dark cavities with restricted apertures in carbonate substrates such as pitted and bored dead coral heads (*Montastrea*, *Diploria*), fused heads of dead *Porites porites*, perforated encrusting coralline algae, and dead, broken conch shells. The body of the anemone is always concealed within the cavity, often to depths of several centimeters. During the day large lobate extensions of the uppermost part of the column [called pseudotentacles by Hyman (*3*), in the related *L. danae*] project from the cavity. As ambient light decreases in the late afternoon, the pseudotentacles slowly retract, and the true tentacles are extended. In the morning the tentacles are withdrawn and the pseudotentacles reextended. This behavior pattern persists in outdoor laboratory aquariums.

The behavior is apparently a direct response to light conditions and is not linked to a circadian system. In order to determine this, animals were maintained in running seawater in laboratory tanks under controlled light conditions for periods of 24 hours. Those animals that were maintained in the light (250 foot-candles; 1 foot-candle = 1.1 lu/m^2) for 24 hours remained with expanded pseudotentacles and retracted tentacles for the duration of the experiment. Those maintained in the dark for 24 hours exhibited the opposite condition.

The response was a graded one, its magnitude depending on the magnitude of the stimulus (light intensity). In order to quantify the response, the following experiment was performed on 15 animals. Each anemone was set in a small hole drilled in a cut coral slab; it was allowed a day to recover and was then subjected to a series of light intensities from incandescent bulbs increasing from darkness to 1 foot-candle and then by steps of quadrupled intensity up to 500 foot-candles. The animals were left at each light intensity for 1 hour and photographed; then the intensity was increased. The resulting photographic negatives were projected at a constant scale and traced; measurements were then made of the degree of extension of the tentacles and pseudotentacles at each intensity. The results (Fig. 2) indicate that the threshold intensity was less than 1 foot-candle. The pseudotentacles extend and swell with increasing light, reaching maximum extension at about 500 foot-candles. The tentacles are partly extended in the dark, but reach maximum extension at about 4 foot-candles (Figs. 1 and 2), which is the light intensity in their native habitat just after sunset and just before sunrise. Pseudotentacles of animals subjected to decreasing light intensities tend to remain slightly more extended at lower intensities than they did at the same intensities during the increase in light. An additional response of the tentacles of *L. coralligens* which should be noted here is that when the attachment of the basal disk to the substrate is broken, regardless of light intensity, the tentacles are greatly extended (Fig. 2).

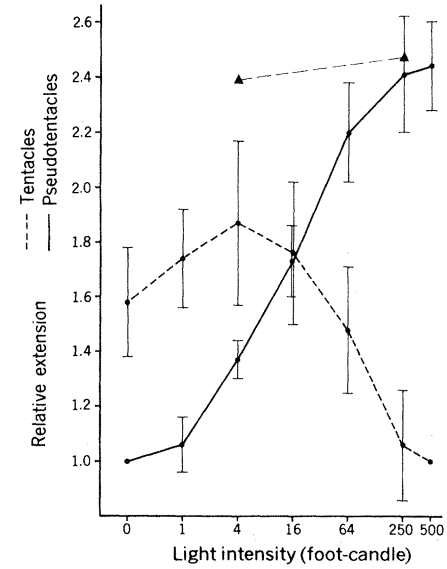

Fig. 2. Relative degree of extension of tentacles and pseudotentacles (ordinate) under a regime of periodically increased light intensities (abscissa); $n = 15$, confidence limits for $P = .10$. Calculated from measurements made on photographic negatives projected at a constant scale. For each animal, the 20 longest tentacles were measured at each interval; the same pseudotentacles were followed in each animal; a minimum of four animals (up to six when possible) was used. The width of the confidence limit markers indicates differences in the amplitudes of extension of different animals, not differences in the basic trends of the curves. The two triangles above indicate the condition of the tentacles (20 longest per animal; $n = 5$) shortly after contact between the pedal disk and the substrate is broken.

There is a real division of labor between these two tentacle-like structures in *L. coralligens*. The diurnally expanded pseudotentacles are packed with zooxanthellae in average densities of about 10,000 ± 1200/mm^2 (mean ± S.D.), whereas the extended tentacles have densities of about 3000 ± 600/mm^2. The surface area of an extended pseudotentacle is six to ten times greater than that of an extended tentacle (Fig. 1), resulting in total zooxanthellar counts being roughly 20 to 30 times greater in a pseudotentacle. It is possible that the anemone is deriving some nutritive benefit from zooxanthellar photosynthesis, as has been demonstrated, for example, in the anemone *Anthopleura elegantissima* (*4*). It has been shown by Pearse (*2, 5*) that *A. elegantissima* without zooxanthellae appear indifferent to light, while those with the symbionts show a variety of responses to different light conditions. Perhaps the greater sensitivity and reliability of the pseudotentacle response in *L. coralligens* is related to the much higher numbers of zooxanthellae in these structures.

Although topographically adjacent, the tentacles and pseudotentacles represent two different body regions (capitulum and column, respectively) which may not be in direct (nervous) communication. The column of the anemone *Calliactis* has been shown to have three separate conducting systems (*6*). Thus it may be that the opposite responses shown by tentacles and pseudotentacles to changes in light intensity are mediated by separate and largely isolated conducting systems. The graded nature of the response is typical of many coelenterate responses (*7*).

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Local-Regional Anesthesia During Childbirth and Newborn Behavior

We are deeply concerned with the possible response of the lay public to the conclusions of the Standley *et al.* report (*1*) on the effect of local-regional anesthesia during childbirth on newborn behavior. The authors are not anesthesiologists and appear to be unfamiliar with the mechanism of action of obstetric anesthesia. Most important, they conclude that there may be a local anesthetic drug effect in neonates whose mothers received spinal analgesia. However, the amount of local anesthetic drug used in spinal block is so small that placental transfer has not been detected. According to Greene (*2*), "... spinal anesthesia has no direct effect on the fetus. This constitutes one of its main advantages over general anesthesia and other forms of major conduction anesthesia (e.g., lumbar peridural) which may result in passage of anesthetic agent across the placenta with subsequent depression of the fetus." Second, the authors failed to differentiate between the various local anesthetic drugs used. Tetracaine, an ester, is hydrolyzed by plasma cholinesterases in both maternal and fetal bloods, while the other three drugs, being amides, are metabolized by hepatic microsomal enzymes at a rather slow rate. However, the amount of bupivacaine transmitted across the human placenta is approximately one-half that of lidocaine or mepivacaine. Consequently, Scanlon and co-workers (*3*) did not find decreased neonatal neurobehavioral parameters after epidural block with bupivacaine in contrast to lidocaine or mepivacaine. Third, the mode of delivery was not considered in the analysis of the data. Forceps delivery was used in 39 cases, most likely all under major conduction anesthesia, whereas the eight women without analgesia or anesthesia most probably had spontaneous deliveries.

In addition, a control group of only seven patients is rather small, especially when the coefficient of variation is as high as 30 to 40 percent. It is also misleading to use parametric statistics to describe and analyze measurements that are on a ranking scale. In the "no anesthesia, no analgesia" group, taking $\pm$ 2 standard deviations gives a range of 20 to 58 around the alertness mean, a range of 0.5 to 26 around the irritability mean, and a range of 0.3 to 29 around the motor maturity mean. Such data cannot be on a Gaussian curve. Finally, it is difficult to understand why anesthesia plus high dosage of analgesia would have less effect than anesthesia alone on such parameters as alertness and irritability.

Studies of this type have far-reaching consequences. Therefore, they must be undertaken under the best possible circumstances. This necessitates (i) prospective investigations without the variability of the present study and (ii) a team approach including obstetrician and anesthesiologist.

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Hodgkinson *et al.* are puzzled by our results because there is no biochemical explanation for them. With regard to their questions about our statistical analyses, they say first that the comparison group of seven infants is too small given the large standard deviations. We, too, wish that this group was larger; however, the small sample size works against finding significant differences because it reduces the power of the statistical tests (*1*). Their second point is that the data do not come from a Gaussian curve and therefore do not meet the assumptions of a *t*-test. In fact, the *t*-test is quite robust with respect to departures from normality (*2*). Nonetheless, the three comparisons between the no drug group and the anesthesia, no analgesia group were recalculated using the Mann-Whitney U test. Because any nonparametric test is not as powerful as the corresponding parametric test, it was expected that the differences would not be as significant as with the *t*-test. This is the case: for the Brazelton scale cluster of alertness, $U = 32$, $P > .10$; for irritability, $U = 18.5$, $.05 < P < .10$; and for motor maturity, $U = 17.5$, $P < .02$, which clearly differentiates between the two groups. The final point concerns the lower mean score on alertness and higher mean score on irritability in the anesthesia, high analgesia group than in the anesthesia only group. These are not statistically significant differences; therefore, the probability is exactly 50 percent that the means would be in the order they are rather than the reverse.

We agree that such findings are perplexing, since there is no physiologic evidence that tetracaine introduced in spinal block persists in infant circulation. On the other hand, it was some time before the possible hazards of the amide-type agents were recognized: Marx wrote in 1961 that "... regional block techniques ... afford optimal conditions for the newborn"(*3*). As research in this area advances, anesthesiologists and psychologists alike are learning more about the many factors which affect the newborn infant. The critics' notes on distinctions between actions of amide and ester agents are not germane here, however, since the infants of the 20 percent of anesthetized women who received other than spinal block did not differ from the spinal block group on any measure. The findings are very similar if the infants of women administered tetracaine in spinal block technique only are compared with infants born to nonanesthetized women. Similarly, as we reported, use of forceps did not differentiate among infants in the anesthetized group or in the subsample of those whose mothers received spinal block anesthesia.

We must therefore conclude that the data and analyses are valid, but we are left with intriguing questions concerning their interpretation. Are the correlations between obstetric medications and infant status spurious relationships that reflect more direct influences such as stresses of childbirth (including perhaps a relative inefficiency of labor following anesthesia administration) or the psychological state of the mother? One suggestion is that maternal posture during childbirth affects maternal venous tone and thereby fetal status (*4*). Another study relating attitudes during pregnancy and labor medication has suggested that an enduring maternal disposition is expressed during pregnancy and in the need for obstetric pain relief (*5*). These findings are supported by subsequent analyses on our data (*6*) which indicate several prenatal and perinatal antecedents of infant outcome. The research effort is a significant one and requires the cooperation and attention of all researchers and clinicians concerned with providing the optimal childbirth experience for infants and parents.

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