20 Key Challenges in Advancing Educational Neuroscience

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In this final chapter, we identify some of the themes that have emerged in this volume, as well as some challenges for the future.

What Should Teachers Know About Neuroscience?

The research covered in this volume shows how advances in neuroscience can give insights into learning in the classroom. But what do teachers need to know about neuroscience? Do they need to know how the brain functions or what methods neuroscience uses? How detailed should this knowledge be? Several views were offered in different chapters. Bell and Darlington saw the goal of understanding learning as a professional responsibility for teachers and the basis of their practice: they drew an analogy to the importance of doctors understanding how the body works and being up to date with the latest treatments. Howard-Jones and colleagues offered a simplified version of brain function that captures key cycles of the process of learning in the classroom: engage—build knowledge—consolidate—apply. For these authors, explanations couched in terms of brain functions permit a visual, accessible, and engaging means to communicate about the learning process, and a basis for teachers to reflect on their practice.

Many of the authors saw neuroscience as part of a wider approach of informing teaching by evidence of what works, for them implicating neural mechanisms of learning. The factors influencing educational outcomes are many and complex; where there is ambiguity and risk of fads and fashions in teaching methods, convergent evidence of mechanistic plausibility increases confidence and motivates investment in more rigorous testing. At the very least, as Howard-Jones and colleagues say, the inclusion of explanations of learning informed by cognitive neuroscience allows for the dispelling of teachers and students' existing myths about the brain and inoculates them against acquiring new ones.

We believe that what teachers need to know about neuroscience, therefore, is threefold: a broad characterisation of how learning works in the brain, to generate intuitions about the factors that may harness it; an understanding of how their own brain function may influence their teaching skills; and an awareness of the importance of convergent evidence across disciplines in evaluating whether teaching methods work.

Development Versus Individual Differences

The volume had four areas of focus: individual differences, development across the lifespan, cognitive enhancement, and translation into the classroom. The individual differences perspective considered what makes children better or worse at learning, either in terms of their cross-disciplinary skills (executive functions, emotion regulation, engagement) or in terms of their discipline-specific skills (e.g., phonology for reading, symbolic magnitude understanding for arithmetic, perceptual and conceptual understanding of physical systems for science). The implication of differences is that children may need to be taught differently depending on their abilities or prior knowledge. By contrast, the developmental perspective considered how abilities change across the lifespan, for example that executive function skills are late maturing providing an early constraint on learning, or that adolescence provides both vulnerabilities (e.g., decision making in the presence of peers) and opportunities (elevated response to feedback) for educators. The implication is that teaching methods need to be appropriate to skills levels at each age.

Taken in isolation, both these perspectives have downsides. The individual differences approach draws focus to the limiting factors on a child's progress, at the expense of understanding the learning mechanisms and environments that are needed to learn a skill at all. Limiting factors can mask each other: if one limiting factor is removed, the next is revealed. Moreover, the approach is sometimes drawn to focusing on those limits and can pay insufficient attention to factors that produce strengths. The developmental approach risks averaging across children, prompting a one-size-fits-all approach that sacrifices the opportunity to personalise learning and build on strengths of the individual. The two perspectives can at times diverge. In the chapters of Donati and Meaburn, and Hackman and Kraemer respectively, we saw considerations of genetic and environmental influences on educational outcomes. It might be that the principal driver of development is the environment, but the principal driver of individual differences is genetics. For example, in reading, exposure to print is necessary to learn to read at all, but if reading experience is sufficient, the limiting factor can be genetically caused differences in phonological ability. The ultimate goal must be to integrate both variation and development within a common framework: to consider individual differences as variations in trajectories of development.

Although the relation of individual differences and development may appear a theoretical concern, it has echoes in policy. Is the goal of education to improve the performance of the whole population—say in literacy or numeracy—by moving the whole distribution of performance further up the scale? This would be a developmental concern. Or is it to change the gaps between children, ensuring no child is left behind? This would be an individual differences concern. In her chapter, Knowland identifies similar ethical implications in the context of enhancing cognition. If school is deemed primarily to concern the acquisition of skills and learning (the developmental perspective of improving everyone), then it is less controversial that cognitive enhancements should have a legitimate role in aiding those improvements; but if school is primarily deemed to be about grades and competing for jobs and places at university (individual differences perspective), artificial enhancement is more akin to cheating in the competition. An integration of development and individual differences would prompt a resolution of these kinds of policy and ethical ambiguities.

The focus on neural mechanisms can also de-emphasise some other important questions here: individual differences and development can be combined into a measure of 'mental age'—should children be taught according to their mental age, so that the same method is appropriate for a young very bright child as an older less bright child? Should classes be streamed by ability? Should group work combine children with mixed abilities or be similarly streamed?

What Works?

Educational neuroscience is consistent with the wider ambition of accumulating an evidence base of what works in education. Quotes from two chapters illustrate this view: 'the integration of different levels of analysis and data has the potential to generate a better explanatory model of mechanisms underlying a particular educational phenomenon [and thereby] constitute a better base for grounding diagnostic approaches and educational interventions' (De Smedt); 'studies of neurophysiological mechanisms allow us to map and predict what happens when different people receive a given intervention in different contexts' (Knowland). The goal is to identify what works and why.

Organisations within the US and the UK now list the growing evidence base for particular techniques, for example the What Works Clearing House[1] and the Education Endowment Foundation (EEF)[2] with its Teaching Toolkit[3]. The EEF funds large-scale randomised control trials. In a randomised control trial, individuals are allocated at random to receive one of several interventions. In addition to the target intervention, there are one or more control conditions to provide a standard of comparison. This could be no intervention/standard practice, or an alternative intervention that is similar in many respects to the target intervention but does not contain the proposed active agent—serving as a sort of placebo to check that any effects are not produced just by 'being in an intervention'. The EEF's trials combine pre-registered studies and independent evaluation of trial outcomes to give maximum confidence in the results, as well as a preference for teaching as normal (or 'business as usual') control groups to verify that the intervention is better than current practice.

There are a number of issues that arise in the use of randomised control trials (RCTs)—the gold standard method used to evaluate new treatments in medicine. First, a consensus is only beginning to emerge in the appropriate methodology for educational interventions that target cognitive abilities (Shawn Green et al., 2019). The consensus distinguishes different types of studies that provide a pathway to produce new interventions and understand how they work. The first is a *feasibility* study, a small-scale study to demonstrate that a method might work. The second is a study of *mechanism*, using appropriate experimental methods and active control groups to establish the mechanisms underlying the effect. The third is an *efficacy* study, evaluating the performance of an intervention on a larger scale but still under ideal and controlled circumstances. The fourth is an *effectiveness* study, investigating performance under real-world conditions. Good practice pursues these types of study in sequence, and the different study types require appropriate control groups and sample sizes.

The second issue is whether RCTs should have the equivalent 'gold standard' status for educational interventions. One challenge is that, cases of deprivation apart, gains in educational achievement may be the consequence of many small influences. Most of the effect sizes of successful interventions listed by the Educational Endowment Foundation and the What Works Clearing House are moderate or small. Large-scale RCTs vary just one factor compared to a control group, against a background of large variation from all the other uncontrolled factors. This leads to a paradox. It may be hard to detect successful interventions because they represent small signals against a background of larger noise. Many RCTs may produce null results. A large body of null results could lead to despondence that educational outcomes can be improved. Yet if all the effects are added together (e.g., in nutrition, sleep, aerobic fitness, stress reduction, engagement/knowledge building/consolidation activities in the classroom, topic-specific focus on core cognitive abilities, topic-general training in executive function skills and emotion regulation, meta-cognitive training to support transfer, resilience training for mental health, specific training in socioemotional skills, increased parental support, reduction of social inequalities, to list a few) very large increases in educational outcomes may be possible. In short, it may be that large improvements in educational outcomes are possible but only by combining many small effects, each of which is hard to establish individually.

Related to this is the tension between *investigating how it works* versus *getting it to work* (Thomas et al., 2019). The objective of the researcher is to understand how each factor contributes to educational outcomes and understand the mechanisms underpinning the effect. They must distinguish causal effects from many naturally occurring correlations, by systematic manipulation of individual factors. However, for those who solely want to improve outcomes, the best strategy is to throw everything at it that might work. While this gives the best chance of a good outcome, the disadvantage is that if there is an improvement, there will be no insight into what factors produced it.

The final issue with RCTs is that they risk producing prescriptive techniques. The researcher has demonstrated that a technique works under certain conditions. They then instruct educators to use the technique replicating these conditions (retaining the so-called fidelity of the intervention). However, prescription undermines the teachers' autonomy. It is at odds with the way teachers normally teach, by adapting materials and techniques to particular content and the children in front of them. Prescriptive techniques are less likely to have wide uptake. By the same token, if teachers vary the technique, they may inadvertently omit the key ingredient by which it operates, rendering it ineffective. And when teachers conclude that—in their own hands—the supposed scientifically verified techniques don't work, there is a risk that confidence in the enterprise of using RCTs to build an evidence base will fall away.

What is needed, as Bell and Darlington articulate, is for researchers to identify the *tolerance limits* of an intervention—how much it can be varied and in what ways, while still retaining its effect. In order to do so, researchers must include an active control condition in their RCT (that is, a control intervention that is similar in all ways to the target intervention except without including the proposed active agent)—in addition to a teaching as normal control. Successful interventions that show benefits compared to both control groups can then be used flexibly by teachers without destroying their effects. Techniques can be adapted to context and content by retaining the active agent.

Translation and Policy

Debate still lingers within educational neuroscience about what type of field it should be. Some researchers view it as primarily a basic science, amounting to a sort of cognitive neuroscience of skills that are relevant to education (e.g., Gabrieli, 2016). Perhaps the majority of researchers view it as more inherently translational, with the goal of informing actual practice in the classroom. We believe that most of the chapters in the volume accord with this latter view. It would certainly be somewhat of a waste not to attempt translation from the basic science, when so many children might gain from the insights that basic scientists gain.

However, translation is not straightforward. As de Smedt says,

> the mere identification of a neural correlate or neurocognitive factor does not readily answer questions about effective teaching and curriculum design. This requires a nuanced translation and an integration of findings from neurocognitive studies with educational theories and frameworks of effective instructional design.

The heart of educational neuroscience must remain as a dialogue between educators, policymakers, and those working in the learning sciences. The dialogue needs to involve teachers influencing the direction of research as much as researchers communicating science findings, and the translation of individual techniques into forms that are useful (but still effective) in the classroom is an enterprise that can only be achieved collaboratively. The field of educational neuroscience will gain from greater investment in infrastructure than can support the influence of educators on neuroscience. There is much to gain from practitioners identifying the questions, puzzles, obstacles, and challenges that impede educational achievement, so that neuroscientific approaches (amongst others) can be used to try to understand and overcome these; and from practitioners helping shape neuroscientific insights into practices that are robust and useful in the classroom.

Interaction with policymakers and influence on policy is a frequently stated goal of researchers working in educational neuroscience. This brings its own set of challenges. For example, the role of policymakers is often to mandate national or regional requirements (such as, say, the phonics screening check given to six-year-olds in the UK, based on psychological research identifying early behavioural markers for future literacy problems). Researchers have to be clear on whether that is the goal of their research, or whether it is to provide a wider set of tools for teachers to (optionally) use in the classroom.

Engagement with policymakers also requires a communication strategy. There is usually debate in scientific fields, but a consensus position needs to be established among scientists prior to communication based on the balance of probabilities. To present dissenting scientific voices to policymakers risks undermining confidence in the maturity of the field and encourages the view that translation is premature.

Policymakers are often keen to have their policies informed by evidence and can be enthusiastic about neuroscience. This represents a dilemma. If educational neuroscientists 'hold their fire' until they feel the science is more certain, policymakers will be listening to others (lobbyists, interested parties) whose views may be less evidence based. Should they go ahead and offer 'best guesses' based on existing science? In the past, educational neuroscience has been criticised for offering policy advice because the science was deemed too immature (e.g., in Bruer's 1999 book, *The Myth of the First Three Years*). Then there is the risk that policymakers will cherry pick neuroscience evidence to fit pre-existing political agendas. Caught between the urge to say something, but not say something on which we are not yet certain, the educational neuroscientist is in a difficult situation. Researchers need to find a balance where they do not overstate the current state of the basic science and the maturity of translation, but do not understate the importance of the science of learning in supporting an evidence-informed approach to policymaking in education.

Can Cognition Be Enhanced?

This volume uniquely brought together in one place chapters evaluating diverse methods proposed to enhance cognition. Education as a whole can be viewed as a form of cognitive enhancement (Ritchie & Tucker-Drob, 2018; though see Cigman & Davis, 2009, for a counter view). The concern here was with more specific methods that might have general benefits for cognition, perhaps even to raise IQ. What conclusions can be drawn from this section as a whole?

First, the general pattern appears to be that cognitive training leads to near transfer—improvements are for the abilities that have been trained, or very similar ones. Far transfer to very different skills (or cognition as a whole) appears to be the exception. Far transfer may require interventions that operate on the functioning of the brain as a whole at a biological level, such as in nutrition, energy supply, or levels of stress hormones, rather than regimes of behavioural training. Or, as suggested by Semenov and colleagues, it may require specific training *in* far transfer, that is, explicit strategies in identifying what pre-existing skills and abilities may be applicable to new situations.

The lure of far transfer is perhaps that it would be so efficient. One would only have to undergo a single training regime to see benefits across other skills, rather than complete separate training in all the individual skills. Despite the many proposals for techniques giving general cognitive benefits, the suspicion is that we will not find this panacea. However, near transfer is not to be scoffed at. Improvements are readily available in individual skills, testament to the brain's enduring plasticity. And as a guiding principle, near transfer can be used to design interventions. For example, Wilkinson et al. (2019) in press) designed a mathematics and science intervention for 8- and 10-year-olds that targeted inhibitory control in learning counter-intuitive concepts. By the principle of near transfer, the training for this executive function skill was embedded in the content of the domain in which it was required (the age-appropriate mathematics and science syllabuses). Improvements were then observed in subsequent achievement tests.

We might briefly reflect on what the pervasive lack of far transfer in cognitive training tells us about how the brain/cognitive system works. The most obvious conclusion is that the brain mostly comprises content-specific processing systems, rather than general 'jack-of-all-trades' processing mechanisms. This is at odds with much of traditional cognitive psychology, which has invoked general mechanisms such as working memory and attention (see Thomas, Ansari, & Knowland, 2019, for discussion). If those domain-general mechanisms really existed and were trainable, far transfer should be much more apparent.

The chapters in this volume highlighted one key challenge in evaluating cognitive enhancement techniques—that of random allocation to condition. Unless the experimental design randomly assigns participants to intervention versus control conditions, any advantage for the intervention condition could be due to other pre-existing group differences rather than the intervention itself. So in a given time and region, bilinguals might be systematically different to monolinguals (say, in socioeconomic status [SES]); teenagers who ended up playing action video games throughout their teenage years might be systematically different to those who did not (say in their motor dexterity or sensitivity to reward structures); children who get to learn a musical instrument and put in a lot of practice may be systematically different to those who never had the opportunity or completed the practice (say in their SES or conscientiousness). Without random allocation, systematic group differences can only be suggestive and must be complemented by properly designed intervention studies. But such studies can rarely replicate the size of the dosages (in our examples, of language learning, of video game playing, and of instrument learning, respectively) that may have had the effect.

Of the interventions we saw in these chapters, aerobic exercise appeared to offer benefits for executive functions (and of course, health benefits), with primary age children perhaps most able to benefit (Wheatley and colleagues); action video games yielded benefits, though more narrowly to top-down attentional control of visual processing—putting aside issues around violent content[4] (Altarelli and colleagues); mindfulness offered benefits for executive function skills, particularly around emotion regulation, though the evidence from younger children was still thin (Semenov and colleagues); learning a musical instrument appeared not to offer wider cognitive benefits (Schellenberg) (see also a similar story with learning chess; Sala & Gobet, 2017; Sala, Foley, & Gobet, 2017); bilingualism improves language skills at some temporary cost to early vocabulary development, but the jury is still out on wider cognitive benefits (Phelps and Filippi); sleep deprivation is bad for learning, and may be a particular current issue for adolescents, but getting more than normal amounts of sleep does not produce particular cognitive benefits (Sharman and colleagues).

Because expectation and novelty can produce temporary illusions of cognitive benefits of an intervention, and because commercial organisations are motivated to promote brain training for financial gain, it remains important for educational neuroscientists to investigate the mechanisms that underpin purported training effects. That is, for each of the putative generally beneficial activities, it is desirable for investigators to propose and evaluate the cognitive and brain structures that mediate the transfer from training task to other cognitive skills. The less plausible the underlying mechanistic basis for the transfer, the more critically the published evidence in favour of the transfer must be examined.

What Is the Added Value of Neuroscience for Education?

Critics of educational neuroscience have sometimes portrayed neuroscience and psychology as being in competition for which discipline should inform education (e.g., Bowers, 2016). In our view, this is a nonsensical position. Neuroscience and psychology are complimentary approaches to studying the same system (the mind/brain), operating at different levels of description and employing different methods. The goal is to have convergent and consistent accounts (see Howard-Jones et al., 2016; Thomas, 2019). In our view, interdisciplinary research is the best way to improve learning outcomes in the classroom—that is, cooperation rather than competition. Moreover, terms like 'neurocognitive' and 'cognitive neuroscience' reveal how the disciplines are blending, so that the contribution of each is not easily discerned. Nevertheless, it is worth attempting to emphasise what particular value neuroscience can add for education, and we therefore requested each of our sets of authors to include a section on this point at the end of their chapters.

There were many different answers to this question, and readers might like to revisit these sections across the different chapters. Here, we pick out a few themes. Some authors pointed to the impact that neuroscience evidence could have both on researchers and policymakers—if something can be seen in the brain, it seems more real. The effect of SES on cognitive development (Hackman and Kraemer), the immaturity of certain aspects of executive function in adolescence (Peters), and the complex interactions between emotion and cognition (Immordino-Yang & Gotlieb) are some examples. Relatedly, some authors pointed to the role of neuroscience in adding convergent, independent evidence that effects observed in behaviour were real, rather than the product of measurement error or artefacts (for example, cognitive differences in bilinguals, Phelps & Filippi; or confirming the structure and function of the reading system, Tong & McBride-Chang. See also Ramsden et al., 2011, for a striking demonstration of how structural brain imaging data supported ambiguous behavioural evidence that intelligence is still changing across the teenage years).

Some authors identified new ideas that had emerged from neuroscience, such as a new sensory basis for dyslexia in rhythm processing (Goswami); symbolic magnitude as a key constraint on mathematical concept learning (de Smedt); adolescence as period of risk and opportunity based on the neural systems maturing in those years (Peters); that shared biology partly underpins the observed relationships between educational performance and a diverse array of social, economic, and physical and mental health outcomes[5] (Donati and Meaburn); and that science education involves the construction of fragmented mental representations, built on top of each other rather than replacing each other, differentially activated according to task and context, and integrated only through the organising role of language-based concepts (Tolmie and Dündar-Coecke).

Other authors pointed to the opportunity neuroscience offers to understand *why* interventions work. For example, action video game playing improves reading speed in dyslexics because it enhances cortical top-down visual attention, although it leaves subcortical extrinsic orienting systems unaffected (Altarelli and colleagues). Mindfulness training produces effects through automatizing the screening out of environmental and emotional distractors (Semenov and colleagues). Peters suggested that if training results in enhanced recruitment of the same neural network that was active before training, this could indicate that capacity is increased by training, while if new brain regions are recruited after training, this may point to a different strategy being employed (see Thomas et al., 2019, for detailed discussion of neurocognitive mechanisms underlying interventions). Peters additionally argued that neuroscience might also lead to predictions of who interventions will work better for: 'neuroscientific measures could eventually be used to tailor interventions to individual students. Understanding which strategies and neural networks a student currently uses during a cognitive task, could potentially indicate which type of intervention may work to bring their performance to a higher level.'

While neuroscience can add value, its contribution also carries some risk. Where structural or functional differences are observed in the brain, they can lead to the assumption that there is nothing we can do about them, such as in genetic differences or those caused by SES. But this assumption would be wrong: the brain's plasticity across the lifespan supports plentiful behavioural change in response to training. Schellenberg, in his review of associations between musical training and intelligence, laments the error of inferring causation from correlation. For music and intelligence, he argues a common causal factor, largely genetic, is probably responsible for the correlation, rather than there being a direct cause of music training improving intelligence. But with supporting evidence, he also argues that educational neuroscientists are more at risk of making this error—in studying the brain, they mistakenly believe they are studying mechanisms, and therefore believe the correlations they observe are more likely to be causal. Lastly, Hackman and Kraemer argue that neuroscience in education can produce too great a focus on individual factors affecting educational outcomes, rather than societal, school, and family factors that may have more powerful effects.

What Are the Concrete Implications of Neuroscience for the Classroom?

We also asked our authors to comment on what they saw to be the concrete implications of neuroscience research for the classroom. Again, the answers were diverse, and we encourage readers to compare and contrast these concluding sections across chapters. Some authors remained cautious. Tong and McBride-Chang argued that there still exists a big gap between laboratory research and practice, such that results of neuroscience research have not been directly and commonly used in practice. Howard-Jones and colleagues said that the dearth of brain imaging studies employing educational-like tasks means that the relevance of cognitive neuroscience to classroom practice is more reasoned hypothesis.

Nevertheless, many concrete examples were offered. De Smedt pointed to the identification of core skills in domains like mathematics that would help detect children at risk of poor learning. Like others, he recognised the potential of neural markers of those skills that could be measured before the developmental emergence of the behaviours themselves, and so offer the scope for earlier intervention. However, he conceded that such markers need to meet appropriate thresholds of sensitivity and selectivity (not to mention cost and practicality) to be of use beyond the laboratory. Sharman and colleagues pointed to both neuroscience and behavioural level responses to the shift in circadian rhythm in adolescence and subsequent reduced amounts of sleep: the neuroscience-inspired response is to shift the start of the school day to match the teenagers' shifted body clocks; the behavioural intervention is to educate teenagers in good sleep practices so that, for example, they don't drink coffee in the evening or use screen devices in darkened bedrooms before going to sleep.

Hackman and Kraemer admitted that the concrete implications of cognitive neuroscience research on SES are as yet limited, but speculated that there may be variation in what predicts academic success both within and across SES levels—and thus a multiplicity of strategies may be most effective rather than specific tailored approaches. This is striking, because the more common narrative that stems from educational neuroscience is one of the personalisation of education. Notably, Hackman and Kraemer proposed that neurocognitive measures need to be considered in a broader context, and that neuroscience may be most useful when it helps guide more refined interventions that focus on social systems and processes rather than on curricula or on individuals.

Several authors identified unexpected potential avenues to remediate literacy difficulties, among them processing of rhythm (Goswami), action video game playing (Altarelli and colleagues) and learning a musical instrument (Schellenberg). Immordino-Yang and Gotlieb sketch out an agenda for the development of the 'intellectual virtues', based on their analysis of the role of emotion in learning. These virtues included interest, curiosity, intellectual humility, and intellectual agency. Howard-Jones and colleagues argued that neuroscience concepts could be powerful tools in driving the reflective processes of both students and teachers. Phelps and Filippi urged educators not to misidentify characteristics of bilingual language acquisition as indicative of developmental language disorder, arguing that in the UK at least, English as an Additional Language (EAL) status has acquired unwarranted negative connotations. Instead, educators should focus on the benefits of experience of multiple languages, both within the classroom and in the wider (global) community. Lastly, Schellenberg argued that even if music training does not raise intelligence, music training improves *musical* skills, which should be enough!

Future Directions

Most neuroscience is not relevant to education. It is too low level, concerning particular biological processes or neural mechanisms; or it utilises animal models unsuited to address the cultural practices of education. By the same token, much of education is beyond the reach of neuroscience, concerning societal structures and institutions, decisions about funding and curricula. This volume has demonstrated, however, that there are ideas from cognitive neuroscience that can contribute to education where the focus is on mechanisms of learning. At this interface, a dialogue must take place, to render research into a form useful in the classroom, and to allow educators to guide researchers towards the most pressing challenges. It is important, therefore, to be realistic about the scope of educational neuroscience. It is not the be all and end all.

Within this restricted scope, it is possible to discern some of the most productive avenues to advance research. The first is to continue to identify sources of individual differences in educational outcome, at multiple levels of analysis. We saw chapters on genetic influences and socioeconomic influences—there are opportunities to integrate these views into a single account of variations in academic achievement. Two crucial questions remained to be answered: how much of the variation (in a given society) stems from home factors versus school factors? The answer should influence policy. Within school factors, how many of these will concern optimising the conditions of brains to learn (and consolidate learning), versus identifying content-specific approaches that will improve outcomes for particular skills or topics, versus enhancing skills that are applicable across learning situations?

The direction of travel of much of educational neuroscience is to identify how to personalise learning—to offer a child the tailored educational environment to get the best achievement (and happiness). Neuroscience and genetic data can offer more information to complement traditional demographic and behavioural data. However, the practical challenge still remains of generating the different pedagogical practices to exploit these data, and of delivering personalised learning given the constraints on materials, skills, and resources within the classroom. We have seen some debates in our chapters—for example, Hackman and Kraemer speculating whether multiple strategies might be superior to tailoring, or Knowland wondering whether the early years (the oft-purported best opportunity for intervention) should fall within the remit of an educational neuroscience at all. Donati and Meaburn opined that the time was right for a societal debate about whether we want to generate genetic information about our children's educational potential and if so, how such data might be safely and ethically used.

Technology has been offered as a solution to personalisation, in that computer-based tutoring approaches (or learning environments curated by the teacher for the child's autonomous exploration) can be adaptive, tailored to the user. Such adaptive systems are underpinned by powerful new developments in artificial intelligence and machine learning. If Facebook can tailor advertisements and swing elections, surely such technology can tailor tuition and swing examinations? We are not there yet. In their review of action video games, Altarelli and colleagues offered tantalising clues about the dynamics of computer games that produce powerful changes in behaviour. Unfortunately, most educational games currently focus on content and do not deliver the relevant game dynamics to deliver equivalent engagement.

Technology itself is an important future direction for educational neuroscience, given the pervasiveness of smart phones and other screen devices. There will be reactionary responses to these changes in society, as they alter past times and ways that people relate to each other. We might ask, how do they modify learning and the potential for learning? No doubt they will alter brain structure and function, because brains are plastic and afford acquisition of new skills. But changes in reliance on external tools will change what we learn, and we may therefore need to alter what we teach. Moreover, changes in habits may have side effects—changes in sleep patterns or levels of physical exercise, changes in the social interactions that provide support or challenges to mental health—that need to be addressed. Understanding the neurocognitive mechanisms will, we believe, help in these endeavours.

We believe the new focus on the role of emotion in learning, outlined by Immordino-Yang and Gotlieb, also offers great potential. This approach can harness both teachers' and students' skills to best outcome. For example, Immordino-Yang and Gotlieb used this framework to characterise what they call *high-quality educational practices*. Such practices 'place the learners' subjective emotional and social experience at the forefront, and help people build scholarly and social identities that incorporate their new skills and knowledge. They help people to feel safe and purposeful, and to believe that their work is important, relevant, and valuable. They support age-appropriate exploration and discovery, followed by cognitive elaboration for deeper understanding. And, they support the learners in pacing themselves to iteratively and authentically move between these modes of engagement as they pursue meaningful learning goals . . . when students are working hard because they are steering toward intrinsic, problem-centered goals, and not primarily because they are trying to satisfy some relatively arbitrary milestone . . . deep thinking and transfer of knowledge are more likely to happen.'

Paul Howard-Jones and colleagues identify the important area of the neuroscience of teaching. This pertains both to the processes underlying teaching skills (where, say, it is useful for teachers to understand that their emotional state with respect to a topic can influence students' learning); and it pertains to the explicit knowledge teachers need about neuroscience that will help them with their practice (such as these authors' schematic of engage-build-consolidate and apply).

Finally, there are areas that were not covered in much depth in this volume and which we think are of importance. The development of an evidence-based pedagogy for Special Educational Needs—informed by an understanding of the basis of developmental deficits and the differential constraints on brain plasticity—is one important avenue. A second is a deeper focus on the neuroscience of adult learning, how it differs from learning in childhood, and how it alters in later lifespan during ageing (see, e.g., Thomas, Knowland, & Rogers, in press, for a recent consideration of the neuroscience of adult learning in the context of adult literacy programs in the developing world).

Educational neuroscience is still a young discipline, with lessons to learn. As psychology demonstrates, translation can be as challenging as the basic science. This volume naturally focuses on the insights offered by neuroscience at the interfaces with psychology and education, but we remain committed to the broader belief that interdisciplinary research is the best way forward for education.

Notes

1. https://ies.ed.gov/ncee/wwc/
2. https://educationendowmentfoundation.org.uk/
3. https://educationendowmentfoundation.org.uk/evidence-summaries/teaching-learning-toolkit
4. See, e.g., www.educationalneuroscience.org.uk/resources/neuromyth-or-neurofact/violent-video-games-make-children-more-violent/ for discussion of this issue
5. See Selzam et al. (2019) for a recent detailed analysis of the extent to which environmental and genetic effects on variation in cognition are confounded.

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