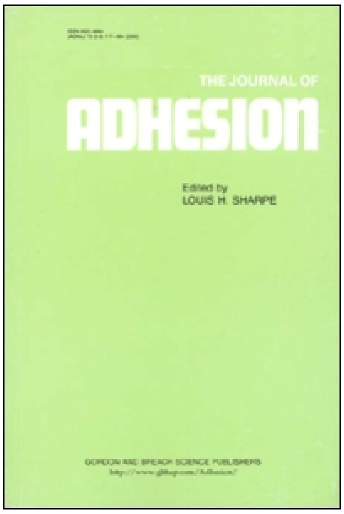

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The Mechanical Theory of Adhesion—Changing Perceptions 1925-1991

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The Mechanical Theory of Adhesion—Changing Perceptions 1925–1991*

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The changing fortunes of the mechanical theory of adhesion are traced from McBain and Hopkin's work in the 1920s to that of Venables in the 1970s and 1980s. Some comments are made on the factors which were associated with changes, during the period surveyed, in the accepted view of the importance of the theory.

KEY WORDS historical context of mechanical adhesion theories; philosophy of science; interlocking hypotheses; objectivity of theory; rationality of change in science; roughness and adhesion; anodising; aluminium; titanium.

An important part of John Venables' contribution to science lies in the work of himself and his colleagues on the structure of surface oxides on metals and the relation of this to adhesion and to the durability of adhesive joints. He has worked particularly on aluminium and titanium and has been especially interested in anodised layers. Keller, Hunter and Robinson[1] deduced the structure of porous oxide films on aluminium, but it was Venables who developed the scanning electron microscopic techniques which produced high resolution micrographs and, from them, beautiful isomeric drawings showing detailed features of these and other oxidised metal surfaces.

Many of these surfaces are porous and lend themselves to micromechanical keying of the adhesive. The importance of this for good and durable adhesion has been a major theme of Venables' work. This paper places Venables' contribution in a broader historical context by surveying the changes that have taken place in the accepted scientific view of mechanical adhesion over a period approaching seventy years. These changes are analysed in the light of different perceptions of the objectivity of theory and of the rationality of change in science.

THE CHANGING FORTUNES OF THE MECHANICAL THEORY OF ADHESION

Newton recognised that the phenomenon of adhesion was one which called for scientific study:[2]

*One of a Collection of papers honoring John D. Venables, the recepient in February 1991 of *The Adhesion Society Award for Excellence in Adhesion Science, Sponsored by 3M.*

"There are therefore agents in nature able to make the particles of bodies stick together by very strong attractions. And it is the business of experimental philosophy to find them out."

There appears to have been a lapse of two centuries before the challenge implicit in the passage quoted was seriously taken up. Most historical surveys of adhesion reckon the work of McBain and Hopkins in 1925 as the earliest scientific study of adhesion.[3] McBain and Hopkins considered that there were two kinds of adhesion, specific and mechanical. Specific adhesion involved interaction between the surface and the adhesive: this might be "*chemical or adsorption or mere wetting*."

Of particular interest here is what they say about mechanical adhesion:

"Mechanical joints are only possible with porous materials. . . . We find that a joint results between porous materials whenever any liquid material solidifies in situ to form a solid film embedded in the pores."

They cite wood, unglazed porcelain, pumice and charcoal as "porous bodies" which yielded strong joints with the majority of recognised adhesives.

This was 1925, but by the fifties a degree of unanimity had been reached which all but dismissed the concept of "mechanical adhesion." Wake,[4] writing in Houwink and Salomon's *Adhesion and Adhesives*—a standard work of the period—considered that

"theories that mechanical interlocking of adhesive and adherend add to the strength of a joint have been largely discredited."

Reinhart[5] claimed that "*mechanical adhesion seldom occurs, if at all*."

Yet the situation changed radically again, and in 1984 Venables could write[6]

"certain. . . . pretreatment processes produce oxide films on the metal surfaces which, because of their porosity and microscopic roughness, mechanically interlock with the polymer forming much stronger bonds than if the surface were smooth."

No scientist with any feel for history will be surprised that the orthodoxy of one period becomes heterodoxy in the next. It is nevertheless interesting to examine the literature to try to assess why the view of mechanical adhesion has twice changed diametrically since the work of McBain and Hopkins.

Mechanical Adhesion Established

McBain and Hopkins' attraction to the concept of mechanical adhesion seems to have been to some extent intuitive. They say that

*"it is **obvious** that a good joint must result whenever a strong continuous film of partly embedded adhesive is formed in situ."* [emphasis added]

They support this by citing the formation of strong joints between porous substrates and the majority of recognised adhesives. An *"excellent model of a mechanical joint"* which they describe consists of two pieces of silver gauze strongly joined by gelatin where two smooth silver surfaces were "*joined but feebly*."

They claim that wood joints are a *"rather surprising example of a **purely** mechanical joint"* [emphasis added]. They support this by two types of experiment. Firstly, they imply that coatings of stain sealed the porosity and reduced the adhesion. Secondly, they were unable to measure any adsorption of gelatin on wood meal. That neither of these experiments provided irrefutable support for purely mechanical adhesion was shown by a different interpretation being placed on the results a year later by Browne and Truax.[7]

Indeed McBain and Hopkins' paper contains the seeds of an alternative interpretation of wood adhesion. They accepted that *"a liquid which wets a surface and is then solidified* ***possibly*** *always makes a joint"* [emphasis added], and that many joints will result from *"both factors, mechanical and specific."* Of course, as was recognised by the physical chemistry of the time *"penetration will be obtained only if the glue wets the wood."*[7]

Thus the concept of mechanical adhesion was established in the 1920's by a combination of contemporary "common sense" which was supported, but not conclusively proved, by experimental results. Why, then, by the fifties had the mechanical interlocking hypothesis largely been abandoned?

Interlocking Hypothesis all but Abandoned

Reinhart, in a highly-regarded set of conference proceedings, considered the question of "specific *versus* mechanical adhesion."[5] His conclusion that *"mechanical adhesion occurs seldom, if at all"* was based on a survey of experimental evidence. He grouped the experimental results cited to support this conclusion under three headings: (A) smooth metal surfaces usually give higher bond strengths than rough ones; (B) roughening the surface of wood lowers adhesion; (C) the peel strength of cotton cloth depends both on the particular formulation of the adhesive and on the surface condition of the cotton fibres, both of which imply the involvement of "specific adhesion."

Reading his text carefully gives the impression that the evidence is suggestive, rather than conclusive. To illustrate this, let us examine the results presented on wood adhesion in the second group, (B). He gives a table (reproduced here as Table I) of shear strength values for maple wood bonded with urea resin and makes the comment

TABLE I
Relation of surface character of maple to compressive shear strength of bonds made with a urea resin adhesive
(Reproduced from Reinhart[5] with emphasis added)

	Compressive shear-strength bonding pressure, lb./in.^{2}						
Surface*	5 lb./in.^{2}	25 lb./in.^{2}	50 lb./in.^{2}	100 lb./in.^{2}	150 lb./in.^{2}	200 lb./in.^{2}	250 lb./in.^{2}
Planed	3120	3000	2810	3370	3220	3010	3760
Sanded	2360	2990	2340	3000	**3380**	3560	3420
Sawed	2690	3040	3000	2780	2990	3110	3080
Burnished	**3140**	2690	**3270**	**2980**	3050	**3220**	2890
Combed	2400	**3060**	2810	3000	2490	2800	3000

*The surfaces are listed in decreasing order of smoothness.

"The shear strength values bear a direct relation to the smoothness of the wood surface with the smoothest surface having the highest strength."

We must set aside, as anachronistic, criticisms that might be made based on our contemporary interpretation of the complex factors which contribute to destructive test results of this kind, and treat Reinhart's comments at their face value. We can see what he means: there is *perhaps* a tendency for the shear strengths to fall as the surfaces become rougher. On the other hand, plenty of the individual test results do not support this general trend. Some of these have been placed in bold type in the present version of the table. Further, it could be pointed out that it is only for *two* of the seven bonding pressures that the smoothest surface actually gives the highest shear strength.

Reinhart also refers to work where a model joint, made with a low melting point alloy between porous wood specimens, fell apart on handling despite (according to Reinhart) the metal's having penetrated into the pores and hardened. Here he has allowed his own view of mechanical keying to colour a careless reading of the original source. The original paper says of the joint that *"no appreciable penetration has been obtained even in the largest vessels of the wood"* and implies that this was because the metal did not wet the wood. The paper was Browne and Truax's[7] referred to above. Reinhart was surely no more than careless in the matter of penetration of the alloy, but he must have known that he was using this work to support his contention that the bonding of wood was not a good example of mechanical adhesion, whereas Browne and Truax's conclusion was significantly different:

"that mechanical adhesion is operative in the joining of wood ***can scarcely be questioned,*** *but [we] take the middle ground believing that specific adhesion is also an important factor"* [emphasis added].

A similar critique could be applied to Reinhart's other experimental evidence under his headings (A) & (C). The point being made here is not that Reinhart was dishonest or in any way "unscientific." At the worst, he was careless in the matter of the penetration of the alloy. He was, in fact, being scientific in reinterpreting results and drawing out facets that supported the scientific model of the day. It seemed to him and to most of his contemporaries that the vast majority of adhesion results could be rationalised without recourse to the concept of mechanical adhesion.

It is worth noting that Reinhart did not make a "frontal attack" on McBain and Hopkins' "common sense" view, exemplified by the joint with the silver gauze, that interpenetration must lead to a joint.

The abandonment of the "interlocking hypothesis" (Salomon Ref. 8) at this time was widespread, but not unaminous. Bikerman[9] accepted the idea of interpenetration leading to a joint as unproblematical as the attachment of a nail or a rivet:

"misunderstanding sometimes occurs when there are many nail-like protuberances on the surface and these are too small to be visible to the naked eye."

Bikerman was certainly idiosyncratic, but he was a formidable figure in adhesion science, having a wide and deep knowledge of the subject. Bikerman's support in 1960 for the concept of mechanical interlocking indicates both his individualistic set of mind and that the experimental evidence against it was suggestive, not decisive.

Mechanical Adhesion Restored

Reinhart was writing in 1954, Salomon in1965; by the mid 1970s the importance of mechanical contributions to adhesion were again being acknowledged along with those of specific adhesion. Wake reviewed the situation in his book *Adhesion and the Formulation of Adhesives*[10] published in 1976. He concluded that

"adhesive joints frequently possess an important mechanical component essential to the performance of the joint but this type of component cannot suffice as the sole mechanism whereby surfaces are joined. It must be enhanced by, just as it enhances, specific adhesion."

Why did the pendulum swing again?

Most of the new work from the 1960s cited by Wake falls into one of two categories. The first is associated with the electroless deposition of metals onto plastics such as ABS and polypropylene. In the process, the plastics must be etched in a way which produces pits on a micron scale. Such a topography had been shown to be a necessary, but not sufficient, condition for adequate adhesion.

The second category was concerned with adhesion to porous or microfibrous surfaces on metals. In 1969 Packham[11] had demonstrated the importance of pore structure in the adhesion of polyethylene to anodised aluminium. Arrowsmith[12] worked with electroformed copper and nickel, and argued that mechanical adhesion was the main mechanism of adhesion of these surfaces to epoxide laminates. There were further reports of the importance of topography in adhesion, for example, to copper[13] and titanium[14] with needle-like oxides.

It is interesting to note that McBain and Hopkins' description[3] of a liquid that solidifies and forms a *"film embedded in the pores"* could be applied to these cases, although the scale of the porosity was often beyond the resolution of microscopes available in 1925.

The demonstration of a number of surfaces where pore penetration occurred and the porosity was linked to the adhesion led to a revival of the mechanical theory of adhesion. Again, it should be noted that a resolute opponent of the theory could still reject it, for example, talking in terms of the enormous increase in area of contact between adhesive and substrate afforded by these porous surfaces, and the consequent increase in specific adhesion. Wake, in his summary,[10] is careful to acknowledge the importance of specific adhesion even for porous surfaces.

Venables' Contribution

This, then, was the context in which Venables' massive contribution was made in the decade or so from 1977. He and his colleagues played a major part in showing that ideas about the importance of porosity, which had been demonstrated for joints studied for their scientific interest, were of tremendous engineering significance for the aerospace and other industries.

By bringing a full range of modern surface analytical techniques to bear and by developing methods of high resolution electron microscopy, Venables was able to show, in detail previously undreamt of, the structure and composition of oxidised films on aluminium and titanium. The impressive isometric drawings of oxide structures from his, now classic, 1979 paper[15] must have been reproduced hundreds of times. With this morphological information he was able to demonstrate with great clarity the relationship between the strength of adhesive bonds and the structure of the substrate surface. With such experimental evidence he was able to argue strongly[16] that for aluminium and titanium

"certain etching or anodization pretreatment processes produce oxide films on the metal surfaces which, because of their porosity and microscopic roughness, mechanically interlock with the polymer forming much stronger bonds than if the surface were smooth."

This, of course, does not detract from the importance of specific interactions in affecting the extent of wetting or deny their importance in augmenting mechanical effects.

Probably, durability in a humid environment is the most important practical limitation to the practical use of adhesive bonds. The studies of Venables *et al.* on durability were often able to link the morphology and chemical composition of the oxidised surface to the durability of bonds made to it. They showed the importance of hydrolytic resistance of the oxidised layer and developed ways of increasing the stability of various surface structures. Their surface behaviour diagrams greatly aid clarity of understanding of the complex interplay of factors.

THE OBJECTIVITY OF THEORY AND RATIONALITY OF CHANGE

The changing fortunes of the mechanical theory of adhesion over a seventy-year period are interesting, not only in their own right, but also because of the light they throw on the process of science. They give several illustrations of (minor) scientific revolutions, showing how a theory (at any rate this one) becomes established, and how it falls from favour, and is replaced by an alternative model.

There is a view of science which sees it as developing logically on the basis of experimental observation, and leading to an ever-increasing cumulation of objective truths about the world. This has been called the "rationalist view" of science[17] and is encapsulated by the statement ascribed to Galileo:

"The conclusions of science are true and the judgement of man has nothing to do with them."

This concept is widely used in the teaching of science[18,19] and can be employed to great didactic effect. It is not surprising, then, that it is widely accepted by the general public and by some, perhaps many, practising scientists.

In contrast to this are views influenced by Popper's work[20] on the nature of scientific knowledge and Kuhn's[18] on the structure of scientific revolutions.

Popper argues that it is impossible to prove the truth of a scientific theory, although it may be possible to refute it, and replace it by another "provisional" theory. Thus

"science is not a system of certain, or well-established statements: nor is it a system which steadily advances to a state of finality."

Kuhn's analysis of scientific change emphasises that a new model (paradigm) is never accepted for purely logical reasons and, similarly, experimental evidence is never logically sufficient to compel a scientist to abandon a model which many peers regard as discredited. "Non-scientific" considerations will often play a part.

Writing about "objective truth and neutral knowledge" in a wider intellectual context, Hawkesworth[21] argues a similar point.

"The conceptions of ***neutral knowledge*** *and* ***value-free methodology*** *are markedly* ***defective****. They fail to recognise that perceptions are theoretically mediated, that facts are theoretically constituted and that methods of enquiry are permeated with value-laden assumptions concerning the objects of enquiry"* [emphasis added].

The school of thought called "New History and Sociology of Science" has taken cognizance of Popper's and Kuhn's ideas in developing a view of science. Some of the ideas of the school are summarised in the following statements.[22]

"Experimental findings are seen as inherently defeasible: all experimental findings may be criticised, and no experimental finding need be taken as crucial confirmation or disconfirmation of a theory. Decisions about the status of experimental findings are not dictated by the findings: scientists' judgements may be informed by commitments to certain accounts of what the natural world contains and by [considerations] usually thought of as 'external' to science."

Where does the mechanical theory of adhesion fit into all this? Can its development be properly regarded as following an inevitable, compelling logic, or were the arguments which brought about the various changes in the scientific consensus at best persuasive? Were there decisive experiments at the critical stages which at that time amounted to crucial confirmation or refutation of the theory, or did different workers give different weight, or even different interpretations, to the same sorts of experimental results? Were some of the positions adopted coloured by "non-scientific" considerations—intuition, common sense, even by personality and psychology?

I would argue that the development of the mechanical theory is better described in the terms of the new history and sociology of science than by the rationalist account.

There is the use of intuition and the appeal to "common sense." McBain and Hopkins urged that it was "*obvious*" that porous solids would provide a mechanical key, Browne and Truax asserted that mechanical adhesion to wood could "*scarcely be questioned.*" Bikerman appeared happy to ignore the evidence that had convinced Reinhart and others. The results (*e.g.* Table I) quoted by Reinhart were certainly open to various interpretations, yet he was so convinced of the unimportance of mechanical contributions that he read—or rather misread—his prejudice into Browne and Truax's paper.

Venables' commitment to the importance of mechanical interlocking is clear (for example in the quotations above); Kinloch,[23] reviewing the literature at much the same date, was much more circumspect,

"in certain instances mechanical interlocking ***may contribute*** *to the intrinsic adhesion mechanisms . . . this appears to happen in only a few instances . . . observed increases in [joint strength with roughness] are* ***usually*** *attributable to other factors."* [emphasis added].

CONCLUSION

Are these philosophical considerations relevant to scientists and others concerned with the development of an understanding of science in general and of adhesion in particular? A scientist who recognises that a contemporary consensus will never represent the only possible interpretation of experimental evidence is likely to be more open minded, and potentially creative, than one who believes we have reached, or are close to, a "state of finality." It is no less important for the public at large to recognise that "experimental findings are inherently defeasible." The uncritical attitude to "what science has proved" has had disastrous consequences in areas of health, environmental welfare, criminal justice[24] and, no doubt, adhesives technology.

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