

## Data are the Natural Enemy of Hypotheses: Reply to Holland (2009)

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In our study (Voracek & Fisher, 2006), we analyzed available online data (as of mid-2001) for a sample of 125 elite (i.e., highly popular and successful) 1990s porn actresses from a leading European adult media company (Private Media Group Inc., [www.prvt.com](http://www.prvt.com)).

Our central finding from this unobtrusive-measures design was the following set of associations. In this sample of porn actresses, lower body mass index (BMI) (an androgenousness cue) was correlated with more frequent movie starring, whereas several curvaceousness cues (waist-to-hip ratio, waist-to-bust ratio, and bust size) were not. Conversely, in the same sample, curvaceousness cues (lower waist-to-hip ratio, lower waist-to-bust ratio, and larger bust size) were correlated with more frequent magazine starring, whereas the androgenousness cue (BMI) was not. Of note, the association of lower BMI with more frequent movie starring remained even after we statistically controlled for models' age, magazine starring frequencies, and curvaceousness cues. Similarly, the associations for each of the curvaceousness cues with more frequent magazine starring remained after we statistically controlled for age, movie starring frequencies, and the respective other curvaceousness cues.

While it is a truism that correlation does not equal causation, correlations do have causes. A pattern of correlations, found in the same sample and as distinct, differential, and stable as the above set of associations, is striking, and, in all likelihood, it reflects some underlying causes. Specifically, in

our case, it led us to speculate whether visual cues to female physical attractiveness might indicate domain specificity. Thus, in our original discussion, we proposed that cues of androgenousness (versus curvaceousness) may be particularly salient for evaluating the attractiveness of moving (versus posing) female bodies.

In Holland' (2009) comment on our work, he takes issue with the general approach we used, calling it "problematic." Specifically, Holland asserted that porn actresses or nude models were an allegedly poor source of information about the features underlying female physical attractiveness, as evaluated and preferred by heterosexual men. Although we are in agreement with Holland in that the majority of previous physical attractiveness research has limited ecological validity, we do not agree with Holland's assertion that sexually explicit media (especially those available on the open market) lack validity and are inappropriate for the study of female physical attractiveness. We do not stand alone in maintaining this perspective; his stance is clearly in the minority, as indicated by the number of contemporary, authoritative appraisals and research examples pertaining to this question that we cited when discussing the background of our own research (Voracek & Fisher, 2006, p. 298).

Most notably, we referred to Salmon and Symons's (2001) well-known statement, reiterated here, that adult media's "characteristic features have been shaped in free markets by the cumulative choices made by...men...who have 'voted' with their money" (p. 56). More research examples along these specific lines have been reviewed in a recent monograph about consumption, as viewed from the standpoint of evolutionary psychology, which was published after our article (Saad, 2007, pp. 228–235). It is precisely this inevitable logic concerning the constraints and dynamics of free markets that are shaped by consumers who vote with their money that is relevant here. Market forces constitute the

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ultimate validity of commercial, sexually explicit media as an informative source for female attractiveness research. Pornography as an industry is about successfully selling visual enactments of male sexual fantasies. If these products were to miss this target, male consumers would not spend their money and adult media companies would become bankrupt. As for the source of the data we analyzed, we made it clear that we did not choose a company at random, but rather an undoubtedly successful and, in fact, a leading company. Private Media Group Inc. was founded in 1965, is active in over 35 countries, was the first adult entertainment company traded on the NASDAQ stock market (in 1999), was chosen by *Forbes Global* as one of the Top 20 best small companies in the world (in 2002), and reportedly owns the largest digital online archive of adult entertainment content in the world ([www.prvt.com/company.php](http://www.prvt.com/company.php); [http://en.wikipedia.org/wiki/Private\\_Media\\_Group](http://en.wikipedia.org/wiki/Private_Media_Group)).

Holland sets aside all of these important facts and does not even mention our central finding (namely the associations summarized above). No alternative analysis or interpretation of our data were offered nor were novel data (observational or experimental) presented that would yield opposing evidence to our specific findings. In essence, in his comment Holland assembled various points which were not new in this research context nor central to our findings; his more or less related points did not threaten the validity of our findings. However, due to the potential interest of readers, we address the major points raised by Holland in turn below.

First, Holland noted a “substantial overrepresentation” of models from Eastern Europe in our sample and further noted that we did not address this observation. From our sample description, it is clear that the sample was ethnically and geographically fairly homogeneous, as more than 90% of the actresses were Caucasian and 85% from Europe. Although, as mentioned, the proportion of East European actresses was sizable (38%), it was not conspicuously or surprisingly large when one considers the company’s European background. Holland overlooked the fact that we analyzed data from an European company, which, as one might expect, almost exclusively features European actresses. If one was to examine an American company, the resulting data would very likely contain fewer East Europeans. Therefore, the fact that European models were used by a European company warranted no attention in our original article.

Holland’s claim that actresses’ weight was underreported is not new to this area of research, as indicated by the accompanying references he cited. However, is there compelling evidence to support the veracity of this assumption? In general, we think not. Numerous studies in the sports sciences have reported a similarly low (i.e., <20) average BMI within samples of young female athletes (for data on female fencers, see Voracek, Reimer, Ertl, & Dressler, 2006), so there is no reason to expect that lean, young women with a BMI of 18 or

19 would inevitably have emaciated looks. Interestingly, Holland also cited a study of female fashion models (Tovée, Mason, Emery, McCluskey, & Cohen-Tovée, 1997) which found a lower average BMI (17.6) than we did for the porn actresses sample (18.4). Presumably, some women who aspire to become models do not and they may later become porn actresses. If this is the case, then these converging measurements appear quite fitting and credible. Holland also criticized previous work of ours (Voracek & Fisher, 2002) as being naïve in that we allegedly relied upon the underreporting of body weight in *Playboy* centerfolds. However, his affirmative reference (Szabo, 1996) was an analysis of merely 11 centerfolds during 1994–1995 of the South African [sic] *Playboy* edition, whereas we analyzed 500+ centerfolds from 1953 to 2001 of the magazine’s U.S. edition. Therefore, his only supportive evidence is irrelevant. These facts aside, even if there were indeed a consistent underreporting bias in model’s weight, the results of our studies (Voracek & Fisher, 2002, 2006) would be entirely unaffected, as both studies were correlational and correlations are invariant under linear transformations of the variables from which they are calculated (e.g., underreporting, or negative bias, of weight).

Holland also claimed that, owing to a supposedly high prevalence (up to 50%) of breast implants, the utility of actresses’ bust measurements was limited. Holland ascertained these prevalence figures from nude pictures, available online and related to the ongoing Private video series “The Private Life of [porn actress name],” but concurrently conceded that there was “an element of subjectivity in discerning breast implants from pictures.” Since Holland provided no methodological details, no independent observers were involved, and the assessments were unverified, there is reason to doubt the validity of his findings. Holland’s statement that “breast implants are not consistent with making moving bodies look firmer” is surprising, to say the least. In fact, we expect that cosmetic surgeons will emphatically disagree with his claim. If this were really true, women in general would not utilize breast implant surgery (Fisher & Voracek, 2006); average bust size in our sample was not conspicuously large (slightly less than 86 cm), suggesting that women must be considering more than size when they opt for implants. Holland also overlooked that our data were collected up to mid-2001 and thus comprised of actresses from the 1990s, when breast implants much were less prevalent than in the late 2000s (Swami et al., 2008). In contrast, the “Private Life of...” series he consulted mainly has release dates around 2005 and later and thus does not overlap with our database. As set out above, even if there was a consistent positive bias in model’s bust size (through breast implants), our results would be unaffected, due to the invariance of correlation coefficients under linear variable transformations.

Finally, Holland asserted that anal intercourse is uncommon in the general heterosexual population and yet it is frequently depicted in productions of Private Media Group Inc. He perceived this incongruity (which we argue to be false) as another clue as to the inappropriateness of the data we utilized. There are two important issues that warrant discussion. First, the rise in the depiction of female receptive anal intercourse in pornography since the late 1990s is well-known ([http://sexuality.about.com/od/sexinformation/a/anal\\_sex\\_stats.htm](http://sexuality.about.com/od/sexinformation/a/anal_sex_stats.htm)) and not specific to this company. Second, Holland fails to quantify what he means by “uncommon.” For example, uncommonness in statistical significance testing may be defined as occurring <5%, which is the conventional significance criterion, or in genetics, as <1% which is the cutoff point between common polymorphisms and rare allelic variants. Closer scrutiny of Holland’s only affirmative reference for his claim (i.e., Laumann, Gagnon, Michael, & Michaels, 1994, p. 130) reveals that 13.2% women (ages 18–59) in cohabitations and 7.6% women in marriages reported any occurrence of anal intercourse in the last year (based on a national probability sample, collected in 1991 in the U.S.). So, then, is anal intercourse truly “uncommon”? Relatedly, there is ample evidence for cohort, period, and cultural effects in this sexual practice which, in general, is more common than oftentimes assumed (Voeller, 1991). It has been estimated that, in terms of absolute numbers, about seven times more heterosexual women than homosexual men engage in receptive anal intercourse (Halperin, 1999). As for period effects, more recent surveys (2000s versus 1990s) yield higher prevalence estimates. For example, according to the U.S. National Survey of Family Growth one-third of women (ages 15–44) reported ever having had anal intercourse (Leichliter, Chandra, Liddon, Fenton, & Aral, 2007), and one U.S. study of female heterosexual STD clinic attendees (Satterwhite et al., 2007) reported prevalence estimates of 18% (1999–2000) versus 7% (1993–1995). As for cohort effects, younger survey respondents (college-age U.S. women) likewise report higher prevalence estimates than respondents from previous birth cohorts (Baldwin & Baldwin, 2000: 23%; Flannery, Ellingson, Votaw, & Schaefer, 2003: 32%). As for cultural effects, heterosexual anal intercourse appears to be more prevalent in some cultures outside of the U.S. (Jaeger et al., 2000). For example, household surveys from Brazil indicate that for 40% of rural and 50% of urban heterosexual couples it is a normal part of their sexual repertoire (Morris, 2004, p. 232). Therefore, contrary to Holland’s assertion, anal intercourse is not uncommon among heterosexual couples.

All in all, the issues we discuss above in response to Holland’s comments seem to prove the truth in the maxim that data are the natural enemy of hypotheses (attributed by McGrath, 2005, to Goethe, but searched in vain by us in his *Maxims and Reflections*; von Goethe, 1998).

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