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The syndromes of hydatidiform mole

To the Editors:

I must congratulate Drs. Szulman and Surti (AM. J. OBSTET. GYNECOL. 131:665, 1978) for their timely effort to dispel some of the confusion that surrounds this "awkward" entity of hydatidiform mole. The confusion has resulted largely from a lack of universally recognized diagnostic criteria and from an incomplete grasp of its nosology. The advent of cytogenetics and the recent recognition of partial moles have expanded our horizons but so far have failed to clear the air.

I agree with the authors that trophoblastic hyperplasia typifies the neoplastic mole and that early abortions, showing villous hydrops without trophoblastic overgrowth, should not be classified as moles. I am not clear about their views regarding "conceptuses with demonstrable embryo/fetus, variable pronounced hydatidiform changes, and focal mild to moderate trophoblastic hyperactivity." If depression of growth activity is fundamental in nonmolar heteroploid gestation, as shown biologically and pathologically,[1] the presence of "trophoblastic hyperactivity" is incompatible with such a diagnosis. I would like to suggest that all cystic placentas with trophoblastic hyperplasia, regardless of karyotype, should be classified as hydatidiform moles, i.e., neoplastic, especially as heteroploid moles are on record, as acknowledged by the authors. If trophoblastic hyperplasia, as stressed previously by Park,[2] is accepted as the crucial morphologic discriminant, pending validation or rejection in the future, it will allow consistency in diagnosis and comparability of reported series.

I was particularly interested in their unusual case of mole associated with 46,XX karyotype. They mention the karyotyped tissues as fetal but they make no mention of the sex of the fetus. I wonder if the possibility of maternal contamination was ruled out in this case.

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