

# Purpose, Insight, and the Review of Literature

Let me assure readers that this is not a tutorial in writing reviews of literature. What this is, instead, is a commentary on the importance of knowing what purpose a review is to serve before it is written.

## *Essential Questions*

The elements of a literature search, the criteria used to rate evidence, and the varied methods and formats for presentation are mechanics discussed in texts and papers easily found with a good search engine. Increasing the utility of a literature review is the subject of numerous critiques and methods papers. Meta analyses still are the gold standard for quantitative studies, particularly comparing results of well-designed RCTs. Meta-syntheses, integrative, critical, and scoping literature reviews may be used for subsets of literature to address different types of questions. Rating systems differ according to the method and level of evidence. As a reviewer and reader of other publications, and particularly as co-Editor of *Public Health Nursing*, the question I am compelled to ask is, “So what?” By that I mean, has the author cut through to the answers that have been found and identified the questions that remain? Does this review help me distinguish between quality and mediocrity? Does this literature enhance my knowledge of a new topic? Has the author of the review found and communicated new insights as a result of sifting through multiple studies?

Why write a review unless you (1) mean to demonstrate that you know what’s been written regarding a topic; (2) want to illustrate how the existing literature either adequately or inadequately addresses a question of interest; or (3) have had a “Eureka!” moment of insight based on an aggregation of published literature that no one has yet put together? In my view, these purposes represent stages of development in scholarship. Obviously, the first condition is one that applies primarily to students who must prove their readiness to undertake independent inquiry in a field in which others

have worked. Many readers of this journal have created some form of literature review to pass comprehensive examinations and defend proposals for theses or dissertations, all legitimate reasons for writing a review but seldom sufficient rationales for publication.

## *Clarity of Purpose*

Item (2) characterizes the majority of reviews submitted for publication; the review provides the justification for the inquiry being reported. Done well, these help situate the research in the landscape of existing literature and set the stage for the kinds of reviews described below. They lead readers logically to the research questions which, presumably, have not already been answered. Occasionally, a review is submitted that describes the state of the literature without an attendant research question. While novices may benefit from an introduction to a field of work, seasoned professionals need focused reviews that move them forward. If the criteria for inclusion of studies are missing or if the purpose of a review is not well-defined, the review fails everyone.

## *Familiarity with the Terrain*

It is the third purpose, item (3), that is the least likely to be submitted or found in published form. Like flawless diamonds, unique insights occur rarely. They sometimes do not occur until a researcher has spent years of searching for exactly the right location to drill down to the most precious stuff and, finding it, studies it for a while to produce the insight. You have to know a diamond in the rough when you see one in order to bring a potentially perfect specimen to light; knowing where to position the mine in the first place is critical to finding any diamonds at all. If you do not know, you may emerge with a perfectly lovely but not very valuable bit of quartz.

At the risk of overworking my metaphor, diamonds almost never are found lying in the dust of the earth’s surface, unless someone has carelessly

dropped one. The rarest insights, too, are found after much preparation and consideration. Isaac Newton may have had his gravitational “Aha!” moment under an apple tree, but probably not before he had put some effort into understanding the laws of the physical world using inductive reasoning. In an effort to find gems of knowledge with facets so complex that they draw us toward new understandings, novice investigators often move from study to study skipping across the landscape amassing great amounts of data of varying quality. Some has potential; much is the dust that lies near the surface. Systematic study of low quality results is more likely to introduce bias than to reduce it; this leads us down dead-end paths. The results fail to yield value. Experienced scientists survey the publication landscape systematically, but without the coordinates for where there is the greatest promise of value, they miss the mark. Systematic reviews of literature that are not situated correctly can yield useful information, but they do not move the science forward. In the worst case, they leave perfect diamonds in the dust for someone else to find. Quality work takes time; insight takes only a moment—after years of preparation.

### ***Perseverance***

It is the capacity for insight that takes time to grow. Aside from savants whose neuronal capacity exceeds what the rest of us possess, most human beings slowly develop appreciation for the nature

of inquiry and for the complexity of whatever phenomena we study. That is normal, and fine. Diamonds *are* diamonds precisely because of the pressure placed on them over time. Knowing the terrain of a discipline takes exposure; creating a mind map of the most productive areas of research can take a lifetime. Does this mean we are consigned to wait until we are ready to retire before we publish? Of course not. The purpose of inquiry remains constant, to answer the questions that lead to better understanding. I believe all scholars aim to find those flawless gems in their own research or by connecting the work of others in newly meaningful ways. When we design research or write reviews of others' work we must know why we are doing it, and take care with its presentation. Select the most promising specimens but let the dust slip back to earth. This is not to say that less than brilliant results do not contribute to the development of understanding. Steps forward get us somewhere new. New vantage points bring new perspectives.

Diamonds, natural brilliance, and uniqueness have at least one conceptual link—rarity. Such results are not assured. Aiming for the best, however, we may deliver high quality in any case. With perseverance, there will be insight, and with time, new diamonds to mine. But, if you find a flawless diamond in the dust, pick it up!

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