned social behavior that becomes strongly embedded? Bell, Weinberg, and Hammersmith (1981a) give both possibilities, but opt very strongly for the former, saying their sample shows that feelings precede any behavior (p. 188). An important question remains: Do SSA feelings before adolescence have any social causes?

Direct Evidence for Social Factors

It is possible to derive such a test from the tabulated data in Bell, Weinberg, and Hammersmith (1981b). We find in the data for those exclusively SSA in adulthood—

hence the clearest case (Bell, Weinbergs, & Hammersmith, 1981b, p. 250)—that 28% of SSA feelings variance in children is explained by social factors, particularly “childhood gender nonconformity” and “strong mother.” Similarly, “began to feel sexually different in adolescence” has 24% of feelings variance explained this way, and numerous social factors are tabulated as contributing to it (p. 250). These numerical results are weak to modest but are far from zero. It is beyond the scope of this paper to give more detailed calculation, but the actual social factors contribute somewhere between 24% and the inflated 76% previously attributed to tracking. So even if the feelings began in childhood, there is evidence for social explanations. The “universal negative” hypothesis of no possible social factors is negated by this positive evidence.

The authors were disappointed in their 1981 study that they could not find a single social cause for SSA. At the time, it was reasonable to seek a single predominant cause. However, in hindsight we must ask why there would not be multiple causes for SSA, just as we find with so many other psychological states? Because the authors were searching for a single unified cause and could not find one, they thought a single biological cause might be more reasonable. However, genetic influences—for example, the genetic influence on schizophrenia—are similarly now known to be multifactorial (Nieratschker, Nothen, & Rietschel, 2010), and we know that in those cases very many genes are involved. Today a social multiple-cause hypothesis would be considered more likely. For example, “The great bulk of psychological traits and of mental disorders is multifactorial in origin” (Rutter, 2006, p. 221).

Social factors as a whole are significant, but individually they are not significant. This could mean a) that the correct questions have not yet been asked, which is rather unlikely; b) that the interactions are much more complex than the model suggests, and the influence of the social factors has escaped detection; or c) that there is much individuality (chance reactions), and although indeed no factor will produce SSA in more than a small percentage of those exposed, such factors are quite important to those who do respond.

Complex interactions could be possible, but individual erratic response is a simpler hypothesis and is already known. An example is early sexual abuse. Most individuals are resilient and not greatly affected in the long term. But a minority are hugely, devastatingly, and enduringly affected (Rubino, Nanni, Pozzi, & Siracusano, 2009).

The boxed numbers in the path diagrams show the fraction of the variance for a particular characteristic that was not explained by factors in the study and was attributed to chance. The numbers can be quite large, and this chance element is another important factor. It seems, then, that the best option is the third option (c).

The work of Otis and Skinner (2004) showed that almost all homosexual people could name a few factors that probably contributed to their homosexuality. Their work also demonstrated that there was much variety in the factors named—in other words, chance and individuality were important.

If SSA is indeed the result of uncommon reactions to social factors, and if indeed most people are not affected, then eliminating apparently unimportant paths—as was done for path analysis—eliminates some actual paths that are traversed and is a quite inappropriate model. A different analysis structure is needed—a kind of outcome analysis that tabulates the paths most commonly traversed to arrive at SSA only, or alternatively those factors that had the most impact on individuals who arrived there. A paper describing this is in preparation and approximates the approach of Otis and Skinner (2004).

**Van Wyk and Geist's Findings Partly Contradict
Bell, Weinberg, and Hammersmith**

Van Wyk and Geist did not agree that sexual feelings are most basic. They found from their calculations on the Kinsey data that sexual experience variables are significantly more important. The two studies clashed. Van Wyk and Geist hence suggested that the predominance of primary feelings in Bell, Weinberg, and Hammersmith (1981a) is an illusion. The feelings could arise from sexual experience, such as masturbation. They also argued that many experiences such as atypical play

interests, ostracism by peers, and uncommon types of sexual experience are easily summarized by the brain as feelings—in other words, “felt sexually different.” Feelings, then, are social causes in disguise. Van Wyk and Geist also argued that idiosyncratic feelings and experiences were eliminated from consideration, but definitely do affect sexual orientation. These are reasonable arguments and would mean that social impacts of various sorts are quite possible.

How should the clash between the two studies have been resolved? Which is more important—feelings or sexual experiences? On the one hand, we have Bell, Weinberg, and Hammersmith arguing an unknown (but probably biological) origin for feelings; on the other hand, we have Van Wyk and Geist arguing that the pleasurable effects of sexual experience lead to a continuation and strengthening of those feelings. But where did the feelings originate? Are they due to innate biological factors?

The assumption of tracking as an explanation really derives from the organizational/activational hypothesis of Phoenix, Goy, Gerall, and Young (1959). They assert that sexual orientation is fixed prenatally in the brain and although quiescent until puberty, when it is activated, it must manifest itself and no social factor will significantly affect it. The work of Van Wyk and Geist seriously questions that hypothesis with evidence that sexual experience factors are predominantly important.

The clashing results pattern of the two studies is counter-intuitive for historical reasons. The Kinsey sample of Van Wyk and Geist was from the 1940s, whereas the sample of Bell, Weinberg, and Hammersmith postdated the sexual revolution in the 1960s and should have contained much more sexual experience in the collected histories. Thus for the Kinsey data the feelings should be extremely important, the sample having perhaps less “outlet” in their terms during that era, but instead it is shown in the work of Van Wyk and Geist that it is the sexual experience in the Kinsey sample that is predominantly important. This contradiction needs further work to resolve. However, it is possible that the very aggressive interviewing style of Kinsey—in which respondents had

to strenuously and actively disagree that they had various suggested behaviors—elicited more accounts of actual behaviors than did the interviewing style of Bell, Weinberg, and Hammersmith.

There has not been another large study since the early 1980s that can be directly compared with these two conflicting studies, though there have been many with other emphases. However, there is a much larger and important study on whether common prenatal factors are overwhelming for teenage same-sex romantic attraction (or feelings). (Bearman & Brueckner, 2002). That study found zero genetic influence, and showed that at least two social influences were significant (opposite-sex twins influenced each other and elder sisters were an influence). But the predominant influence was chance, or nonshared individualistic influences. This shows clearly that the increasing development of SSA throughout adolescence is not due to prenatal influence nor the sudden emergence of a preprogrammed biological process. We conclude that strongly individualized experiences are paramount. This means that the broad-brush sociological approach is not helpful for uncovering specific causes of homosexuality, and we must return to the individualized clinical case-studies.

Invocation of Social Factors in the 1981 Study

Although Bell, Weinberg, and Hammersmith (1981a) discount social factors, they invoke them (highly inconsistently) elsewhere in the study as significant and significantly different for different subgroups. They devote chapter 18—nearly 10% of the book—to discussing path diagrams for exclusively homosexual people, effeminate homosexual people, homosexual people in therapy, and Blacks. They find different path diagrams for each case—in other words, different social factors—and even conclude that for Blacks, experiences are more important than feelings (p. 197). In light of this, the blanket statement that tracking is responsible for adult homosexuality as a whole is quite inconsistent. If tracking is genuinely overwhelmingly important, then the differences in

social factors to which they devote so much space are quite meaningless. If, however, different social factors are genuinely responsible, then the blanket statement of “no social factors” is again quite inadmissible:

The findings reported in this chapter clearly demonstrate that there are variations in the patterns of homosexual development among different types of homosexual men and women” (p. 210). “For the bisexuals, by contrast [to exclusive adult homosexuality], a homosexual preference seemed to emerge later and to be more tied to learning and social experiences” (p. 211).

Social factors are indeed important.

Summary

It is simply a myth that there are no sociological data showing influence on adult sexual orientation.

Path analyses from 1981 have been used to argue that there is no social or familial basis to homosexuality. The path has proved rather slippery and the conclusion is completely unjustified. Bell, Weinberg, and Hammersmith (1981a, 1981b) thought prenatally-established tracking accounted for most adult homosexuality. This is quite wrong.

The current paper emphasizes the importance of individuals and their experiences, which is the traditional case-study approach of psychological clinicians. Sociological study confirms that social causes as a whole are significant, but the details must be filled in by narrated personal experience. It is also likely that chance reactions to the same common environmental factors are far more important than usually thought.

It is ironic that a study touted as disproving all influence of social factors actually shows that social factors as a whole are significant. Bell, Weinberg, and Hammersmith

(1981a) say, “To therapists, we would suggest that exclusive homosexuality probably is so deeply ingrained that one should not attempt or expect to change it” (p. 211) Rather, their own evidence shows that even therapeutic change should not be dismissed *a priori*.

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