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In re: Experimental Analysis

William Brooks

This article begins by considering the interaction of notation and analysis by means of a comparison of scores from the 1950s. This discussion leads to the domains of error, value, and ethics. An 'experimental' mode of analysis is proposed, which takes taxonomy and statistics as a basis; from this one acquires an understanding that is observational rather than evaluative. A philosophical basis is noted in pragmatism, in the writings of James and Peirce. The consequences of this approach are explored through detailed study of Music for Piano and the compositional process that Cage employed; certain anomalies emerge.

Keywords: John Cage; Analysis; Experimental Music; Error; Pragmatism

I

I was never analyzed. I'll tell you how it happened. (Cage, 1961, p. 127)¹

Let me begin with a little miniature, for harp and string quartet, from the mid-twentieth century (Figure 1). How might I analyze this?

Well, I might start by noting that the piece falls clearly into three sections, separated by unmetered silences of nine seconds. There are also three clearly distinct timbres: bowed strings (*non vibrato*) in the violins and viola, pizzicato in the cello, and a special, nasal (*pres de la table*) color in the harp. All the plucked sounds (harp and cello) are meant to ring, and the cello timbres seem to cover something of a spectrum in themselves, from the open C-string, which will ring like the harp, through an open-string harmonic (bar 10), to the fully stopped, dull thuds of the upper-register pizzicati at the beginning and end. Moreover, certain bowed sounds are shaped almost in imitation of the harp envelope, as if they were plucked (notably the viola C#s in bars 14–18). There seems, then, to be an interest both in clearly differentiating material (of three types, it would seem) and in suggesting a rapprochement between them.

Doucement ♩ = c. 88

The musical score is divided into two systems. The first system includes staves for Harp, Violin I, Violin II, Viola, and Cello. The Harp part begins with a chord of D, C, B♭, F, G, A. The Violin I part has a 'non vibrato sempre' marking and a dynamic of *mp*. The Violin II part has a 'non vibrato sempre' marking and a dynamic of *p*. The Viola part has a 'non vibrato sempre' marking and a dynamic of *p*. The Cello part has a 'pizz.' marking and a dynamic of *mf*. The second system includes staves for Hp (Harp), Vln I (Violin I), Vln II (Violin II), Vla (Viola), and Cello. The Hp part has a 'L.V. (sempre)' marking and a dynamic of *mf*. The Vln I part has a 'non vibrato sempre' marking and a dynamic of *mp*. The Vln II part has a 'non vibrato sempre' marking and a dynamic of *pp*. The Vla part has a 'non vibrato sempre' marking and a dynamic of *p*. The Cello part has a 'non vibrato sempre' marking and a dynamic of *mp*. The score is in 4/4 time and features various dynamics and articulations.

Figure 1 From the Mid-Twentieth Century.

This impression is confirmed by a closer look at the pitch material. The opening section (bars 1–2), with a closely voiced E-major triad followed by the pitch A, seems to suggest a tonal analysis; the F which follows perhaps implies the key is A minor, rather than A major. The final section, in which a reiterated F♯–E is again followed by an A, is tonally consistent with the opening, though the mode might be major; but again a minor modality is affirmed by the note which follows, a C♯. The center section, however, is quite different: measures 4–10 work exclusively with a whole-tone complex, while measures 12–20 seem to imply a tonality built around

The musical score for Figure 1 Continued consists of two systems of staves for five instruments: Harp (Hp), Violin 1 (Vln 1), Violin 2 (Vln 2), Viola (Vla), and Cello. The first system covers measures 14 to 21, and the second system covers measures 22 to 22 (likely a typo for 23). The time signature changes from 3/4 to 5/4 at measure 22. The key signature has one sharp (F#). Dynamics include *mp*, *p*, *mf*, and *pp*. The score shows various musical notations including rests, notes, slurs, and dynamic markings.

Figure 1 Continued.

C#. Hence the pitch material, like the sections and timbres, is divided three ways, with roughly the same length of time (overall) devoted to each type of material; A minor, however, is given the greatest emphasis by its placement at beginning and end, creating a kind of A-[B1-B2]-A form.

Like the timbres, the pitches suggest a rapprochement or reconciliation. The F that concludes the first section also begins the second; the two types of material in the center section are linked with a held B. Only the final section is unprepared. Material is also linked by the voicing: the closing sonority of the opening (E, F, G#, A) is a close-voiced symmetric chord; the sustained notes in the following section (C#, G, F, B) form

another symmetric chord, though in a more open voicing; and the next aggregate sonority (mm 14–16: C♯, G♯, B, F♯) is again symmetric and even more widely spaced. Again only the final section is different—but that, of course, is strongly linked to the beginning through its pitch content.

Shifting to a larger perspective, we might note that the same ambivalence, between links and distinctions, is played out in the perceived trajectory of the work. Although time and (to some extent) pitch define a three-part, symmetric structure, the timbral form is developmental, moving from sustained to plucked sounds. In the opening section there is one and only one pizzicato; in the final section there is one and only one arco. Moreover, the nasal, penetrating harp plays a more and more prominent role; wholly absent in the first section, it supplies a single note in each of the B sections, and then completely dominates the close. Hence, though one would be inclined to hear the work architecturally, with balanced and symmetric sections, one is also drawn towards a more continuous narrative, in which the harmonious opening is increasingly disturbed timbrally, registrally and even tonally (especially with the quite unsettling final B♭).

We could go on; but this suffices for my point. What I have just offered is a wholly conventional analysis of a seemingly conventional piece. We do this sort of thing all the time, even though we might not always admit it: confronted with a new score and limited rehearsal time, we try to grasp the essence of the piece quickly and efficiently, making analytical judgments to guide interpretive decisions. And this sort of analysis serves that purpose well; we determine (we hope) the character of the work, and from our determination we are able to form and test hypotheses about various un-notated details: how soft? how connected? how to be fingered, bowed, played?

Somewhere in this process we form a set of conclusions about *value*. We might hesitate to express these directly, but they affect our behavior and inform our decisions. In the example just given, for instance, at least some of the value that attends this piece follows from its balance, from the three-part structure. We might conclude, then, that special effort should be made to preserve or strengthen that; we might decide (as an instance) that the symmetric fermatas really do need to be carefully timed, so that they are the same length. We might conclude, if pressed, that giving the second fermata only a six-second duration would detract more from the value of the piece than dropping three seconds somewhere in bars 10–13. Or, in another instance, we might think that it would be an especially ‘costly’ error if the final note of the first section were misplayed as an F♯, since the correct note not only confirms the A-minor modality but also creates a common-tone link between the first two sections.

An analysis of this sort is informed by at least two sensory streams. Our eyes perceive an image, and our ears (perhaps in our imagination, perhaps at a keyboard or a rehearsal) hear sounds. Both are regulated by certain learned conventions, and both are shaped—confined, perhaps—by those same conventions, in a looping, reiterative stream. I am inclined to a wholly conventional analysis for this work because it *feels* wholly conventional; and because it feels conventional, I discover patterns that are comfortably familiar. I am inclined to hear the music tonally because the *feel* of an E-major, second-inversion triad carries me in that direction; and because I hear

But what happens when one is surprised, when a score looks or *feels* strange?

2

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Figure 2 Also from the Mid-Twentieth Century. © 1960 by Henmar Press Inc., New York. Reproduced by permission of Peters Edition Limited, London.

To be sure, we have staves, clefs, notes, accidentals, and playing instructions. But there are no barlines, and there are no visual cues to sections or divisions in the continuity. All the noteheads are the same, apart from calligraphic variations. The unconventional clefs in system 2 make it difficult to grasp pitch relations; the note-by-note playing instructions impede our desire to find a narrative.

We might go to a piano to help our internal ear in this unfamiliar environment. Playing the first three notes, we would sound the keys that correspond to B, E, and G $\sharp$, but we would be unlikely to *feel* an E-major, second-inversion triad; the spelling is wrong, the noteheads are unconnected, and we are disinclined to apply the conventions we have learned. Then again, though we might observe that we press the same key (F) to produce the antepenultimate and ultimate notes on the first system, we would not infer a connection between sections. No sections are evident; or if there *are* sections in this piece, they seem to arise from the practical act of returning our eyes to the left of the page—four systems, four sections. They exist in operational terms only, and their validation (if any) resides in the embodied habit of reading left-to-right.

We would probably conclude that whatever *value* this piece has, it does not reside in balance and symmetry. Nor is it to be found in a sophisticated mix of distinctions and links. It follows that the value is not diminished if the time allocated to the empty spaces is incorrect; nothing is lost if the first long silence is arbitrarily shortened. And nothing would appear to be lost if the muted note on the first system were misplayed as an F $\sharp$; there are no especially ‘costly’ errors to avoid in performing this work.

Yet [Figure 1](#), as is surely obvious, is a very faithful transcription of [Figure 2](#). Presented with correct recordings of both, given as a dictation exercise, an assiduous student would probably produce nearly identical scores, allowing for the instrumentation. And if we were given the two scores that that student made, our analyses of them would also be nearly identical.

[Figure 2](#) is *Music for Piano 4*, by John Cage. Is it ‘wrong’ to analyze this as a symmetrical construction, in three sections, beginning and ending in the key of A? If so, why? And if not, why don’t we do it, then?

II

Composing’s one thing, listening’s another, performing’s a third. What can they have to do with one another? (Cage, 1961, p. 15)

The link between composing and performing is situated in part in the *score*. In the Western art-music tradition with which we are presently concerned, a composer does not, in fact, make ‘music’—that is, an artwork in sound. The composer makes a score; using this, the performer makes the ‘music’. And this music is heard by listeners who have no knowledge of the score, but who—we usually assume—somehow understand from the auditory experience some of the properties the composer created and set down in notation.

The binding agent in this entire process is *analysis*. The performer analyzes the score and the acoustic event (heard, possibly, in the imagination); that is exactly how we approached Figure 1 above. Both phenomena—the ‘object’ perceived by vision and the ‘music’ felt in the ear—inform the analysis, which is judged in part by whether it explains the presumed relationships between them. The composer, likewise, stipulates a performer and a listener and attempts to make a score that not only stands in an unambiguous relationship to the felt music, but also contains visual clues that will suggest a plausible analysis to the performer. And the listener, even though unconcerned with the composer’s score, will very likely construct a different object—a memory, a narrative, a picture of the felt experience—by a kind of analysis of the sounded music.

Cage, however, wants to decouple composing from performing from listening. The composer makes a score; the performer makes music; the listener listens. The looping, reiterative stream that analysis perpetually reconstitutes is diverted into three still ponds, wholly disconnected. When these are linked—when we impose an analysis—we violate Cage’s intention; we make his work conventional. That is *wrong*, not for esthetic reasons but for moral ones; it is *wrong* to force a person to be other than he is, to attribute to him views that are not actually held. To divide *Music for Piano 4* into three large sections, the first and last of which are in the key of A, is akin to dividing the night sky into constellations and elaborating these with stories; the result might be entertaining, beautiful or illuminating, but *we* put it there, not the creator.

With analysis denied, *value* can no longer have the place it had before. *Music for Piano 4* with a misplayed F is indeed no more or less valuable than a ‘correct’ *Music for Piano 4*, if we consider value to reside in the relations between musical components—in the analysis, in fact. About another, roughly contemporaneous piece, Cage writes:

Value judgments are not in the nature of this work as regards either composition, performance, or listening. The idea of relation ... being absent, anything ... may happen. A ‘mistake’ is beside the point, for once anything happens it authentically is. (Cage, 1961, p. 59)

But that does not give the performer license to be negligent; there are still ‘wrong’ performances, in two senses.

The first sense is, again, ethical or moral. A score is, among other things, a request for certain actions; and a performer of a score undertakes, among other things, to perform those actions. It is ethically wrong to knowingly perform different ones, just as it is ethically wrong to knowingly violate a promise one has made. An error is an error, in either case; but if an error could have been prevented (by better preparation, for instance), the performer is, simply, *wrong*. Indeed, care in preparation—discipline, devotion, faithfulness—serves as a new indicator of value: a disciplined performance is better than a casual one, regardless of what sounds are made.

The second sense is semiological, and here I borrow from Charles Saunders Peirce. The noteheads in [Figure 1](#) are *symbols*, in Peirce's sense; when I suggest that the A is a kind of tonic, that is a relational assertion and resides in me, the 'interpretant'. In a different situation I might make quite a different claim. But the noteheads in [Figure 2](#) are *indices*; they merely point, without relations, to a particular sound or action. The A, in this case, merely says 'this one'; the E, 'this other one'; and so forth. It is 'wrong' to point incorrectly, in the same sense that it is wrong to point at petunias when talking about pansies. The error here is neither esthetic nor moral; it is logical, as one might erroneously file a letter from 'Adam' in the cabinet headed 'B'. It doesn't necessarily detract from the value, but it misdirects; it requires correction, just like the misfiled letter.

Because Cage's notation is indexical, because it explicitly eschews *relations*, his music is often described as *experimental*. In a recent article (Brooks, 2012) I discussed that adjective at length; here I need only note the distinction drawn there between 'test' and 'observation'. A test is relational and judgmental; an object or action is encountered and then accepted or rejected because it possesses certain properties relative to other objects: it is faster, smoother, a better fit. Observation, in contrast, is non-relational and non-judgmental; observing, one simply sees what is there.

'Experimental' music, as Cage came to use the term, is pure observation. In *Music for Piano 4* the listener hears (observes) 28 sounds. These are the sounds that are indexed by the 28 noteheads that the performer observes. These were notated by the composer, who (it turns out) observed 28 imperfections in a piece of paper. There are no symbolic connections among any of the 28 items at any stage, and composer, performer, and listener are linked only by indexical signs—markers of 'is-ness'.

There is no place in this music for analysis of the sort we applied to [Figure 1](#). That analysis was concerned with value and with testing; I arrived at the idea of a tonal center by examining several hypotheses about pitch relations—relying, of course, on prior experience—and then choosing the one that seemed 'best'. In biological terms, I might have been describing a kind of ecosystem: I am not interested in what the pitch A *is*, but rather in how it functions in a complex system of interconnected elements.

But there is another kind of analysis—simpler and more 'experimental', in the sense given above. This 'experimental analysis' is taxonomic, not ecological; it is concerned only with descriptions and quantitative comparisons. No relations are imposed, no judgments are implied; the analysis serves only to clarify the observation of what is there. Just as Linnaeus might have measured the stamens and pistils of several floral specimens, sorting and counting them by length or shape, so can we study the sounds and the notation, creating an inventory of details. And the properties of the aggregate can tell us something about how it came into being—about what John Cage observed, and when, and how the *process* of observing determined the result.

Shall we begin?

~

Certain aspects of the notation of *Music for Piano 4* are wholly conventional. The placement of noteheads on five-line staves determines the actions to be taken (the keys to be played), and hence (in this case) the pitches to be produced. Our

understanding of the staves is mediated by clefs; in *Music for Piano* these are placed, as is conventional, at the beginning of each staff, and the staves are visually disposed in pairs, as is usual in the 'grand staff' used for piano music. However, the staves in the pairs are not joined by bars or braces, and the clefs do not necessarily appear in the conventional order (treble above, bass below). The grand staff appears to have been broken into constituents (staves and clefs) that recombine unpredictably, much like free radicals made from stable molecules. The relational implications of the grand staff (for instance, melody and accompaniment) are thereby denied; we need to observe staff and clef independently, rather than joined in a single unit.

The actions following from staff position are adjusted by conventional symbols (sharps, flats, and naturals) that are also placed conventionally. However, there is no consistency in their use (F♭ and E♭ both occur), and there is no shorthand (no key signature). We might conclude that the relational implications of accidentals (key, scalar function) are also denied; and again we are led to treat accidentals and noteheads separately, as free radicals that combine to produce stable, performable units.

Finally, we can note the presence of three playing techniques: 'pizz.', 'mute', and an assumed 'ord.' that is applied in the absence of any other indication. These techniques are themselves unconventional in piano music, and they too seem to be independent; there is no evident association with any other notational component.

In our taxonomic analysis, then, in homage to Linnaeus, we might think of a kind of species (the piece) with 28 individuals (the notes), each defined by different combinations or quantities of physical properties. We are better able to *observe* if we approach the music in this way, rather than as an undifferentiated continuity. But, to reiterate, we assert nothing about hierarchy, relationships, or value; we merely see more clearly, as through a magnifying glass.

With this taxonomy it becomes possible to gather some quantitative data, to investigate the characteristics of our 28 individuals (notes) in aggregate, so that we can learn something about the species that is the piece. Treating separately each of the constituent units we have identified, we find that: (a) there are 6 bass clefs, 2 treble ones; (b) 18 notes are natural, 6 have a sharp, 4 have a flat; (c) 7 are 'pizz.', 7 are 'mute', and 14 are played normally; and (d) the number of letter-names (for pitches) is 4 for A, 4 for B, 5 for C, 1 for D, 2 for E, 7 for F, 5 for G. The sample size (especially for clefs) is too small to generalize convincingly, but it is nonetheless striking that a sizeable majority of notes are natural, exactly half are played normally, and the distribution of letter-names is fairly uniform. Possibly these are properties of the species, considered as a whole.

We might ask how this genus—this piece—came into being. And here it becomes useful to expand the field; *Music for Piano 4* is actually part of a set of sixteen pieces (hence the '4–19' at the top of the score)—16 *species* forming a *genus*, as it were—and this set of 16 is itself part of a *family* that contains five related sets: 4–19, 21–36, 37–52, 53–68, 69–84. (*Music for Piano 1*, 2, 3, and 20 are also part of that family but will not be discussed here.) We cannot here embark on a history of the entire family; for the present it suffices to note that, although to my knowledge John Cage never wrote or spoke specifically about *Music for Piano 4–19*, he did write about the

creation of other families; and we can assume that his explanation for these might apply, at least in part, to *Music for Piano 4*.

And so we turn to 'To Describe the Process of Composition Used in Music for Piano 21–52' (Cage, 1961, pp. 60–61), where Cage sets out a procedure in seven stages. First, the score-paper was made, as a master that will eventually be traced. The eight five-line staves are positioned so that each has room for nine ledger lines above and six beneath, so that each can be either treble or bass clef and the whole range of the piano is included. (The paper also included a single line between pairs of staves, used to indicate extraneous sounds; but this is not included in *Music for Piano 4–19* and we will ignore it for the present.) Second, a chance procedure is used to determine the number of noteheads to be inscribed on a given page (a piece, a 'species'). Third, a blank sheet of transparent paper is chosen, and a corresponding number of imperfections are identified and 'intensified with pencil'. Fourth, the transparent page, with intensified imperfections, is overlaid on the master page; using ink, first the staves are drawn and then the penciled imperfections are converted into noteheads, with ledger lines drawn when necessary. Fifth, a clef for each staff is determined by tossing a coin. Sixth, chance operations are used to determine a distribution of normal, plucked (pizz.) and muted notes; then a series of random numbers determines the outcome, note by note, according to the distribution. Seventh, a 'similar procedure' determines whether a note is natural, sharp or flat.

Shall we conclude that this is the procedure that brought forth *Music for Piano 4*? Our experimental, taxonomic analysis can test this hypothesis, to some extent. But to be more statistically robust, we might use all of *Music for Piano 4–19*, thus multiplying our data more than tenfold. In so doing, of course, we make the assumption that each 'species' in this 'genus' of pieces was made the same way—a plausible surmise, given the titular grouping and the overall similarity of appearance. Thus, though *Music for Piano 4* might seem to challenge the use of coin tosses for clefs—since it contains three times as many bass clefs as treble—the 128 clefs in the whole 'genus' divide into 58 treble, 70 bass; and this is well within the range of statistically probable outcomes if coin tosses were indeed used.

However, the data about playing technique suggests something quite different. Cage, we recall, set the proportions independently for each piece (each 'species') in the sixth stage of his process (above). The proportion of plucked, muted, and normal notes would thus become one of the identifying markers that distinguishes each of the 16 species (each piece) from the others; the proportions would be expected to vary quite widely. Over the entire genus, however, there would be roughly equal use of the three techniques (plucked, muted, normal). However, we already noted the strikingly unequal proportions (7:7:14—i.e. 1:1:2) in *Music for Piano 4*; expanding our data to include the remainder of the family (Figure 3), we find the overall proportions to be statistically similar: 93 plucked, 117 muted, 234 normal. Moreover, there seem to be no significant differences among the individual works that make up this family: in all cases except three (nos. 11, 13, and 16, all of which contain a very small sample size, with five or fewer noteheads) there are

Piece no.	plucked	muted	normal	total
4	7	7	14	28
5	10	12	22	44
6	7	19	31	57
7	3	3	13	19
8	12	12	17	41
9	0	1	2	3
10	11	16	21	48
11	2	1	2	5
12	4	10	19	33
13	2	1	2	5
14	13	11	30	54
15	1	4	9	14
16	0	1	0	1
17	2	1	3	6
18	12	12	29	53
19	7	6	20	33
Totals	93	117	234	444

Figure 3 Playing Technique in *Music for Piano 4–19*.

more normal notes than either plucked or muted; and in all cases in which sample size exceeds twenty the distribution approximates the proportions 1:1:2. Of course, it could still be the case that Cage chose the proportions independently and they just happened to turn out to be very similar; but a far more plausible hypothesis is that a single set of proportions was used throughout. Since coin tosses determined the clefs, we might surmise something similar for techniques: perhaps, for a given note-head, a first coin toss determined whether the technique was normal or not; if it was not, a second toss determined whether it was muted or plucked.

Thus we can conclude with reasonable certainty that the procedure for *Music for Piano 4* was not exactly the procedure described by Cage and used in *Music for Piano 21–52*. And, in fact, a similar difference emerges with respect to accidentals: again, if the procedure was ‘similar’, as Cage asserts, we would expect both overall equality and piece-to-piece variation; but again, in the genus that is *Music for Piano 4–19*, we find both consistency across the genus and a skewed, consistent proportion that is again 1:1:2 (a total of 101 flats, 118 sharps, and 225 naturals). We might again conclude that Cage used coin tosses or a similar method to obtain two binary outcomes.

Our analysis enables us to clarify the properties of *Music for Piano 4* and to distinguish it, to some extent, from works that seem to be similar—*Music for Piano 21*, for instance. Again, think of Linnaeus: differences in the details of reproductive organisms enabled classes of plants to be formed and distinguished from others that appeared, on the surface, to be near relatives. But no judgments are applied, no values imposed; *Music for Piano 21* is not ‘better’ than *Music for Piano 4*, any

more than day-lilies are ‘better’ than water-lilies. The analysis only improves observation: through it we are in a ‘better’ position to see (and hear) what is actually *there*.

What has this ‘experimental’ analysis to do with *error*? It tells us nothing about performance and has no bearing on the moral implications of playing F $\sharp$ instead of F $\natural$. But it does raise new questions about ‘mistakes’. These are essentially of three types.

The first concerns the execution of the process that brings a piece into being. We might liken this to the reproductive mechanism in biology; occasionally something goes wrong and a deformed offspring occurs. An instance from Cage’s *Music for Piano* arises in connection with the accidentals applied to the two extreme notes on the keyboard: no sharp can be applied to the top C, and no flat can be applied to the bottom A. Cage acknowledges this when discussing the procedure for deciding accidentals: he notes that it is to be ‘altered, of course, for the two extreme keys where only two possibilities exist’ (Cage, 1961, p. 61). Yet in *Music for Piano 26* the antepenultimate note is a low A $\flat$. This is a deformation that, if strictly interpreted, makes it impossible for this individual—this notehead—to survive. What should be done?

I asked Cage about these sorts of errors in a 1979 interview that ranged across many different works (Cage, 1979). ‘I’m finding mistakes’, I said, ‘would you like to know about them?’ Cage replied: ‘I would. I would find them too. I apparently make them quite easily’. And later I noted (making a mistake of my own), ‘I think in *Music for Piano 23* there’s a low A $\flat$, for example’. Cage responded with interest: ‘Oh, there is?’ Then, shortly after, when I spoke about transposition errors in *Cheap Imitation*, Cage asked to have them pointed out, saying ‘that’s like a typographical error’. The implication—though Cage never said this directly—is that such mistakes should be corrected. The individual should be enabled to survive.

The second type of mistake is manifested in an incompatibility between the composer’s own experimental analysis—such as Cage’s article about *Music for Piano*—and the music itself. In the same interview, speaking generally, Cage remarked that ‘I think there are some more serious mistakes, like compositional errors. ... You could discover [these] through analysis’. Giving an instance from his early work, he explained, ‘In those twenty-five-tone pieces there are some very curious things going on, and I was obliged to revise ... some pieces because of that’.

In *Music for Piano*, too, ‘some very curious things’ are going on. Let me return to the matter of probability distributions, but now with attention to the music that Cage’s article actually addresses. There are two ‘genera’ here: *Music for Piano 21–36* and *Music for Piano 37–52*, and Cage is very clear about how playing technique was determined: ‘The sixty-four possibilities of the *I-Ching* are divided by chance operations into three groups relative to three categories: normal (played on the keyboard); muted; and plucked (the two latter played on the strings)’ (Cage, 1961, p. 61). He then gives an example: if the divisions are 6 and 44, only the numbers 1 to 5 will produce a normal technique; 6 to 43 will be muted and 44 to 64 plucked. This

Family	plucked	muted	normal	total
21–36	207	230	436	873
37–52	79	92	142	312
Totals	286	321	578	1185

Figure 4 Playing Techniques in Two Families, *Music for Piano* 21–36 and 37–52.

process is to be repeated afresh for each page of manuscript—each piece, each ‘species’. Both genera are identically treated in this respect.

It follows from the procedure described that the two dividing points will form normal distributions around 21 and 43 (the ‘thirds’ of 64), and that over time the proportions of plucked, mute, and normal sounds will average out to 1:1:1—that is, they should occur in roughly equal quantities. But the actual totals for the two families are given in Figure 4.

Statistical theory demonstrates that this distribution is astronomically unlikely if the overall proportions are set at 1:1:1—and hence astronomically unlikely if the compositional procedure was exactly as Cage described. The internal variance is also far less than would be expected; indeed, in the first family, there is only one instance (*Music for Piano* 34, which contains only six notes) in which there are fewer normal notes than either plucked or muted. In fact, statistically there is no significant difference between these two later genera and *Music for Piano* 4–19. Except for Cage’s article, there would be no reason to think that the compositional process was not identical for all three genera. The evidence is quite conclusive that Cage did not do what he said he did; either the composed music or his account is ‘wrong’. What are we to do about *this*?

I didn’t ask Cage about that particular anomaly, but in the same interview, Cage did comment indirectly:

My ideas about what it was I did change ... When I set about analyzing something I discover that I didn’t do it as I thought I had done it. ... But then the time has passed and I really don’t know what to do about it, except admit that I made the mistake, and I did something different than I thought I had.

Much later in his life he wrote, more compactly and famously: ‘My / mEmory / of whaT / Happened / is nOt / what happeneD’ (Cage, 1993, p. 6). It would seem that we are asked to trust the phenomena, not his account—to assume that at the time Cage did follow his procedure correctly, but that his account of the procedure is wrong. In this case, he did *not* in fact divide 64 possibilities into three unbiased groups; there was a consistent bias in favor of normal playing. But we must be wary of reading too much into our results; there is no tangible evidence, for instance, that our surmise about coin tosses is anything other than a convenient explanation.

We must also agree that probabilities are just that: probabilities. There is always the exception; someone always wins the lottery, no matter how unlikely the odds. When I proposed to Cage that he had selected accidentals by first dividing the possible outcomes into two parts (accidental/no accidental), he replied:

JC: More apt to have been divided into three.

WB: But it winds up that would be impossible.

JC: Really? ... That's if the theories of probability are right.

WB: Yes ...

JC: I never have any confidence in ... [he pours tea] ... I observed too often that ... ummm ...

WB: That you get strange results.

JC: Yes. Constantly.

There are two ways of reading Cage's response. His suspicion of probability theory could have arisen because he misunderstood it and was forming expectations that the theory actually does not support. Or it could have arisen because he performed so many chance operations that any unusual outcome attracted his notice, as a variegated sprig of leaves draws attention to itself in a field of green. But, crucially, he appears in either case to have accepted the outcome; its unexpected nature is not grounds for 'correction'; there is no need to cut out the variegated branch.

The matter arose again in connection with other data. To assess the distribution of the imperfections on the paper Cage used, I counted the number of notes that appear on the staves and compared it to the number of notes that do not (i.e. those that require ledger lines). If the imperfections were randomly distributed, those numbers should be proportionally consistent with a comparison of empty-space to staff-space, calculated over the entire page. All is well in the first two families (4–19, 21–36), but in *Music for Piano* 37–52, taken as a whole, there is a distinct imbalance: more than twice the number of notes appear on the staves than one would expect. The sample size is large enough to calculate that the odds are less than one in a hundred that this distribution would occur by chance. I questioned Cage quite closely about *Music for Piano* 37–52; I suggested that he used different paper, that he inscribed the staves first, that his method of working had somehow created additional imperfections. Cage was gently insistent that none of these were the case; and in the end we both agreed that the result was, simply, 'marvelous'.

The final type of 'mistake' arises when analysis of a result reveals an unexpected consequence of the process. The most cursory comparison of *Music for Piano* 4–19 with the later genera reveals an obvious difference: the later pieces contain a center line around which will be notated 'noises' produced on the exterior or interior of the instrument. There is no such line in the earlier set. In fact, a more careful comparison reveals that the spacing of the staves is also quite different: while the later pieces have room for nine ledger lines above the staff and six beneath (allowing all keys on the piano to be notated), the early family allows room only for six lines on both sides of the staff. One result is that a different kind of type-one ('typographical') error occurs: in pieces 12 and 18 there appear single instances of seven ledger lines, which (given the space allowed) appear to have been errors of execution. But a more important result is that the highest pitch that can be notated is the highest E on the piano, in the space over the sixth ledger line above the treble staff. The keyboard continues up to

the C above, of course; thus there are 8 pitches (8 keys) for which there is no representation. The process has imposed a filter, an impediment, and certain observations are impossible to make.

This is, in part, what Cage meant by ‘compositional’ errors. They invite correction—not because the omitted sounds are preferable, but because all that can be heard should be available to be heard. Observing birds in the field, we don’t want to be positioned in a way that obscures the sky. In fact, the only way in which one observational procedure can be ‘better’ than another is by being more *inclusive*. And so the remaining families of *Music for Piano* contain a redesigned staff, together with provision for non-keyboard ‘noises’. The observable world has been enlarged.

A similar situation arose some years later for Cage, in devising his ‘subtraction’ method (Brooks, 1993). He was working with early American hymnody, and the problem was to free the individual sounds from the choral, often homophonic texture. As he told the story, ‘My first questions were superficial ... When I got to a piano and tried [the results] out, they were miserable. ... The cadences all remained recognisable’ (Kostelanetz, 1987, pp. 85–86). The *sounds*, that is, could not be heard; they were too firmly locked in a perceptual habit, and the process had not broken them free. The problem here was not mechanical, as in *Music for Piano 4–19*, but psychological; but the effect was similar: the process was unable to expand the field of *observation*, which was already impeded by history. The process had therefore to be corrected.

But such corrections have nothing to do with randomness. Cage was not troubled by bias; he was perfectly willing to contemplate a garden with only a few plants (*Litany for the Whale*), or one in which the plants are unequally distributed (*Freeman Etudes*). Indeed, all sounds are *not* equal in *Music for Piano*, even in the later genera. The pitches between the F at the bottom of the bass clef and the E $\sharp$ four octaves higher can be notated two ways: on the staff, using the obvious clef, and by means of ledger lines above bass-clef or below treble-clef staves. Pitches higher or lower than these have only one possible notation, by means of ledger lines. Hence sounds produced by keys in the outer three octaves of the keyboard are half as likely to occur as those in the four center octaves. Then again, the white-note keys A, G, and D can only be written with naturals, whereas all other keys can be written two ways (G $\sharp$ /A $\flat$, for example). A, G, and D will therefore occur less often relative to the others. Furthermore, because naturals occur twice as often as sharps or flats, the enharmonic pitches E, F, B, and C—which can be written with either a natural or an accidental—will occur more commonly still. All this can be confirmed by an ‘experimental’ analysis in which one simply counts the pitches.

If Cage were concerned with ‘balance’ or ‘equality’, these would be compositional flaws. But he is not; he is concerned simply with observing sounds, regardless of their distribution; and the slight overemphasis on E and B (or F and C) does not call up habits of listening (tonics and dominants) with enough force to impede observation. This consequence is entirely tolerable, and there is no need to modify or reject the process or its outcomes.

What I am calling ‘experimental analysis’ is appropriate for Cage (i.e. for Figure 2) because it enhances observation, rather than interfering with it. It does not impose meaning; it does not link composers with performers with listeners. Conventional, relational analysis does both, and hence is appropriate for conventional, relational works like Figure 1. Though Figures 1 and 2 are, in a sense, the ‘same’ work, they are to be distinguished—but by the analyses embedded in or welcomed by the scores, not by the sounds that occur in their presence.

In fact, the adjective in the phrases ‘experimental music’ and ‘experimental analysis’ might plausibly be replaced with *experiential*. Let me again invoke William James, who argued that from infancy the stream of consciousness that constitutes experience is, in effect, the creation of a taxonomy of existence. We distinguish objects one from another, bounding and boxing the ‘blooming, buzzing confusion’ (James, 1890, vol. 1, p. 488), simply in order to survive. We create a taxonomy of plants in part to be able to determine which are poisonous without having to experiment with our lives. But sounds are not mushrooms, and the usefulness of an experiential *musical* taxonomy does not lie in the prevention of illness or death. It lies in precisely the same purpose that is served by experiential music: it enables us better to observe what is around us.

Experimental analysis, then, is coextensive with composition itself. Cage asks what composing, listening, and performing have to do with one another. *Nothing*, is the implication; and indeed, if conventional analysis is assumed, if the results from one are used to test hypotheses about the other, *nothing* must be affirmed. But if experimental analysis is assumed, they are one and the same, and the analysis itself is folded in and around them. There is no need to partition and separate; all can lead to the same eminently desirable state: freed from value, freed from symbols, freed from theory and relations and meaning, but fully aware of what *is*, in its infinite detail and variety.

Cage was never analyzed; but his music was—and will be. And that is how it happened.

Notes

- [1] I have modified this quotation for rhetorical purposes; Cage actually wrote ‘I was never psycho-analyzed ...’.
- [2] My debt to William James is obvious; see in particular Chapter IX, ‘The Stream of Thought’, in *The Principles of Psychology* (James, 1890, vol. 1).

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