Bioethical Inquiry (2007) 4:29–31
DOI 10.1007/s11673-007-9036-7

Rejoinder

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Published online: 28 February 2007

I have clearly escaped the heat of a more challenging Clark-Grilling [1] of my argument for the importance of quality evidence in health care [2] when my critic concedes that "the value of a scientific research base for therapeutic practice is not in question."

Two angles of criticism are easily disposed of. That orthodox practice is far from achieving its own ideal of being evidence-based is perfectly true, but this is irrelevant to arguments supporting TI-CAM. Ironically, "quackery" was once characterised by orthodox physicians as reliance on the observed, while they legitimised their clinical practice on the basis of their particular illness theory [3, 4]. Old habits die hard, but this just means that the epistemic warrants of existing and proposed treatments must submit to tests of legitimacy.

I also agree that external conditions have prevented CAM from occupying a level playing field with orthodox medicine, in order to build an equivalent scientific evidence base. The claim implies that with different external conditions, CAM would be further along the track of scientific authentication as valid and safe. Quite so.

Clarke-Grill's central critique focuses on conceptual inconsistencies in the mainstream discourse, with supporting commentary on primary care and informed consent. Far from conflating orthodox medical practice and EBM, I showed how the medical profession is applying EBM principles *to* clinical practice, thus distinguishing the two. I should, perhaps, have explicitly stated that evidence is not the only element of clinical practice, alluding to the current debate about how evidence, clinical experience, pathophysiological rationale and patient preferences should be integrated [5, 6].

My second "fundamental mistake of reasoning" was to conflate EBM and science. According to Clarke-Grill, using "science" to signify "the systematic inquiry into natural phenomena" would rightfully put science into a category other than "world-views" or "perspectives," but I define "science" as the narrow disease and molecular premises of the biomedical sciences, and this is a perspective. Not so. I criticised CAM precisely for not employing systematic inquiries, and I also showed that EBM methods can demonstrate efficacy in areas like psychiatric practice, where the working concepts are psychological as well as molecular.

Clarke-Grill's statement "Whether the value of a perspective is considered superior, equally valid, or inferior clearly has mostly to do with the beholder" is startling in the contexts of health care and her concessions to the value of the scientific research base. Tom Cruise may value Scientology with a passion, but more is required when deciding on the

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public funding of health care and the legal standard of care. The beholder as patient has the final say on value, but only in the sense in which we reserve to competent patients the right to refuse treatment which they disvalue, all things considered. This is distinct from the need for scientific inquiry to provide maximally useful strategies to choose from or refuse.

Many CAM approaches are certainly considered useful by patients despite their lack of scientific validation, but such "clinical legitimacy" is inadequate. Patients of doctors who are found guilty of gross over-servicing charter buses to Canberra to support them, but nobody thinks this demonstrates clinical legitimacy. My doubtless incomplete account of the rise in popularity of CAM provided some indication of the complexity of the phenomenon, beyond dissatisfaction with orthodox medicine. *Pace* Clark-Grill, Astin's 1998 survey [7] was no confirmation of clinical legitimacy. The survey concluded that dissatisfaction with conventional medicine did not predict the use of alternative medicine, and Clarke-Grill's misleading paraphrasing ("He questioned 1,035 randomly selected participants, and asked what their main grounds for the use of alternative medicine were.") inflates the level of dissatisfaction discovered, given that only 4.4% of the participants relied primarily on alternative health care.

Against Clarke-Grill's assertion that I defend biomedical scientific premises as the only valid ones, I claim in the paper that what is at stake is the natural regularities / probabilistic predictability/regulatability nexus, against which any putative conception of health care must be tested. I showed that rudimentary scientific quantification gets its foot in the door the minute a practitioner goes beyond treating someone as a *pure* individual, unrelated even by simple analogy to anyone else. So I am happy but puzzled, when Clarke-Grill joins me at the TI-CAM barricade, to explain how homeopathy is being tested against EBM criteria. If studies continue to demonstrate the numerical efficacy which Clarke-Grill apparently – and rightly – respects, the implications for the basic understandings of orthodox medical science will indeed be momentous. But that is just how science works: testing pathophysiologic theories by observation and trial, reviewing theories in light of trial data, and so on.

Similarly, either the TCM practitioner has provided the tendonitis patient with a treatment that might be helpful to others, or it is an *absolutely* individual treatment. If the latter, on what possible basis does the TCM practitioner make the diagnosis? But in fact we know that the EBM door lies ajar, since the patient's problems were identified as part of a "common diagnostic category." If in fact CAM holds "many success stories" like this case, it cannot be sufficient to imply that preferring indirect (trial) evidence over primary experience, represents merely an "epistemic choice" rather than a scientific necessity [3: 1216]. Anyone holding out as an effective practitioner should indicate how successful their treatments are.

Finally, Clark-Grill concludes that EBM research data is an unreliable foundation for primary care treatment because of medical complexity, paucity of primary care research, the focus of orthodox research on diseases and organ-specific outcome measures, and early presentations not conforming with diagnostic criteria. All true, but grounds for a different conclusion: primary care practitioners must determine how well the available research data applies to their individual patients [8]. A great mistake is perpetrated when a principled distinction is made or implied concerning the applicability of quality evidence to specialty practice and primary care [9].

Unfortunately, Clark-Grill has muddled the concepts of uncertainty and evidence. The uncertainty inherent in probabilistic reasoning is the *currency* of EBM, and properly informed choice *does* require an indication of probabilities of risks and benefits, which practitioners who eschew EBM cannot provide. Orthodox medicine has not perfected information provision, but serious educational strategies [10–12] and evolving medico-legal requirements have been evident for two decades in Australia [13] and other western jurisdictions. The assertion that many doctors who use CAM therapies, but not those in orthodox practice, indicate to patients whether information is available or not, is evidence-base*less*.

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