This study is a retrospective chart review of a convenience sample of school-age patients receiving occupational therapy in an outpatient clinic. As such, the data collected has significant limitations: not prospectively collected, convenience sample of children already receiving services for the identified concerns, thus subject to selection bias. These are serious limitations that are not laid out by the authors. The question posed by the authors is well-stated, but not as clearly defined as it could be. For example, groups are defined as LBW, Normal and HBW however, the standards used do not conform to uniformly recognized groups. 15.8% of the infants are listed as premature, but there is no indication how many of these were ELBW vs VLBW, vs. LPT (late preterm). This is important information because each of these groups carries their own specific health risks and have been identified as having associated developmental sequelae.

Health factors listed include the fact that a quarter of the sample (24.7%) spent some time in intensive care (presumably NICU although this is not specified). 5.8% had "birth injuries" but these also are not specified: HIE (hypoxic ischemic injury)? BPI (brachial plexus injury)? The reader can only guess. Feeding issues also are only grossly defined. Since the question relates to the connection between BW and obesity in childhood, it would be relevant to have specific information re the types of diabetes where diabetes is present, and whether the child was formula or breastfed, given breastmilk by bottle, and for how long. There is a dose-response relationship between the amount of breastmilk fed to premature infants, and their developmental outcome, including cognition, so this would be a relevant piece of information to report.

There also is a growing literature relating problems with executive function and externalizing behavior among infants born as late preterms, and yet these infants are not identified in the sample. Thus, the question being asked is not as simple as the authors have represented.

The means for developmental milestones show a wide standard deviation, which may imply the need for additional statistical measures to deal with this issue.

The discussion section is concerning because there are some statements that reflect a poor understanding of the newborn population. Stating that higher birthweight infants are less likely to experience assisted delivery is naive. In fact, infants who are LGA are more likely to have shoulder dystocia, brachial plexus injuries, and hypoxic injury. Infants born at ELBW, VLBW and LBW are likely to require assisted ventilation at delivery due to immature lung development, and
the distinction should be made between infants who are LBW but AGA, vs. SGA. Babies who are small for their gestational age are at higher risk than babies born early who are AGA. The suggestion that mothers of infants born at low birthweights may have had poor nutritional status ignores the myriad reasons for premature birth. Finally, the finding that infants with higher birthweights had earlier acquisition of certain skills may not hold up when the infants are grouped more specifically into appropriate weight groups. We know that infants born LGA are more likely to be born to women with diabetes (need to specify type) and that subgroup has additional risks for outcome.

1. Major Compulsory Revisions: I would expect the authors to revise the manuscript with, at a minimum, the birthweight categories specified according to currently accepted definitions for ELBW, VLBW, LBW, SGA, AGA, and LGA, and also specify more clearly the medical involvement of the 24.7% of their sample who spent time in an ICU - term vs. preterm. Of the 15.8 % who were premature, how many were in each of the above categories? Which of these infants were premature vs. term? The discussion section has some concerning limitations that should be corrected. The authors need to identify their population more specifically and then address the risks that present to each of those sub-groups. Considering the limitations of the convenience study sample, the most important of which are likely selection bias and the weaknesses attendant with retrospective reviews, I do not feel this paper represents a contribution to the field.

The authors should reframe their purpose in a more honest fashion. The sample should be described as convenience data, taken retrospectively and categorized from what appears to be their intake form at the clinic. The authors could then describe trends in health and developmental characteristics of children in their clinic population. Such a revision would still produce a manuscript of little interest to the broader medical/scientific community, but could be of interest, if published in a trade-publication, for other occupational therapy programs serving school-aged children.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.