Human Reproduction vol.13 no.10 pp.2973–2980, 1998

Letters to the Editor

Effects of agonist dose on in-vitro fertilization

Dear Sir,

Alvarez *et al.* (1997) have made a useful contribution to this field of research. However, there are possible alternative interpretations for their findings which we feel might be usefully considered. Their study appears flawed in that there are no data concerning concentrations of follicle stimulating hormone (FSH) on day 3 of the cycle, or on previous pregnancies and their outcomes — both features are important when assessing outcome variables. In our experience, we also find that obese patients require significantly longer duration to achieve down-regulation, using the same criteria as the authors, but this has no impact on pregnancy rate.

Finally, and most importantly, the chosen cut-off point of 13 days duration of gonadotrophin-releasing hormone (GnRHa) administration was not justified in their manuscript. We find this biased, since in their small sample shifting two patients from GII (longer duration) to GI (shorter duration) by choosing a cut-off of 14 or 15 days can lead to no significance in the pregnancy rate between the two groups. To demonstrate this statistically we carried out the following exercise: the reported pregnancy rates in the study for GI and GII were 44 and 20% respectively which is 9/27 and 12/45. Using the c^2 test gives $P = 0.027$. By increasing the cut-off point by 1 or 2 days the pregnancy rates may be: 12/43 and 9/29 (assuming no change in the number of the pregnant patients) which makes $P = 0.06$, (not significant). Furthermore, the authors did not indicate how often they measured the patients serum oestradiol to verify the concentrations <50 pg/ml. This has to be on a daily basis so that the cut-off point may be in days. However, every other day or weekly measurement of oestradiol does not give accurate duration in days. On the other hand, as the authors indicated in their study, the patients were seen in the first few days of their period for ultrasound scan and oestradiol measurement. Since their measurements were not on a specific day, inadvertent over-estimation of the days of GnRHa administration could not be ruled out. In conclusion, we feel that a prospective study is necessary to address these issues effectively. Care must be taken in over-interpreting data collected from small numbers.

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Alvarez, C., Cremades, N., Blasco, N. and Bernabeau, R. (1997) Influence of gonadotrophin releasing hormone agonist total dose in ovarian stimulation in the long down-regulation protocol for in-vitro fertilization. *Hum. Reprod.*, **12,** 2366–2369.

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William Ledger
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Dear Sir,

We appreciate the interest that Drs Lashen and Ledger have expressed in our recent paper (Alvarez *et al.*, 1998). In response to this comment we would like to clarity that patients were separated into two groups according to the number of days of agonist administration, and the_value of the median (13 days) was chosen as a cut-off point. When we chose other cut-off points (12 or 14 days), the pregnancy rate per cycle and per transfer was significantly lower in the patients group suppressed for a longer period (Neyro *et al.*, 1994) (Tables I and II).

Ovarian quiescence was verified for the first or second day of cycle with no follicular growth observed by trans-vaginal ultrasound and an oestradiol serum concentration of <50 pg/ml on the same day. Certainly, data concerning concentrations of basal follicle stimulating hormone (FSH) might have been considered (Olivennes *et al.*, 1996), but we lacked this information because it can only be measured in patients with previously cancelled cycles due to poor response. When we have high basal FSH concentrations, ovarian stimulation can be achieved using a short protocol. It is possible that patients who need a lower dose of gonadotrophin-releasing hormone (GnRH) agonist for suppression of ovarian activity, had a high basal FSH concentration, but this hypothesis cannot be confirmed with our data.

We think that although the sample size is relatively small it is still large enough to obtain significant differences. However, we plan to carry out a prospective study in future, in order to gain more conclusive results.

Table I. Pregnancy rate (cut-offpoint 12 days)

	Pregnancyrate/cycle (%)	Pregnancy rate/transfer (%)
GI ($<$12 days) ($n = 17$)	52.9	69.2
GII ($\geqslant$12 days) ($n = 55$)	21.8	27.9

n = cycle number.
Statistically significant differences were found between the groups ($P < 0.05$).

Table II. Pregnancy rate (cut-off point 14 days)

	Pregnancyrate/cycle (%)	Pregnancy rate/transfer (%)
GI ($<$14 days) ($n = 38$)	39.5	71.4
GII ($\geqslant$14 days) ($n = 34$)	17.6	24.0

n = cycle number.
Statistically significant differences were found between the groups ($P < 0.05$).

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Alvarez, C., Cremades, N., Blasco, N. and Bernabeau, R. (1997) Influence of gonadotrophin-releasing hormone agonist total dose in the ovarian stimulation in the long down-regulation protocol for in-vitro fertilization. *Hum. Reprod.*, **12,** 2366–2369.

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The secular decline in reported sperm counts

Dear Sir,

Handelsman (1997) draws attention to a most important point. All data on sperm counts derive from men who, in some sense, are volunteers. He cites his own work (Handelsman *et al*., 1985) reporting that volunteer sperm donors are more outgoing, intelligent, assertive, adventurous, extrovert and independent, than controls. In short, donors score more highly on a psychological dimension called 'sensation-seeking'. Handelsman speculates plausibly that this might have affected their sperm counts; and indeed he might have cited reports that sensation-seekers have higher testosterone concentrations than controls (Daitzman *et al*., 1978; Daitzman and Zuckerman, 1980).

The interpretation of the mean (or median) of any individual sample of sperm counts is problematic. Handelsman (1997) also illustrates this point with a roughly two-fold unexplained variation between such medians in data from his own research centre.

However, in an attempt to discredit the claim that there has been a real decline in sperm counts, Handelsman writes: 'It is notable that this large bias effect size within a single centre is comparable with the magnitude of the alleged effect size for the decrease in sperm concentration over 50 years according to the Carlsen meta-analysis'. It seems to me that Handelsman is failing to address the real problem here: that posed by the fact that when all the reported sperm counts are considered in time, there is a highly significant decline. Even when it is acknowledged that there is a large, and possibly unquantifiable, bias associated with each mean value (quite apart from the more tractable problems of standard error), that decline remains to be explained. It may be useful to summarize the possible forms of explanation: (i) it is possible that the bias itself has a secular trend: a downward bias might have increased, or an upward bias decreased across the years. The bias could be of volunteers, or of counting technique or via a decreased time interval since last ejaculation; (ii) it is possible that sperm counts in general are higher in some places than others, and that by chance, the places with high levels tended to report data before those with low levels; (iii) it is possible that in some places at some times, there was a real decline.

In the absence of persuasive evidence for either of the first two of these possibilities, one might hesitantly suppose that the third is correct. If it is correct, it is clear that the decline has not occurred contemporaneously worldwide.

Although some workers have failed to detect longitudinal declines within their own data (e.g. Handelsman, 1997) other have detected such a decline (Leto and Frensilli, 1981). These constitute the best evidence on the point. This is so, because any changes in practice (that might otherwise account for changes in bias) would be known to the authors and offered in explanation. The fact that, e.g. Leto and Frensilli (1981), could think of no other explanation seems to be strong evidence for a real decline – at least at some times and in some places.

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Dear Sir,

Dr James posits three alternative explanations for a non-phenomenon and concludes with an assertion that is either non-refutable in the Popperian sense or a *non sequitur.* In plentiful good company judging by any continuing debate devoid of valid data, he seems to miss a crucial point of my paper which, for the sake of brevity, is best summarized by the well-known dictum 'garbage in, garbage out'. Despite the bewitching allure of numbers on paper to the mathematically inclined, unfortunately the provenance of data sometimes makes all the difference in the world – in this case it certainly does. It is simply misguided to go on blithely analysing convenience samples of sperm counts from self-selected volunteers as if they were a valid, representative sample from their city of origin. It seems to me most improbable that valid samples representative of any city can be obtained, a conclusion virtually identical to that concluding a previous run of this same controversy 16 years ago (MacLeod and Wang, 1982). Finally, John Tukey's wise comment comes to mind '...The most important maxim... is this: far better an approximate answer to the right question, which is often vague, than an exact answer to the wrong question, which can always be made more precise'. In this case, regrettably perhaps, the right question is clearly unanswerable at present for lack of valid data. Consequently his alternative interpretations deflate even

before one can, in the absence of any empirical basis, argue the toss between his subjective preference for option 3 over mine for option 1. Perhaps Dr James could turn his fertile imagination to developing a strategy to get valid data rather than further overinterpreting invalid data.

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Gestation length and sex of child

Dear Sir,

In a recent letter, James (1996) makes the statement that 'short gestations are associated with male births'. He used in an earlier paper on this subject (James, 1994), data derived from US census data. However, these data are very imprecise; much better data on length of gestation are available (Koller, 1983) from a study of 6793 pregnancies in order to determine different risk factors. This study was conducted at almost 30 university and large city hospitals, the staff consisting of almost 100 university professors of gynaecology, paediatrics, human genetics and cytology. In judging the duration of gestation, the specialists made a careful analysis of the maturity of the newborn as well as his or her placenta, weight and colour of the amniotic fluid, etc. Thus of 6793 live births, only 5692 babies were considered mature at their births. The great majority of mature babies were born after 39–42 weeks of pregnancy; the majority of the 'unripe' babies had been born earlier. For these 'unripe babies',' 565 babies were male, and only 490 were female. Hence, the overall sex ratio was 115.3. On the other hand, of the 5589 'ripe' babies born after 38–41 weeks of gestation, 2883 were male, so that the sex ratio of mature babies was 106.6, i.e. the 'normal' German secondary sex ratio. The premature infants in the German study are more often male than female and this does not mean that normal male babies have a shorter gestation than females. There were 171 females born after more than 42 weeks of gestation, but only 161 male babies. Thus, there was not, as James (1994) had stated, a U-shaped curve when the sex ratio of births was plotted against duration of gestation.

Jongbloet *et al*. (1996) are correct in stating that the interval before a male birth is shorter than the interval before a female birth, something I had reported in 1958 and since confirmed by Renkonen and Lehtovaara (1962) and others. I attributed this to a stronger sex drive among high sex ratio progenitors which Rasmussen (unpublished, cited Bernstein, 1961) had shown to be a genetic trait. This finding also explains, at least partially, the finding by Jongbloet *et al*. (1996) that a decease in number of births means an increase in the secondary sex ratio. In the German data, the number of births per 1000 population decreased from 26.8 in 1914 to 13.9 in 1917, rising back to 25.8 by 1920 (Bernstein, 1948) with an opposite trend in the sex ratio. The decrease in the number of births meant mainly a decrease in births from the lower classes where men had no chance of becoming officers or be draft exempt because of a war essential occupation at the home front. During wartime, during epidemics and during economic hard times, the total number of births goes down (Bernstein, 1948; Jongbloet. *et al*., 1996), but the decrease in births takes place mostly in the lower social classes who are known to have a lower secondary sex ratio (Winston, 1931; Matthews, 1947) and this creates the correlation between number of births and the secondary sex ratio as observed by Jongbloet *et al*. (1996).

James comments on the observed sub-binomial distribution of sexes in animal litters as being due to the variation in maternal hormone levels during conceptions and stated that 'I know no plausible alternative explanation'. However, research by Boklage (1985) provides another explanation. He found an interaction between opposite dizygotic fetuses which cuts down prenatal as well as postnatal deaths. If this protection of prenatal deaths in humans also exists in animals, that may explain the preponderance of male–female fetuses observed in litters.

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Dear Sir,

Thank you for the opportunity to comment on Dr Bernstein's letter. The first paragraph seems merely to confirm what I wrote. The reported mean gestation (time between last menstrual period and delivery) of boys is of the order of 1 day less than that of girls. The data I cited were from US Vital Statistics (not census). The regression of sex ratio on reported duration of gestation is ↘-shaped every year.

In the second paragraph, Dr Bernstein has only a small data set (in contrast with the annual US births). Her data are

consistent with (though admittedly give no support to) the contention that there is an upward turn on the right side of the regression. However, if Dr Bernstein wishes to deny the existence of the right arm of the regression, then she needs to provide grounds for supposing bias in the US data (because that right arm is present every year).

The explanation in the third paragraph is a possible interpretation, but for it to be plausible, evidence should be adduced that lower class men actually were away at the front getting killed, while upper class men were at home, living it up. (I am aware of contentions of this sort in regard to the US forces in Vietnam: but the high wartime sex ratios which it is trying to explain occurred in all the belligerent countries in both world wars). Moreover, the suggestion that lower class births are associated with a low sex ratio is itself contentious.

With regard to paragraph 4, I acknowledge that Boklage provides a possible explanation of the sub-binomial distribution of the combinations of the sexes in mammalian litters. However, as far as I know, there has been no confirmation of his work in other species (or ours).

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Spermatid conception

Dear Sir,

With respect to the paper by Sousa *et al.* (1998) in the February 1998 issue of *Human Reproduction*, we carefully reviewed the photographs showing 'round spermatid' injection under Hoffman optics. We do not feel that these cells can be verified to be round spermatids under Hoffman optics. There are simply no clearly defineable features under Hoffman optics that can identify those round cells as spermatids.

Please see Figure 1 of this letter in which the arrows point to a Sertoli cell nucleus (seen under Hoffman optics in patients with Sertoli cell-only), which is typically misinterpreted as a round spermatid. See also Figure 2 which is a phase contrast view of a testicular sperm extraction (TESE) specimen which has mature spermatozoa, Sertoli cell nuclei, and round spermatids present. Note the arrows point to the round spermatids, reliably apparent by the acrosomal vesicle. Phase contrast microscopy allows a more reliable identification of cell types at TESE–intracytoplasmic sperm injection (ICSI) than Hoffman optics.

Furthermore, a careful review of all our light microscopy stained slides of maturation arrest, Sertoli cell-only, and normal spermatogenesis patients has revealed no round spermatids when there were no mature sperm, or spermatids with tails. These results were then compared with a phase contrast study of TESE specimens in all of our patients with azoospermia. Under phase contrast, one again could reliably identify round spermatids, but they were never found in the absence of mature spermatozoa. This observation confirms the observations of

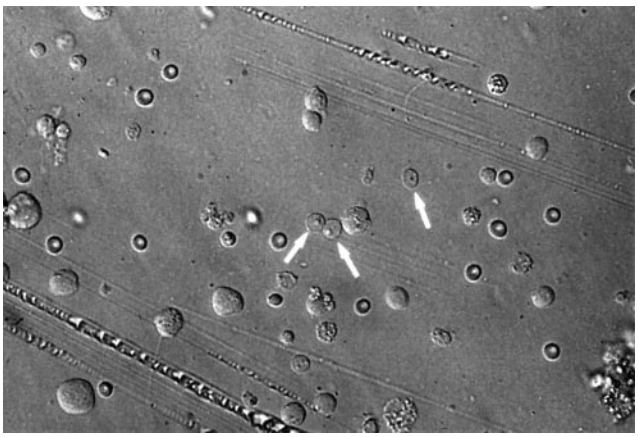

Figure 1. Sertoli cell-only viewed under Hoffman optics. Arrows point to sertoli cell nucleus, not a round spermatid.

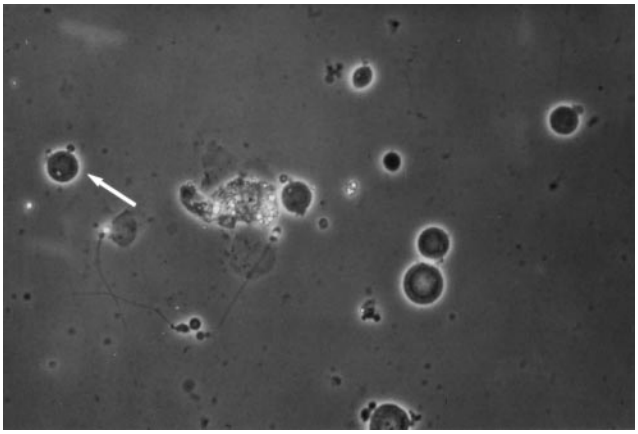

Figure 2. Phase contrast view of testicular sperm extraction (TESE) specimen which has mature spermatozoa and round spermatids. Arrow points to round spermatids.

Söderstrom and Suominen (1980) which state clearly: 'In meiotic arrest, the spermatogenic cell differentiation process seems to proceed normally up to the late pachytene or diplotene stages of meiotic prophase. However, no spermatids can be seen in the tubules . . . the site of meiotic arrest was always very constant in the late meiotic prophase and did not vary even between different patients.'

This also confirms the observations of Verheyen *et al.* (1998) in which no round spermatids were observed in the absence of spermatozoa or mature spermatids. Therefore, we think it is time to seriously reconsider whether round spermatid nuclear injection (ROSNI) and round spermatid injection (ROSI) are solutions to non-obstructive azoospermia when no spermatozoa or mature spermatids can be identified in the TESE specimens (Silber and Johnson, 1998).

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Dear Sir,

Round spermatid injection (ROSI) into oocytes is a recently developed technique of assisted reproduction that can be used for men who fail to produce spermatozoa; using this technique, several human births have been reported (Tesarik *et al.*, 1995, 1996; Vanderzwalmen *et al.*, 1997; Barak *et al.*, 1998). On the other hand, with the current state of the art, the risk of ROSI failure remains high (Amer *et al.*, 1997; Vanderzwalmen *et al.*, 1997).

The Silber *et al.* article commenting on our opinion article on current problems with spermatid conception (Sousa *et al.*, 1998), is actually a mere repetition of arguments that have been refuted in another recent paper (Tesarik *et al.*, 1998a). These arguments are biased by two major flaws, concerning methodology and interpretation respectively.

The first flaw concerns methodology and relates to the author's unjustified confidence in the value of a simple microscopical observation of native preparations of testicular cells. It appears that Silber *et al.* are ready to give the label of spermatid only to those cells in which an acrosomal vesicle is visible at that level of observation. Here, of course, they are wrong, because the acrosomal vesicle can be observed only during a limited time period of round spermatid development. As a matter of fact, round spermatids from many patients suffering from complete spermiogenesis failure remain arrested at the Golgi phase of acrosomal development. It is well-known that no acrosomal vesicle can be observed in round spermatids at this stage. This was the reason why, in the original detailed description of the ROSI technique (Tesarik and Mendoza, 1996), we only mentioned the presence of the acrosomal vesicle as one of the characteristics of round spermatids to be detected, certainly not as the decisive one. Notwithstanding, human round spermatids can be identified in the native state by simply respecting the criteria of cell size (approximately that of red blood cells, that usually are numerous in testicular biopsy samples) and by detecting the presence of a round nucleus surrounded by a rim of cytoplasm (distinguishing round spermatids from small lymphocytes, in which the outline of the nucleus cannot be seen) (Tesarik and Mendoza, 1996).

The application of optical systems facilitating the recognition of the acrosomal vesicle, such as the use of DDL phase contrast, is thus only of relative value. The spermatid nucleus and the acrosomal vesicle can be recognized by an experienced worker even with the use of standard Hoffman-contrast optical systems that are currently used in laboratories performing micromanipulation-assisted fertilization. This is demonstrated in Figure 1 of our recent paper (Tesarik *et al.*, 1998a) or in Figure 2 of another recent publication (Vanderzwalmen *et al.*, 1998) in which both structures are clearly visible. Unfortunately, the resolving power of figures in our previous paper (Sousa *et al.*, 1998) has been partly lost during the conversion of the original colour prints to halftones. Even so, only a very inexperienced worker might be able to confuse these cells with Sertoli cell nuclei, simply because the latter are considerably larger than round spermatids, as discussed previously (Tesarik *et al.*, 1998a). Confusion between a Sertoli cell nucleus and a round spermatid is thus definitely no serious obstacle of ROSI.

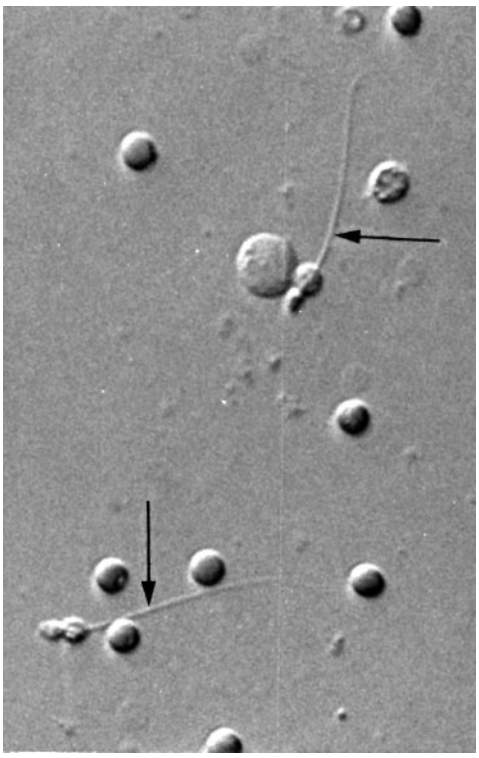

Figure 1. Spermatids still retaining a round-shaped cell body but showing a protruding, partly condensed nucleus and a clearly visible flagellum (arrows). These spermatids have developed, during 48 h of in-vitro culture, from originally round spermatids recovered from a patient with complete spermiogenesis failure. Reproduced from Tesarik *et al.* (1998b) with the permission of Communications Media for Education.

The tendency for putting too much stress on the detection of the acrosomal vesicle is likely to be at the origin of the inability of some workers, including Silber *et al.* to identify Golgi-phase round spermatids in the absence of elongated spermatids or spermatozoa in testicular biopsy samples. Interestingly, in the interpretation of Silber *et al.*, this methodological shortcoming has been at the origin of a 'theory' that is being defended by the first author for a couple of years against solid arguments showing the contrary, including results from laboratories using advanced techniques of germ cell recognition, such as fluorescent in-situ hybridization (FISH) and

Table I. Some experimental conditions interfering with spermiognesis in animal models. [Updated version of Table taken from Tesarik *et al.* (1998b). Reproduced with the permission of Communications Media for Education].

Species	Molecular pathology	Target cells	Reference
Rat	Testosterone deficiency	Sertoli cells	O'Donnell *et al.* (1996)
Rat, mouse	Vitamin A deficiency	Sertoli cells, spermatids	Eskild and Hansson (1994)
Rat	RARa mutation	Sertoli cells, spermatids	Akmal *et al.* (1997)
Mouse	RXRb mutation	Sertoli cells	Kastner *et al.* (1996)
Mouse	HR6B mutation	Spermatids	Roest *et al.* (1996)
Mouse	CREM mutation	Spermatids	Nantel *et al.* (1996) Blendy *et al.* (1996)

immunocytochemistry with germline-specific markers (Mendoza *et al.*, 1996).

In addition to contradicting these empirical findings, the reasoning of Silber *et al.* also goes against the current understanding of molecular biology data obtained in animal models of human spermatogenic arrest. Maturation arrest at the round spermatid stage can be produced experimentally by deficiency of hormones and other regulatory factors, as well as by targeted mutation of genes for receptors, elements of signal transduction pathways or intracellular repair systems (Table I). The idea that similar molecular defects can occur spontaneously in humans (Tesarik *et al.*, 1998b) and lead to a similar pathological picture, referred to as complete spermiogenesis failure (Amer *et al.*, 1997), is supported by the recent detection of spontaneous mutations of the CREM gene (Table I) in some men with maturation arrest at the round spermatid stage (Weinbauer *et al.*, 1998).

Instead of pursuing vain speculations, similar to those of Silber *et al.*, workers in the field of assisted reproduction should be encouraged to apply methods for the evaluation of the biological quality of round spermatids from patients with spermiogenesis arrest before the inclusion of each patient in a ROSI programme. Recent findings show that patients with complete spermiogenesis failure have an unusually high frequency of round spermatids undergoing apoptosis and consequent DNA degradation (Tesarik *et al.*, 1998c). Because cells at early stages of apoptosis cannot be distinguished from healthy cells at the time of ROSI, neither can they be recognized by conventional viability tests, the risk of injecting oocytes with round spermatids carrying irreparable DNA damage is high in these cases. This can explain the low implantation rates after the transfer of embryos resulting from ROSI in cases of complete spermiogenesis failure (Amer *et al.*, 1997; Vanderzwalmen *et al.*, 1997). The development of methods enabling the selection of non-apoptotic spermatids for assisted reproduction is thus a major challenge for the future use of spermatid conception. This will hopefully be possible by in-vitro culture of human male germ cells. Preliminary results (Tesarik *et al.*, 1998b) show that, under appropriate culture conditions, round spermatids from some patients with complete spermiogenesis failure can resume spermiogenesis *in vitro* and develop into abnormal elongated spermatid forms (Figure 1).

In the light of these new developments, the current debate about the possibility of finding round spermatids in the absence of spermatozoa would appear anecdotal if there were not a real risk of irreparable damage to infertile couples because of incorrect evaluation of testicular biopsy samples. It should be realized that testicular biopsy is not a zero-risk procedure (Schlegel and Su, 1997) and that excessive useless testicular tissue sampling, motivated by the inability to recognize round spermatids that are present in the sampled tissue contradicts the basic medical principle of *primum non nocere.*

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Difficulties in distinguishing between a mature spermatid and a testicular spermatozoon

Dear Sir,

We refer to the article by Kahraman *et al*. (1998). In this paper are photographs which the authors describe as a testicular spermatozoon (Figure 1) a round spermatid (Figure 2) and a mature spermatid (Figure 1). The photograph that the authors classify as a mature spermatid could, in our opinion, be a testicular spermatozoon. It is suggested that the difference between the two is that the spermatid has a residual body around the midpiece and a shorter tail, although the authors say that it is hard to distinguish between a spermatid and a spermatozoon.

To our understanding the residual body of the spermatid described in the text, but which we could not detect in the photograph, could be the cytoplasmic droplet of a testicular spermatozoon. This is part of the normal morphology of a testicular spermatozoon because, although spermiogenesis is complete, it has not yet begun its journey through the epididymis during which this remnant droplet of cytoplasm will be discarded.

Our concern is that if the authors are correct in their description of a spermatid, it will be extremely difficult to distinguish between this stage of spermatid and immature spermatozoa and those performing intracytoplasmic sperm injection (ICSI) with intracytoplasmic sperm extraction (TESE) could unintentionally inject oocytes with such elongated spermatids against the ruling of the Human Fertilisation and Embryology Authority.

We would be interested in other readers' opinions on this important issue.

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Sheena E.M.Lewis and Neil McClure

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Dear Sir,

I would like to take the opportunity to reply to Drs Lewis and McClure. With regard to the residual body of a mature spermatid and cytoplasmic droplet of a mature spermatozoon: residual cytoplasm is localized around the head and neck region of a mature spermatid which is still trapped by Sertoli cells. The cytoplasm of this cell is discarded during the epididymal transport. The residual bodies lie in the apical cytoplasm of a Sertoli cell and they have a relatively dense cytoplasm which contains numerous organelles (Kingsley-Smith and Lacy, 1959; Dietert, 1966; Sapfold *et al.*, 1969; Phillips, 1974): mitochondria in groups, vesicles, fenestrated membranes, ribosomes in large complexes and lipid droplets. A small residual cytoplasmic droplet remains attached to the neck region of testicular spermatozoa during the release from the seminiferous epithelium into the tabule lumen. As these cells undergo further maturation during epididymal transport, this cytoplasmic droplet migrates along the tail and finally disappears.

It is usually difficult to discriminate a mature spermatid from a mature spermatozoon. The only difference is really a technical one; a spermatid is still embedded in the Sertoli cell and a spermatozoon has been released from the Sertoli cell (Silber and Lenohan, 1995). Figure 1 shows the residual body of a mature spermatid around the sperm head and neck region (big arrow) and is still embedded in the Sertoli cells (small arrows). Mature spermatids surrounded by a residual body around the head and midpiece are generally observed to be immotile. They rarely have a twiching tail motility between the Sertoli cells. However, the discrimination criteria can be complicated.

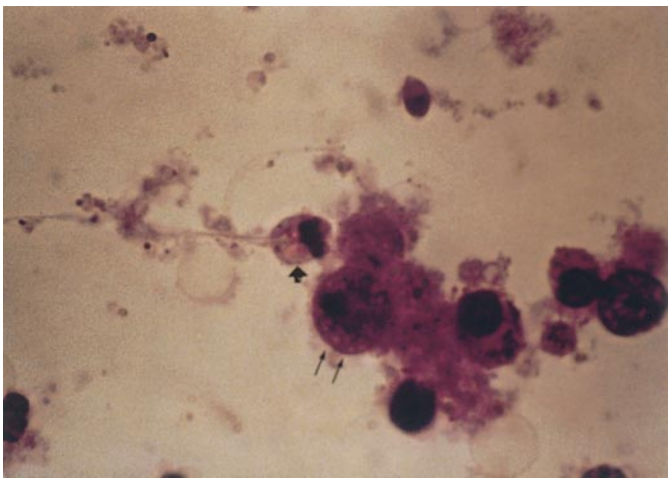

Figure 1. A residual body of a mature spermatid around the sperm head and neck region (large arrow) which is still embedded with the Sertoli cells (small arrows).

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Erratum

Statutory information for the children born of oocyte donation in the UK: what will they be told in 2008?

by **H.Abdalla, F.Shenfield and E.Latarche**

Hum. Reprod., **13**, 1106–1109, 1998

The following errors were published in the above article.

Page 1107, last line, the number 71 should read 75.

Page 1109, the 3rd paragraph should be replaced with:

Nevertheless, 10% of those in higher education responded as opposed to 5-6% of those with lesser education. Single women responded with the same rate as married and divorced women. Only two out of 182 (1.1%) of known donors responded to the request for a pen portrait, an expectable result if a priory anonymity is not part of the donation/recipients equation. This was, however, significantly lower than anonymous donors where 8.9% (36/403) responded to this question. $\chi^2 = 12.66$, df $= 1$, $P < 0.003$.

Page 1109, 4th paragraph, 84% should read 94% and the table reference should be Table III.