

About This Book: The Nuts and Bolts

This chapter provides a roadmap for the rest of the book and describes our systematic approach to evaluating and presenting evidence, an approach that reflects our twin priorities of rigor and transparency.

The book begins with a section on “The Cesarean Epidemic,” encompassing both primary and repeat cesarean surgery. Cesarean surgery is the quintessential issue that distinguishes the medical and physiologic models of care. Here they diverge both in terms of beliefs about and consequent attitudes toward cesarean surgery and in the panoply of care practices that, in the medical management model, tend to lead down the track to surgery and, in the physiologic care model, promote vaginal birth. In addition, cesareans have the most salient consequences for women and babies.

Sections III through VII, forming the bulk of the book, examine what practices promote optimal outcomes—the highest probability of spontaneous birth of a healthy baby to a healthy mother who feels pleased with herself and her caregivers, ready for the challenges of motherhood, attached to her baby, and who goes on to breastfeed successfully. The final section steps up to the systems level to evaluate the impact of care providers and place of birth.

The book also contains two appendices. One summarizes best practice for some clinical situations that we call “not-so-optimal,” situations that are deviations from normal but not true pathology. The other looks at preventing maternal psychological morbidity, a crucial but neglected issue.

We structured the chapters with the intent to be useful to a wide range of users with varying purposes. They can be read independently, allowing readers to look up what interests them. Each chapter begins with an *essay* that examines the cultural and historical underpinnings of conventional obstetric practice, delineates the gap between evidence and practice, and explores the factors that explain why that gap persists in defiance not just of evidence, but of biological principles and even common sense. We believe this knowledge is vital if we hope to bring about change. As we also wanted to provide practical information, the chapter essays in the meat of the book end with evidence-based and best practice “Strategies for Optimal Care.” Chapters conclude with a set of *mini-reviews of the literature* summarizing the body of research relevant to the points being argued in the essay. The primary difference between a systematic review and a mini-review as defined

by Griffiths (2002) is that systematic reviews typically address multiple outcomes simultaneously while mini-reviews are more focused and consider just one or two outcomes.³ (Our methods are described below.) These mini-reviews enable readers to determine for themselves the strength of the support for our position. In many chapters, we begin the mini-reviews section with a discussion of the global limitations and weaknesses of the research underpinning that chapter, which can alert readers not only to issues with studies we include but studies that may be published in the future.

While we have focused on care in the United States, the book has international applicability, as clinicians use obstetric interventions injudiciously almost everywhere in the world, and as we have drawn on the worldwide medical literature for our sources, much of which is published in English-language journals. To further enhance the book's value in countries other than our own, we have included research on some practices that differ between the U.S. and other parts of the world, notably mediolateral episiotomy and the prophylactic use of ergot derivatives in third-stage labor, and on systems of maternity care service delivery that differ substantially from the physician-led obstetric management that prevails in the United States. We believe we have produced a book that will be useful to anyone hoping to improve maternity care. We hope you will agree.

Limitations of Scope

While we mention the impact of certain medical interventions on maternal psychological wellbeing and breastfeeding success, have an appendix on optimal practice for protecting maternal mental health, discuss how economic motivations drive practice, and touch on factors affecting maternal satisfaction with the birth experience, we do not perform an in-depth analysis of any of these topics. There are several reasons for this. First, with a few exceptions, the quality of evidence for an association between intrapartum care practices and these outcomes is relatively poor, with many studies subject to considerable bias or relying on surrogate endpoints or survey measures that have not been validated in childbearing women. Second, study results often cannot be generalized to other populations, institutions, systems, or eras. Third, satisfaction ratings depend on many factors, including who is asking (caregiver or neutral party), where the survey is completed (hospital or clinic or woman's home), and time elapsed since the birth, and the validity of ratings depends on whether the woman had access to a comprehensive set of options and chose freely among them based on accurate and complete knowledge of their benefits and harms. None of these nuances or caveats will be captured in simplistic Likert scale ratings. Finally, space constraints forced

us to sacrifice some topics in order to provide detailed critical analysis of the clinical outcomes for which a larger body of research was available.

Although we do not cover these outcomes in-depth in this book, we think a strong argument can be made that physiologic care will optimize mental and emotional health and satisfaction and promote breastfeeding. And to cite but one example of how cost *ineffective* our maternity system is, studies have shown that the cesarean surgery rate could safely be less than 15% in a mixed-parity, mixed-risk population. (See chapter 4.) The 2009 U.S. rate was 33%, or 1,359,000 women of the 4,131,000 women who gave birth that year.⁴ If the rate had been 15%, only 620,000 women would have had a cesarean. This means that 739,000 women or more had cesarean surgeries that could have been avoided. The average maternity services payment for a cesarean delivery in 2010 was \$13,000 vs. \$8400 for a vaginal birth,² a difference of \$4600. Multiply \$4600 by 739,000, the excess number of cesareans, and we see that avoidable cesarean surgeries cost the health care system an extra \$3.4 billion dollars in 2010 alone. In addition, it costs an extra \$6450 for a vaginal birth in a hospital compared with a freestanding birth center, not counting the cost of anesthesia services, newborn care, and birth practitioner.¹ The cost of a home birth would be lower still. Imagine the savings if every low-risk woman planned to give birth in a low-technology environment attended by a midwife.

For more information on these issues, we recommend the following resources:

Declercq ER, Sakala C, Corry MP and Applebaum S. *New Mothers Speak Out: National Survey Results Highlight Women's Postpartum Experiences*. New York: Childbirth Connection, 2008.

Sakala C, Corry MP. *Evidence-Based Maternity Care: What It Is and What It Can Achieve*. New York: Milbank Memorial Fund, 2008.

Smith LB. *Impact of birthing practices on breastfeeding*. Sudbury, MA: Jones & Bartlett; 2009.

OUR MINI-REVIEW METHODS

Mini-reviews are more rigorous than narrative reviews, in which reviewers tend to cherry pick evidence to support their own theories and omit what does not.³ But, as we saw in chapter 2, any review process, including rigorous systematic reviews, is vulnerable to bias. Our solution is transparency. Bias cannot be eliminated completely, but we can make our point of view explicit, describe our methods, and explain our reasons for deviating from conventional review methodologies.

Transparency also allows us to retain the rigor of systematic reviews to determine prospectively search strategies and inclusion/exclusion criteria while at the same time allowing for individual judgment. Chapter 1 gives you our point of view, our biases, if you will, and our grounds for thinking the way we do, while here we describe our review methods and, where relevant, the reasoning behind them. In this way, we place all cards face up on the table, so that our readers can decide for themselves whether our arguments have merit.

One problem, however, could not be resolved because it is inherent to the obstetric research: we rarely know what the outcomes would have been had women received optimal care. For example, VBAC studies in hospitals report rates ranging from 61-72% in women with no prior vaginal births, but a study in freestanding birth centers reported an 81% rate, 9 to 20 more VBACs per 100 women. (See chapter 6.) Keep this caveat in mind as you read the mini-reviews.

Data Sources

We searched Pubmed and the Cochrane Database of Systematic Reviews using relevant keywords and MESH terms. We used the PubMed “related articles” function, the bibliographies of relevant studies, including narrative reviews, and hand-searched our personal files. When a potentially pertinent study was not readily available, we made reasonable attempts to obtain it by using interlibrary loans and querying colleagues.

Inclusion/Exclusion Criteria

We included relevant studies applicable to contemporary care in maternity care systems with adequate resources and where we had sufficient information to judge the quality of the research. Our exclusion criteria fell into two categories: absolute and relative. Absolute exclusions included the following:

- Studies published in languages other than English. The English-language abstracts that accompany some non-English-language studies do not provide sufficient information to judge study quality.
- Abstracts. Studies published only as abstracts or brief reports also do not provide sufficient information to judge study quality.
- Narrative reviews, commentaries, clinical guidelines. These are all opinion pieces and as such provide unacceptably weak evidence.
- Systematic reviews that include other systematic reviews. We felt that this removes us too far from the original sources and therefore the ability to evaluate them.
- Unpublished studies or those not published in peer-reviewed journals. While we may quarrel with the quality of many studies published in peer-reviewed journals, the peer review and editing process sets a minimum standard. Unfortunately, this meant excluding reports published by organizations or

agencies such as Childbirth Connection's excellent Listening to Mothers surveys (although we did cite data from these in chapter essays).

We also had relative exclusions—exclusions that do not necessarily apply under all circumstances. When we include studies with the following characteristics, we provide our rationale for inclusion, either in the text or in footnotes:

- Studies published before 1990. We chose a cutoff date, albeit an arbitrary one, to ensure that care in the study was reflective of current care. However, if a systematic review included studies published prior to 1990, we did not exclude it on that basis.
- Studies in low-resource maternity care systems. We excluded such studies except in rare cases where we lacked data from high resource maternity care systems and when the study's methods section established that care in that setting provided access to appropriately trained and knowledgeable clinicians and modern technology, or, alternatively, when lack of resources would not affect the outcome of interest.
- Studies reporting only surrogate outcomes. We excluded studies reporting only laboratory values or imaging results believed to be in the pathway to outcomes of clinical importance but not in themselves of clinical importance (Apgar scores, for example) or problems that may be part of normal recovery after birth (urinary incontinence limited to the first few months, for example).
- Studies included in included systematic reviews. Including these studies would duplicate results, thereby giving a misimpression of the strength of the data. Sometimes, however, a systematic review did not report on an outcome of interest. In those cases, we included the study for that outcome alone, but excluded it for remaining outcomes included in the review.

Studies qualifying for inclusion could also be excluded based on our appraisal of individual quality. Following our principle of transparency, we will list the reasons why a particular study was excluded, although, in the interest of length, that information will be posted on the book's website (www.optimalcareinchildbirth.com). Studies could be excluded from one mini-review but included in another, as it is possible for study weaknesses to affect reliability of some findings but not others, or because no well-conducted study of stronger design could be found for some particular outcome.

Finally, if well-designed randomized controlled trials (RCTs) or systematic reviews offered reliable evidence pertaining to an outcome or association of interest, we often excluded observational studies for that outcome. Sometimes, however, observational studies were included anyway because they added pertinent data that expanded or deepened our understanding of the issue at hand.

How We Present Statistics

Wherever possible, we present absolute differences (the arithmetical difference in occurrence rates) instead of or in addition to relative differences, such as relative risk (RR) and odds ratios (OR), because relative differences provide an incomplete and sometimes inflated picture of the effects of interventions. For example, tripling the odds or risk can mean going from 10% to 30% or from 1% to 3% or from 1 per 10,000 to 3 per 10,000. For this reason, the absolute likelihood of a particular outcome or the absolute difference in risk if the woman adopts a specific behavior or accepts a specific intervention is more useful for making clinical decisions. In some studies, the investigators reported absolute differences or the number needed to treat or harm, which are calculated from absolute differences, but where they did not, absolute differences could usually be calculated from study data, with one caveat: unlike odds or risk ratios, raw occurrence rates in observational studies* have not been adjusted for confounding or correlating factors. Still, adjustment usually has little effect on the ratio, so we felt that our calculation was a reasonable proxy, if not a strictly by-the-book statistic. In all cases, readers may assume that differences are statistically significant unless stated otherwise.

No review strategy is perfect, but we think we have developed one that serves the purpose of evaluating physiologic care better than medical-model strategies designed to evaluate treatment and prevention of disease. We hope that we have provided a model that can be further refined by those interested in what the research establishes as optimal care for the essentially healthy woman and baby.

REFERENCES

1. Average facility labor and birth charge by site and method of birth, United States, 2007-2009. 2011. (Accessed Jul 15, 2011, at <http://transform.childbirthconnection.org/resources/datacenter/chargeschart/>.)
2. Average maternity services payments, United States and other countries, 2010. 2010. (Accessed Feb 22, 2011, at <http://transform.childbirthconnection.org/wp-content/uploads/2011/03/international-maternity-payment-comparison-2010-3.pdf>.)
3. Griffiths P. Evidence informing practice: introducing the mini-review. *Br J Community Nurs* 2002;7(1):38-9.
4. Hamilton BE, Martin JA, Ventura SJ. Births: preliminary data for 2009. *Natl Vital Stat Rep* 2010;59(3):1-29.

* Data from RCTs does not need to be adjusted for possible confounders because participants have been allocated by chance.