Amblyopia

Why is the adult amblyopic eye unstable?

C S Hoyt

Surely, understanding the mechanisms that account for improvement and ''slippage'' in the acuity of the adult amblyopic eye should be more thoroughly studied

In 1981, David Hubel and Torsten Wiesel were awarded the Nobel Prize in Physiology or Medicine. Their elegant animal models of various deprivation studies in infancy have provided invaluable information to practising ophthalmologists.[1 2] Hubel and Wiesel described the potentially irreversible anatomical consequences of early monocular occlusion.[3] The profound and specific cellular loss that occurred in layer IVc of the visual cortex and the laminae of the lateral geniculate nucleus subserving the deprived eye was interpreted as a model of form deprivation amblyopia.[4] The fact that permanent cellular changes occurred within weeks of initiating occlusion (eyelid closure) in an infant animal prompted ophthalmologists to consider early surgery in the management of congenital cataracts.[5 6] That the strategy was successful in some cases of monocular congenital cataracts was seen as clinical validation of Hubel and Wiesel's experimental studies.[6 7] Many of us could imagine that if understanding the neural anatomical alterations resulting from experimental visual deprivation states resulted in improved visual outcomes in infants with cataracts surely neuropharmacological studies would be even more enlightening and might provide ''medical'' therapies to replace the traditional patching therapy for amblyopia.

The subsequent years have proved how elusive a complete understanding of amblyopia is. Human necropsy studies of anisometropic and strabismic amblyopia have demonstrated that layer IVc of the visual cortex does not appear to be adversely affected in these types of amblyopia.[8 9] The early maturation of layer IVc makes it an unlikely site for changes caused by any type of unequal visual inputs other than occlusion.[8] Appropriate models of anisometropic and strabismic amblyopia to define the neuroanatomical consequences of these types of amblyopia remain unavailable. Moreover, early but careful studies to treat amblyopia in children with pharmacological agents have been thought provoking but disappointing.[10] Yet, in a broader sense we must ask the question—can neuroanatomical and neuropharmacological studies address all of the clinical behaviours of the amblyopic eye? For example, it is now recognised that patients with strabismic amblyopia may show a ''slippage'' of visual acuity in the amblyopic eye well after the so called ''sensitive'' age (6–8 years of age).[11] This acuity loss can be reversed with occlusion of the non-amblyopic eye even in adults, although it may subsequently ''slip'' again with cessation of occlusion therapy. Surely this phenomenon of unstable visual acuity in the older strabismic amblyope is not likely to be explained by anatomical changes in the brain stem or visual cortex.

Equally puzzling is the ability of some strabismic or anisometropic amblyopes to show spontaneous improvement in the amblyopic eye if injury or disease reduces the acuity of the non-amblyopic eye. Although this has been the subject of numerous case reports there are now two good epidemiological studies that agree that this is not a rare occurrence.[12 13] Rahi and coworkers, in the United Kingdom, reported that 10% of adult amblyopes who suffered visual loss in the non-amblyopic eye exhibited spontaneous visual improvement in the amblyopic eye.[12] In this issue of the *BJO* (p 1119) Chua and Mitchell report their findings from the Blue Mountains Eye Study in Australia. Their findings are strikingly similar to those of Rahi and coworkers. In all, 9.1% of adults with amblyopia showed significant improvement in the amblyopic eye after a two line or more visual loss in the non-amblyopic eye. The underlying inhibitory influence of the non-amblyopic eye on the amblyopic eye that apparently counts for this is undefined. However, there may be more than one potential mechanism responsible. Although most, but not all, forms of amblyopia are uniocular it is an important disability. Chua and Mitchell also point out in their study that amblyopes have an incident 5 year visual impairment risk in the non-amblyopic eye of 33%. This compares with the 12.5% risk in non-amblyopes. Unsuccessfully detected or treated amblyopia does have its consequences. Early detection of amblyopia remains a practical goal. Appropriate treatment of children with amblyopia is effective.[14] Yet, surely understanding the mechanisms that account for improvement and ''slippage'' in the acuity of the adult amblyopic eye should be more thoroughly studied. Newer, more effective treatments of amblyopia may be the result of such studies.

We are indebted to Hubel and Wiesel for their pioneer modelling of visual deprivation states. Yet, the pathophysiology of amblyopia remains to be fully delineated. The dream of direct pharmacological treatments for amblyopia (not penalisation of the non-amblyopic eye) remains largely a dream. The mysterious ways of the adult amblyopic eye merit better study. Better treatment of amblyopia could be the result.

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BJO style

Who is Ivan Schwab?

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Author qualifications

We live in the age of biography. They fill and spill over the shelves of bookstores. They have become gigantic in length (example, Lord Black's new biography of Franklin Delano Roosevelt is 1278 pages in length).[1] It is perhaps, therefore, not surprising that Papalkar and Francis in their letter to the journal this month ask, ''Who is Ivan Schwab?''[2] Actually, if we understand them correctly they are really asking, ''What are Ivan Schwab's qualifications to write about interesting aspects of the eye in various animal species?'' The fact that Ivan Schwab is a renowned corneal surgeon with a side interest in the eye in evolution is interesting. That he plans a sabbatical in the homeland of Papalkar and Francis to study with a marine biologist is perhaps even more noteworthy. Yet, is he qualified to write on these topics? He does not have a PhD in zoology, animal behaviour, etc. As with most American physicians he has only a single degree, an MD. Is any of this important? Are the covers of the journal less interesting because of Schwab's amateur's status? Are his descriptions any less provocative because he lacks validation with a university degree in zoology, etc?

The question Papalkar and Francis ask—who is qualified to contribute to the journal?—is an important one. Yet, we strongly disagree with their conclusions. We would assert that qualifications necessary to contribute to the scientific literature include intellectual curiosity, knowledge of appropriate study design, and most important—honesty. Listing authors' degrees and academic appointments seems unlikely to address the question of whether they are qualified to write on a specific topic. Reviewers and readers alike will come to their own conclusions after carefully reading the paper in question. Or to make the point another way—did Linus Pauling's multiple degrees and two Nobel prizes make him qualified to wax on about the wonders of vitamin C?

We fear that what Papalkar and Francis refer to as ''qualifications'' are in fact primarily statements about ''authority.'' After all, the hospital trolley boy may be very qualified to write about first hand observations in the hospital. The history of medicine is replete with tales of authority suppressing new, important information from those with a less authoritative voice. For example, Richard Horton, in his discussion of Sherwin Nuland's new book, *The Doctor's Plague: Germs, Childbed Fever, and the Strange Story of Ignac Semmelweis*, points out that although Semmelweis ''turned obstetrics into a respectable science'' he also ''revealed how professional eminence and authority could breed stupidity and bitter jealousy.''[3] The resistance of the Viennese medical establishment to the young Semmelweis's investigations of epidemic childbed fever is a sordid tale of well respected, seemingly qualified medical authorities obstructing the dissemination of an important medical discovery.

We do agree with one aspect of Papalkar and Francis's letter—the journal should be consistent in ''appending qualifications.'' We would suggest that the journal should not publish any such ''qualifications'' or ''academic titles.'' We would also propose that perhaps masking the authors' names and qualifications from reviewers might provide for a more uniform and fair review process that would make it *less likely* that second rate papers from world authorities are published and that original thought provoking ones from less well known physicians are not overlooked. Although we do not agree with the thesis of the letter by Papalkar and Francis we wish to thank them for bringing the issue to our readers. We would encourage readers of the *BJO* to express their opinions as a ''rapid response.''

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