Does Inducing Labor Increase the Likelihood of Cesarean Surgery?

By Henci Goer

For many years, the prime argument against liberal use of induction of labor has been that induction itself, as opposed to the indication for it, increases the likelihood of labor ending in cesarean surgery. More recently, however, a cadre of researchers has published studies concluding that the opposite is true, which if correct, would abolish the strongest rationale for restricting use of induction. Who is right? Let's take a look at the evidence.

The case that induction increases the likelihood of cesarean

The contention that induction increases the likelihood of cesarean surgery comes from studies comparing low-risk women undergoing elective induction at term with similar women with spontaneous labor onset. The restriction to induction for nonmedical reasons ensures isolation of the effect of the procedure.

Studies making this comparison consistently find more cesareans in women induced electively, even after adjustment for such factors as gestational age and birth weight. Fifteen out of 16 studies (1-16) report that first-time mothers roughly double their likelihood of cesarean with excesses ranging from three to 31 more women per 100. The wide variation in absolute differences largely depends on two factors:

- Cervical readiness for labor, a factor not overcome by cervical ripening agents (17, 18)
- Baseline cesarean rates, by which I mean that if the cesarean rate with spontaneous onset is say, 12% in a population of low-risk, first-time mothers, and induction doubles it, then 12 more women per 100 have a cesarean. If the rate with spontaneous labor onset is 24%, then doubling it means that 24 more induced women per 100 will have a cesarean. Four of the five studies of women with prior births and no prior cesareans (13, 17, 19-21) also find them to be at increased risk of labor ending in a cesarean, although absolute excesses are smaller — one to five more women per 100 in three of them (17, 20, 21) and 14 more per 100 in the fourth (13).

This finding makes biological sense. Initiating and continuing effective labor involves a complex cascade of feedback mechanisms that mutually reinforce and limit each other (22). It involves not just cervical changes, but changes in the uterine muscle, including proliferation of oxytocin receptors and the formation of gap junctions, which permit propagation of coordinated contractions. One would therefore expect simplistic attempts to overcome the natural timing and unfolding of the process to fail.

The case that induction decreases likelihood of cesarean

What, then, is the evidence that this conclusion is wrong and that induction is the better bet?

Induction proponents point to randomized controlled trials (RCTs) of induction prior to 42 weeks versus expectant management, as these are actually trials of elective induction. RCTs trump observational studies; that is, studies in which researchers don’t intervene but merely observe, analyze and interpret results, and participants are randomly allocated to one form of treatment or the other, which eliminates some sources of bias. Caughey et al. (23) conducted a systematic review, a “study of studies,” of elective induction versus expectant management that pooled cesarean rate data, called meta-analysis, from eight RCTs and found rates of 18% with induction versus 21% with expectant management.
Induction proponents also contend that the question isn’t whether women do better with induced or spontaneous labors, but whether women reaching term are better off with elective induction compared with women continuing pregnancy to some later date. They say this is the relevant question because expectant management will end in substantial numbers of women being induced later, have even higher cesarean rates as a result and that cesarean rates rise with gestational age even with spontaneous onset. Three studies making this comparison report lower cesarean rates with elective induction, amounting to three to five per 100 fewer first-time mothers and three per 100 fewer women with only vaginal prior births, and a fourth reports no differences in cesarean rates at 39 weeks or more.

Holes in the case that induction decreases the likelihood of cesarean

Those rebuttals seem compelling, but let’s take a closer look, turning first to the superiority of RCTs.

Hannah et al. can serve for all the RCTs in Caughey’s meta-analysis, both because the other trials share its weaknesses and because it is by far the biggest trial, which means that its results largely determine the meta-analysis’ results. Of the 6,054 participants overall in the eight RCTs, Hannah et al contributed 3,407, or more than half of them. Hannah et al reported that the cesarean rate was 21% among women allocated to induction versus 25% of women allocated to expectant management. This would appear to support the opposition’s position, but appearances can be deceiving.

One weakness is that the trial combines first-time mothers with women with prior vaginal births and no prior cesareans, which would diminish the effect of induction because such women are much less likely to have labor end in cesarean delivery. A secondary analysis that stratified results according to first or subsequent birth uncovers this masking effect: While the cesarean rate among induced women overall was 26%, it was 34% in women with no prior births but only 8% in women with them.

A much bigger weakness, though, is crossover. RCTs keep participants with their assigned group when analyzing results, because to do otherwise would negate the point of random assignment, which is to avoid bias. But when sizeable percentages of participants receive the treatment of the other group, it diminishes the differences between them potentially making it falsely appear that there is no difference or that results favor one side when in truth they favor the other. In the Hannah trial, one-third of the women allocated to expectant management were induced and one-third of the women allocated to induction began labor spontaneously before their scheduled induction. As we saw above, 25% of those assigned to expectant management versus 21% assigned to induction had cesareans, or four fewer induced women per 100 had cesareans when analyzed according to treatment allocation group (“intent to treat”). Quite a different picture emerges in the secondary analysis, which gives us the rates according to actual treatment: Among women actually having spontaneous onset of labor, 18% had cesareans versus 26% of women actually induced, or eight more women per 100 had cesareans in the actual induction group.

Now let’s look at the studies comparing term elective induction at each week with pregnancy continuing beyond that week. In the expectantly managed groups, women could begin labor spontaneously, be induced for cause or even undergo planned cesarean.

To begin with, three of the four studies using this design compare apples to oranges. All three isolate a group of women undergoing elective induction at term and compare them with expectantly managed women who are not as low-risk as women electively induced. In two of the studies, expectantly managed women could have hypertension, preeclampsia/eclampsia, or low or excessive amniotic fluid while the electively induced group were free of those conditions, the rationale being that previously healthy expectantly managed women might develop those complications after 39 completed weeks. The third study isn’t so specific, but it does say that women in the expectantly managed group could be induced for medical reasons or have planned cesareans. Small wonder, then, that cesarean rates were higher in expectantly managed women. The fourth study includes only low-risk women, and, as you might predict, it reports higher cesarean rates among induced women relative to expectantly managed.

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rates in electively induced women at every gestational week in both first-time mothers and women with prior births. The fourth study has its own problem, however. Investigators find no difference in cesarean rates after adjusting for BMI, age and use of epidural analgesia. (Unlike the U.S., epidural use was uncommon in women beginning labor spontaneously [18%] and much more common in induced women [35%].) If, as seems likely, being induced increases the odds of cesarean and healthy older and high BMI women are more likely to be induced, and if being induced is more painful and women are more likely to want an epidural, which also may increase their odds of cesarean, then this adjustment removes the effects of the very handicaps that induction imposes.

Furthermore, these studies are making the wrong comparison. Glantz (30) points out that comparing outcomes with induction in a given week with outcomes of all other women after that week is invalid. It biases in favor of induction by excluding women who labored spontaneously during that same week. The correct comparison is induction in a given week compared with expectant management of all other women at or beyond that week. Glantz analyzes a large dataset according to the two strategies and finds that while induction doesn’t increase likelihood of cesarean after adjustment for parity, risk and demographic factors using the “at or beyond that week” strategy, it does using the “at or beyond that week” strategy, amounting to one to two additional cesarean surgeries per 25 inductions. Investigators in two studies (25, 26) with the “after that week” design take up Glantz’s challenge and perform an “at or beyond that week” analysis as well. One study (24) finds that the results flip to favor expectant management in the “at or beyond that week” analysis; the other study (25) states that the “at or beyond that week” analysis didn’t alter results but doesn’t present the data, which, considering the importance of the question, is a concerning omission.

Finally, we have the elephant in the room: medical-model management. Several strands of evidence support that medical-model management confounds all these studies.
REFERENCES


