Time Loss Through Gating of Asynchronous Logic Signal Pulses

IVOR CATT

Abstract—The gating of asynchronous signals causes logical errors. It is possible to reduce the frequency of these errors, but the price paid is a severe loss of time and extra cost in hardware.

Consider Fig. 1. Suppose \( B \) is an asynchronous signal entering some clocked logic with clock \( A \), and it is brought into synchronisation by ANDing \( A \). There is a statistical possibility that error will occur, as indicated by the special case in Fig. 2, because the flip-flop may enter its metastable (half-stable) state for an extended period of time. As a result, the system will still get out of step. For discussion of this metastability see Appendix I.

The gating of asynchronous signals will always carry a statistical chance of logical failure, because it carries with it the risk of the appearance of a half-amplitude, half-true logic signal. The interaction of asynchronous signals with clocked logic is a well-known phenomenon [4, 8]. There is a statistical possibility that error will occur, as indicated by the special case in Fig. 2, because the flip-flop may enter its metastable state for an extended period of time. As a result, the system will still get out of step. For discussion of this metastability see Appendix I.

Figure 3 shows the circuit (A+B) tries to set FF1 at time \( t_0 \). At time \( t_0 \), after a delay of length \( s \), the output of FF2 is interrogated, and it is hoped that FF1 will be set at that time have left its metastable state and be in one of its stable states, so that during one of the clocks \( E \), FF2 will be set unambiguously. The probability that at time \( t_0 \) FF1 will be set will be unambiguously set, and therefore, that a failure will occur in spite of its protective circuitry, is computed in Appendix I. It is easy to make this probability acceptably low. Unfortunately, the time \( t_0 \) for most processes is unacceptably large.

mittent failure resulting is more serious than a catastrophic failure, because it introduces undetected errors.

The following is a description of how to reduce the failure rate to acceptable proportions. A good rule of thumb for acceptable failure rate for a logical circuit is one failure in about 1000 days, or three years.

In Appendix I, it is easy to make this probability acceptably low. Unfortunately, the time \( t_0 \) for most processes is unacceptably large.

APENDIX I

Discussion of the Metastable State

Figure 4 shows the transfer characteristics of the two halves of the bistable. The transfer characteristic for the first half is drawn in the first and third quadrants. The transfer characteristic for the second half is drawn in the second and fourth quadrants. The transfer characteristic is metastable state. The bistable can continue indefinitely in this state; it is possible even in a noisy environment. There is a certain pattern of random noise which will alternatively move the bistable to one side and the other of its metastable state, but not far enough for it to move to one of its two stable states.

It is possible for the bistable to continue indefinitely in its metastable state if the transfer characteristics of the two halves of the bistable differ. Figure 5 shows the case where the first half of the bistable has a low threshold and the second half has a high threshold. The metastable state is where the two curves intersect.

The dotted line in Fig. 4 shows the case when the spike in Fig. 2 has pushed the bistable once to its metastable state in this case, slightly further than the metastable state. We can see the events that follow by going clockwise, starting with the first quadrant. In this case the bistable gets out of the transition region in a time \( t_0 \) or we round the loop in the bistable.

The practical case of a metastable state will be more complex than the steady-state picture described previously. In practice, the metastable state might be represented by an oscillation of the bistable outputs about a mean level represented by the steady metastable state. This is the situation investigated in Appendix I.

APENDIX II

Calculation of Failure Rate

In Appendix I, it is assumed that the number of times per second that \( B \) goes true (and, therefore, there is a chance of failure) is \( f \). A reasonable figure for \( f \) when using unprotected data transfers from a number of tape stations into a computer would be \( 10^9 \).

If we are considering a three-year period (equals about 108 seconds), then the number of times \( B \) goes true is about \( 10^8 f \). If
Thus, $R$ represents the proportion of the small spikes which will be of such amplitude as to partly succeed in setting bistable FF1, and so FF1 enters its metastable state ($180^\circ$ phase shift) within three years.

We now have the picture of a roughly half-scaled pulse reciprocating around the feedback loop of the bistable FF1. We may assume that there is a regular distribution of amplitudes among these pulses, starting with small ones which just get into the transition region, through pulses nearly half way through the transition region, to large pulses which are only just below the top of the transition region. Each time this pulse passes around the loop, it is amplified twice. If the amplification in each half of the bistable is $B$, the amplification around the loop is $B^2$. In one trip around the loop, a number of pulses will drop out of the transition region. After $n$ trips around the loop, the number of pulses still within the transition region will decrease by a factor $1/(10^P)$.

Now we must wait to lose that small pulse remains in the transition region during three years. This means that:

$$10^P R^2 = \frac{1}{10^3}$$

Therefore, the number of times that the pulse must be allowed to circulate round bistable loop is:

$$n = \log \frac{10^P R^2}{F}$$

The time $t$ in Fig. 3 is $n \times d$ where $d$ is the delay round the loop of the bistable.

Example

As an example, let us substitute the following values into the formula:

- Number of times per second that signal $B$ arrives $= 10^9$
- Transition width $= R = 10^5$
- Signal logic swing $= 10^6$
- Frequency bandwidth of the transistor $= F = 10^4$
- Gain of one half of the bistable $= 5$

Delay round the bistable $= d = 10/F = 10$ ns. Then,

$$n = \frac{1}{2} \log \frac{10^9 \times 10^6}{10^5 \times 10^{-10}} = 10^2 \times 10^{-20} = 6.6$$

Now $d = 10$ ns,

$$t = n \times d = 66 \text{ ns}$$

Worst-case delay

The worst-case (longest) delay will be greater than 64 ns for two reasons:

1. In Fig. 3, the worst-case delay is $(2t + d)$. This equals $2t$ plus $(10/20)$ 20 ns. This 20 ns allows for the length of pulses $R$ and $d$ and also the gap between them, so that we have a delay of $2t + 20$.
2. The 66 ns was computed on the basis of various tentative assumptions. The author considers that for safety, it should be doubled. This gives a total delay of $4t + 20 = 276$ ns.

Thus, we end up with a worst-case delay of about 300 ns. Note that this is when using transistors with a frequency bandwidth of the order of 1 Ge/s.

APPENDIX III

DISCUSSION OF THE SOLUTION TO THE PROBLEM

Figure 3 shows the circuit and timing diagram which solves the problem as far as it is possible to solve it. What is necessary is to get the logic signals out of the transition region as they enter the system. The basic element of a successful circuit is a very high-gain digital stage. That is, we need an element with a very narrow transition region. This can be achieved by placing a number of logic elements in series. However, it seems easier to use only two logic elements thus coupled. The result is a bistable FF1.
**DISTORTION AT THE AMPLIFIER–SPEAKER INTERFACE**

The problem of "intermodulation distortion" at the amplifier–speaker interface was a common issue in audio systems. This page from a 1980 issue of *WIRELESS WORLD* discusses the topic in detail.

**THE DEATH OF ELECTRIC CURRENT**

The page discusses the death of electric current as a concept due to the advancement of digital audio and the increased use of digital signal processing.

**TELEVISION SETS FOR THE DEAF**

The page explores the development of television sets designed specifically for the deaf, highlighting advancements in audio-visual technology for improved accessibility.

**HERBERT DINGLE**

The page features an obituary for Herbert Dingle, a notable figure in the scientific community, detailing his contributions and influence on science and technology.
THE SCIENTIFIC REFEREE SYSTEM

M.H. and B.R. MacRoberts
740 Columbia, Shreveport, Louisiana 71104, USA.

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Abstract

There has been very little written about the scientific referee system but a lot has been implied. It seems to be widely believed that the system works well, even though there are cases of disparate judgement. These however are usually explained away in an ad hoc fashion. We find that novelty is characteristically resisted by scientists and suggest reasons for this.

1. INTRODUCTION

The referee system began in the seventeenth century in England in the form of a collator who saw papers through the press. Because of the low quality of many manuscripts, this system was soon replaced by editing and refereeing. The practice of evaluating the substance of manuscripts soon developed. Referees were chosen on the basis of their expertise. "Almost from their beginning, then, the scientific journals were developing modes of refereeing for the express purpose of controlling the quality of what they put into print."(1). This is the current practice. Each journal has an editor who receives and judges papers and passes them on to reviewers for judgement.

Because short articles in journals have become the major publication method in science, referees and, to a lesser extent, editors have become the "lynchpin" about which the system pivots. This being so, it is not difficult to see the importance of editors and referees to the process of scientific communication and scientific advancement. The question that we raise here — and one raised by others — is how well does the system work?

2. THE RECEIVED VIEW

The majority opinion, which we will call the "received view", is that although the referee system does not work unfallingly, it approximates the ideal. This view is implicit or explicit in much of the writing of those sociologists who make the institution of science their speciality. (2, 3) The assumption underlying this view is that scientists are largely apodictic thinkers, testing ideas against nature. The individuals who act as referees are believed to be objective, disinterested, sceptical, sympathetic, open, tentative and hospitable to change. (4) Because of this and the fact that they are "experts", referees should be nearly infallible judges of work that falls within their areas of specialization. Papers published in "reputable" journals therefore "bear the imprimatur of scientific authenticity" because they do not merely represent the opinions of their authors but also those of the editor and the referees. (5) Consequently, the practice "of monitoring scientific work before it enters into the archives of science means that much of the time scientists can build on the work of others with a degree of warranted confidence." (6)

According to the received view, papers submitted for publication fall along a simple continuum. At one end are the very bad ones, written presumably by dullards; at the other are a few excellent papers, the "cognitive products" of the elite. In between are the majority, "bad work", as Darwin (7) called it. The editors' and referees' task is simply to assign each paper to its position and decide on a cut-off point.

Adherents to the received view, however, do not overlook the fact that the system does not work with unfalling effectiveness, but — and this is the point — their treatment tends to dismiss incompetent judgements as ad hoc events. Consequently, the received view does not address itself to the possibility that intellectual bias is an important factor in the assessment and acceptance of scientific work.

Traditionally and typically, supporters of the received view have painted a reassuring picture, one mirroring the Panglossian mode in which almost everything that happens, recurrent or otherwise, is seen as basically for the good. Anomalies are distorted into isolated and unusual events or are sometimes even construed as being of positive value, that is, when anomalies are admitted, some institutional necessity is used to justify them. (8)

Therefore, it would seem that the received view would predict that the rejection of important, novel work should be infrequent. Certainly important work should not be repeatedly rejected. Figure 1 (below) approximates what we take to be the received view. Scientific progress in this view is easily understood.

3. THE MULTIPARADIGM MODEL

The necessity for a different interpretation than is provided by the received view begins with the realization that the frequency with which good papers are rejected and the extent to which scientists resist new ideas are not accounted for by the received view. (9) Inconsistencies between what occurs and what is predicted to occur are too major and too frequent to be explained away as occasional mistakes. In fact, what the "mistakes" suggest is that they are not mistakes at all, for as Taton (10) has shown, "the number of revolutionary discoveries which came into their own, only after hard battles, is legion."
We therefore reject the received view and propose another interpretation of these events. According to this interpretation, scientists are not unbiased, objective, skeptical, disinterested, sympathetic, open, tentative or hospitable to change, but the opposite. Scientific judgement is in terms of prevailing opinion. It is paradigmatic. Put differently, scientists are "encapsulated," or as Hoyle has said, "straight jacketed." Kantor puts it simply, "established doctrine in science is more powerful than factual evidence."

Although Kuhn is responsible for popularizing the notion of paradigmatic science, this conceptualization has been around for a long time and has been variously expressed, for example by Kantor and by Koestler. Koestler perhaps has captured the idea behind the concept of paradigmatic commitment as well as any. He sees typical paradigmatic behaviour as involving a "cognitive matrix with a distorted logic, the distortion being caused by some central axiom, postulate or dogma, to which the subject is... committed, and from which the rules of processing the data are derived." He continues, "The amount of distortion involved in the processing is a matter of degree... it ranges from the scientist's involuntary inclination to juggle with data as a mild form of self-deception, motivated by his commitment to a theory, to the delusional belief-systems of clinical paranoia... But," he concludes, "to undo a mental habit sanctified by dogma or tradition, one has to overcome immenseley powerful intellectual and emotional obstacles." Thus the scientist is committed to a world view from which reality is constructed, and things are fitted or "twisted" as Kantor has said into conforming with that view.

Scientific work in these terms can be depicted as a three-dimensional field, as in Figure 2. The large hill in the foreground represents the major paradigm of a discipline, for example, Functionalism in sociology. Most scientists accept the major paradigm, and when judging the work of others, they do so on the basis of its approximation to the standards of the paradigm. To the rear of the field are several small hills, which symbolize small coteries pioneering new approaches. Members of such coteries are working under assumptions different from those of the majority. These assumptions are not understood by adherents to the major paradigm, yet members of the coteries understand the major paradigm, for they work out from it — in fact, it is usually this they are rejecting.

![Figure 2. Multi-paradigm model](image)

Visualized two-dimensionally, scientific papers do not fall into a single bell-shaped curve but rather into a series of curves in which there are good and bad papers (see Figure 3 above). Most referees can only judge papers that fall into one or two curves. But they attempt to judge all papers on the basis of their own standards. For example, if the referee is a mentalistic psychologist, to suggest that the current view of the nature and function of the central nervous system is traceable to the Patristics is completely ascientific is to kill the paper before it is even born. Again, to suggest that Newton was badly confused about the part played by light in vision and that instead of seeing light, we see things by the medium of light, is to ask for trouble, and so on.

What is being said by a heretic seems ridiculous to adherents of the accepted paradigm. To them, papers that are novel often appear to be unpolished, simple-minded, improperly referenced, misguided, poorly conceived, amateurish, or just plain wrong, depending on the degree to which they deviate from the paradigm. Their authors are considered, as Van Valen and Pitelka put it, to be "presumptuous." But the reason for this is that the new knowledge or point of view is not part of the paradigm or goes directly counter to what is thought to be "fact." Therefore, the innovator is judged wrong or incompetent by the fact of showing glaring deviations from what is accepted or acceptable.

Storer captures the essence of the problem when he says, "A contribution must be an understandable extension of knowledge, and if it fails to meet this test, either because others simply cannot understand its relation to current knowledge or because it seems to fly in the face of accepted standards of reasoning, it will be rejected and its author's credentials as a scientist will become suspect. This means that a scientist who is so far ahead of his colleagues that they cannot understand how he arrived at his conclusions or why they are important will be treated in the same way that a hollow-earth theorist is treated."

Because journals are usually controlled by adherents to the major paradigm, the consequences of submitting non-paradigmatic work are predictable.

4. CONCLUSION

While the referee system came into being in order to separate the trivial and incompetent from the competent at a time when it may have been possible and perhaps necessary to do so, the system also immediately became an
obstacle to creativity and innovation, for although it does sort, it does so not only on a good-bad basis but also on a paradigmatic-nonparadigmatic basis, and herein lies its great and inherent weakness.

Whatever positive aims it is ensnared by instituting the referee system and whatever positive goals it achieves, the seeds of dysfunction were sown at its inception, for given the paradigmatic nature of science and the encapsulation of scientists, the natural outcome can only be resistance to innovation and rejection of novelty. The weak links in the system are the referees themselves, for they like all scientists have repeatedly shown themselves to be unable "at any given time to distinguish between an idea that is entirely wrong and an idea that may be received as brilliant at some later date." [25]

In conclusion, let it be said that even though it would be hard to find a scientist who is not aware that some good papers are rejected and some novelty is resisted, it would also be hard to find a scientist who altered his own behaviour because of this knowledge. It is not surprising that individuals who have not experienced resistance accept the received view. Not having had work rejected for paradigmatic reasons, it is easy for them to say that this either does not occur or that it represents an unusual event in science. The Nageli-Mendel incident illustrates the problem perfectly. Here is the "Father of Genetics" trying patiently to explain what he has found to the expert on plant hybridization, only to be rebuffed and sent back to his garden. If supporters of the received view can only imagine this incident multiplied a thousand times and momentarily identify with Mendel, perhaps they can grasp the magnitude of resistance and the utter frustration that is involved in attempting to explain something that is novel to someone who is simply incapable of understanding — but someone who theoretically should be capable of understanding. Neither Nageli nor anyone else had any comprehension of what Mendel had discovered, no matter how Mendel explained himself, until quite independently three individuals rediscovered genetics. Others had to repeat Mendel's work, that is, they had to experience what he had done, before they could understand what he had said. All scientists should realize that when they review others' work, they are potential Nagelis. But characteristically scientists persist in failing to learn from history and so failing are condemned to repeat the past. Surely the "expert" must realize that if the history of science teaches nothing more than that it contains in the Nageli-Mendel incident, it behoves him to admit to the possibility that he himself may be a little short of omniscient. Perhaps with this first admission, he can go on to teach himself to say, "I do not know".

References

5. Ref. 1, p.491.
6. Ref. 1, p.495.
11. See Mullay, K., Some aspects of cultural growth in the natural sciences, Social Research, 36, 22-52 (1969). Our conclusions are very similar to his. See also Lindsay, D., The Scientific Publication System in Social Science, Jossey-Bass, San Francisco (1979), for a critical study of some aspects of the refereeing system in the social sciences. This publication also contains a literature review.
18. Ref. 17, p.289.
20. Ref. 17, p.179.

Dear Mr. Catt,

Thank you for your letter and reprint. I'm glad that the referee paper was of some interest. The best work on the subject is Lindsay (see note 11) who summarizes most work up to 1970 or thereabouts. I don't know of any major work on this that has been done since then although one can find dissenting voices usually in the form of letters to editors in such journals as the New Scientist; e.g., Physics Today, 1979 (April), Vol 32, 14-15, New Scientist, 16 July 1981, 176, but these amount to very little. I would be interested in your "The rise and fall..." paper.

Michael