In his Round Table article “Sampling 3-D seismic surveys” (TLE, July 1994), Norman Neidell presents his conjecture on higher-fold sampling of coarse grid data. His motivation is his concern for a better velocity imaging field, and the high acquisition costs of finely sampled data. He states that the discussion hinges on the premise that higher-fold seismic data allow better determination of the imaging velocity field. Whereas there is nothing wrong with this premise, many of the arguments Neidell uses to develop his conjecture are seriously flawed. Therefore, I would like to take a position on the opposite side of the Table.

According to Neidell, two generally accepted “truths” are ill-founded:

1) Closely spaced soundings better define small targets.
2) Dips impose a spatial sampling constraint.

However, he embraces another, generally accepted, truth:

3) Uniform parameter sampling within each bin reduces subsequent artifacts.

He also asserts that:

4) “The ultimate spatial sampling density (after migration, my comment) of the subsurface by the seismic survey is not tied to the bins as initially acquired in any prescribed or fixed manner.”

Furthermore, he believes that at least 24-fold is required for velocity analysis:

5) “A 24-fold CDP redundancy can be taken as a minimal requirement to establish acceptable velocity control.”

All those points find some place in the development of his conjecture:

6) “If N is the target dimension, then bin centers at 2.5 N spacing or less should suffice, if data fold is 24 or greater.”

In summary, Neidell wants to make 24-fold (or higher) data more affordable and he argues that we can achieve this using coarser acquisition sampling without sacrificing resolution (he denies the truth of statement 1). His solution is to use migration to interpolate to a finer sampling interval.

Statement 5 is Neidell’s motivation to develop his conjecture; i.e., because 24-fold is needed, and cost has to be contained, coarse sampling might be a nice solution.

After some remarks about statement 5, I will discuss Neidell’s various arguments to defend his conjecture.

In the article, statement 5 is considered as a given; it is not further discussed. Yet, how much fold is required for adequate velocity control is not as clear-cut as Neidell suggests. In the old days, velocity information was deduced even from single-fold data. Since their introduction in the early 1970s, semblance-type programs have become more or less standard as a velocity analysis tool. These programs perform best with high-fold data. However, the analysis is hardly ever carried out for all available data. Instead, some kind of interpolation in a sparse grid of velocity control points is used to create velocity maps. It may be possible to trade scattered velocity analysis based on high-fold data for continuous velocity analysis on lower-fold data. In other words, 24-fold or higher may not always be necessary for acceptable velocity control.

Statement 4 is 100% correct. The output sampling interval of migration may be selected as fine as you like it. (As long as migration is the last step in the processing sequence, the output sampling interval may also be selected as coarse as you like it, but that is not what Neidell wants to do.) However, it does not make sense to go to a finer sampling interval, if the input data to migration is already aliased.

The case for fine (alias-free) sampling is clearly made by the well-known formula repeated, but refuted, by Neidell:

$$f_{max} = \sqrt{\frac{v}{4\Delta x \sin\theta}}.$$

Neidell asserts: “Monochromatic, steady-state plane wavefields do not describe the seismic problems we address, and so this limitation as presented does not apply as stated.” However, the above equation does not say something about monochromatic wavefields, it states something about all frequencies larger than $f_{max}$; they are all aliased, and cannot be treated properly in seismic processing. It is true that the interpreter may have no problem in discerning a strongly aliased event, if it stands out nicely on its own. However, not all geologies are that kind to us, and normally there is a whole suite of unknown apparent velocities in our data that we should be able to handle properly in processing. Many spatial processes fail if there is spatial aliasing, hence statement 2 is correct, dips (and diffractions!) do impose a spatial sampling constraint.
The question now arises: Where then does the reasoning suggested in Figure 1 (in Neidell’s paper) break down? Indeed, the figure is very suggestive. But it does not describe the process of migration, even though it describes the task of migration. What Figure 1 presents is a construction procedure. It answers the question: If I know that there is a small anomaly somewhere and I have two soundings, can I then reconstruct the position of an anomaly? The answer is yes (in a 2-D world, in 3-D you need three soundings) and the construction works best if the soundings lie far apart.

Seismic migration, on the other hand, deals with an unknown subsurface, which can only be reconstructed properly if there is a dense covering of available soundings. The truth of statement 1 is clearly demonstrated in Chapter 4.3.5 of Seismic Data Processing by Oz Yilmaz (SEG, 1987). For good measure, I have added my own Figure 1 to emphasize the importance of regular and dense sampling of input data (a 3-D zero-offset section in this case) to achieve correct imaging in an output point 0. Each trace in Figure lc is a modified version of the corresponding trace in Figure lb. Spatial aliasing in these data sets may be caused by coarse sampling or by irregularities in the data set. Spatial aliasing in the input data of Figure lb leads to suboptimal migration results in two ways: (1) it leads to spatial aliasing in Figure lc with incomplete cancellation of the energy in the flanks of the migration operator, resulting in migration anisotropies, and (2) it causes sparse or irregular sampling of the zone of stationary phase leading to incorrect amplitude and phase of the image. Note that if there is no spatial aliasing, single-fold is sufficient for proper imaging (the 3-D zero-offset section is single fold).

What about migration of the multifold data to compensate for coarse sampling? The above description of migration can be extended to prestack migration. If data acquisition has done a perfect job, then the prestack data set can be subdivided into alias-free single-fold subsets (as many as the fold of the survey, see my paper “3-D symmetric sampling” in SEG Expanded Abstracts 1994), each of which can be used to produce a correct migration result (see Schleicher et al. in Geophysics, August 1993). An example of such a subset is the 3-D common offset/common azimuth gather that can be gathered from a densely sampled 3-D marine geometry. In each subset, the maximum frequency that can be faithfully imaged is still given by the above equation. Spatial aliasing in these data sets leads to imperfect imaging and to artifacts. Then fold has to compensate for the shortcomings of the individual subsets. While high fold would improve the signal-to-noise ratio, and would reduce artifacts produced by the migration of the individual subsets, the resolution cannot be any better than the resolution determined by the sampling in acquisition. This reasoning shows that migration cannot be used to compensate for coarse sampling.

The above reasoning also leads to a modification of statement 3. Though the distribution of offsets and azimuths within each bin determines the stack operator (hence is important), migration artifacts are not caused by variations within each bin, but by bin-to-bin variations in the various single-fold subsets. Therefore, at least as important as uniformity within each bin is uniformity from bin to bin, i.e., spatial continuity.

Would Neidell’s idea ever work? Perhaps it might work in the situation where a benign geology is very well known, because it is essentially flat, and the play is a hunt for anomalies. In that case, the geophysical problem is in fact one of a difference measurement. The background response is known, and the hunt is for deviations from that background. If there is a deviation, a few widely separated soundings are sufficient to detect the presence of an anomaly. However, if we do not know whether the anomaly is large or small, we have to apply imaging techniques requiring fine sampling to determine its location and shape.

Finally, I suggest that the results of Marianeschi and Tili should be interpreted differently. These authors make a clear distinction between resolving power of a technique and sensitivity (detectability). Neidell correctly states that they argue that there is “no theoretical limit to the size of flaws which could be detected using ultrasonic illuminations of a particular wavelength.” However, their method does not “transcend what appears to be resolution limits.” It is only aimed at detection of defects, not at determining their location and shape. In fact, they state that for a small obstacle “it is no longer possible to receive information from the scattered energy which will indicate the form of the obstacle.” Their’s is also a difference method, now with a zero background response (no defect).

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Reply by Norman Neidell:

Gijs Vermeer has produced a remarkably clear and concise exposition of my thoughts on sampling 3-D seismic surveys. He summarized my thinking in his statements 1-6 and I freely admit that he has expressed them better than I did. For this I thank him.

His discussion and arguments with some of my ideas do not follow his points, identified as 1-6, however, and so for ease of understanding, I address his comments more or less as he ordered them. In fact, he starts with 5 where I conjecture 24-fold redundancy as being adequate for recovery of detailed velocity determination. This is an empirical judgment on my part, indeed, but it is based on years of study and work on stratigraphic imaging and hydrocarbon detection. My im-
pression is that Vermeer and many in our industry do not appreciate the need for dense and highly accurate velocity control if we wish to image stratigraphic changes and detect hydrocarbon presence, particularly for consolidated formations. For background, I refer readers to AAPG Memoir 39, *Seismic Stratigraphy II (Use of Seismic-Derived Velocities for Stratigraphic Exploration on Land: Seismic Porosity and Direct Gas Detection)* and in particular to the discussion “Limits of seismic visibility.”

We do agree about statement 4. Our views concerning aliasing diverge quite substantially, however. The formula cited by Vermeer is “well known,” but is without sound basis. Hence we disagree significantly also with regard to statement 2. Somehow everyone has decided that a space and time coordinate constitute an orthogonal coordinate system and that these coordinates (having differing units and dimension) can be treated independently using sampling theory along each axis with simple geometry and velocity scaling imposing the guidelines. This is not what the equation implies. It is only the physics of the wave equation which allows these coordinates to constitute an orthogonal reference system. We have allowed a computational method of solving this equation to cloud our reasoning.

When we look at actual seismic data in an attempt to
“prove” a point, we introduce so many variables and unknowns that it is difficult to put forward an argument that is totally convincing. Nevertheless, I shall try to illustrate my point using two figures computed by my associate, Maggie Smith. The data is from the “edge” of the German North Sea Salt Basin where the salt features look remarkably similar to some of those seen in the offshore Gulf of Mexico.

In Figure 1, we show the same data window migrated using identical velocity functions via the Mercury International Technology processing package with an f-k and Kirchhoff approach (a and b, respectively). Note, in both, a region of very steep dip just to the right of A and a second region having less dip just to the right of B.

We must further appreciate that aliasing actually has two faces. While we usually consider aliasing via the process output, there is also aliasing at the input stage. Both can be illustrated with the data selected. The migration step is, of course, poststack and we can repeat the computations using now only alternate traces, but again using both methods. These results constitute Figure 2. For the f-k approach, input aliasing on 2a does not allow the very steep dip to be addressed effectively and, hence, it is not migrated. On 2b, output aliasing occurs, indicated by the distinctive pattern so often illustrated in text books.

The Kirchhoff approach performs “better” but the word better must be qualified in this instance. Using fewer input traces, of course, produces a noisier result. One might argue that the noise pattern for the Kirchhoff method in 2b has character which is in some sense equivalent to aliasing. This is not the case. Improving the signal/noise situation for Kirchhoff calculation 2b simply requires data redundancy without the positioning requirement implied by aliasing and its cure. We should add that it was important to show improved performance for both instances of aliasing as this strongly suggests the differences move beyond the algorithmics. We should also note the region to the right of C in Figure 2, where the sampling of the shallow reflectors is not adequate to allow the Kirchhoff method to image well, yet the f-k method performs better here.

I should also discuss a reference which I had missed and was pointed out by Dan Ebrom of the University of Houston. Safar, in his paper “On the lateral resolution achieved by Kirchhoff migration” (GEOPHYSICS, July 1985), used a mathematical model to clearly demonstrate the tradeoff of aperture and signal bandwidth using Kirchhoff methods in attaining resolution. He asserts, “If not restricted by the cost of computing, then by using a focused array any desired lateral resolution can be achieved.” In this work, focused array is a Kirchhoff operation either weighted or unweighted. Safar’s findings are quite compatible with our independent researches.

With regard to Vermeer’s Figure 1, it does describe the task but it constitutes also a zeroth order migration procedure and can be very easily extended to have as much accuracy as we wish. In fact, some of the early migration procedures of the late 1960s and early 1970s worked in precisely this way. We would assume some arrival at every time sample of every vertically plotted stacked seismic trace and distribute or “smear” the amplitude of each time sample over appropriate wavefronts as defined by the estimated velocity field. The superimposed sum of all of these operations would form an image of the subsurface above the noise level for an adequate number of soundings. Extension of the method to first and second order wave-equation solutions simply requires application of some appropriate digital operations distributed along these same wavefronts. Such a method is quite analogous to and mathematically equivalent to the diffraction-stack approaches. Recall that the original Hagdoorn migration used both the diffraction curve and the wavefront in repositioning samples.

Seismic migration indeed must deal with an unknown subsurface, but reconstruction requires more than an adequate covering of soundings (I did not say dense!); we must also have an appropriate knowledge of the velocity field. I argue that sampling density and the knowledge of the velocity field do “tradeoff.”

I am not clear on what Vermeer’s Figure 1 demonstrates. The noise inherent in an imaging procedure is quite separate and distinct from an aliasing phenomenon. The effect of noise can be diminished by increasing the number of soundings, but aliasing contamination cannot be addressed only in such manner. What is quite clear from the figure is that if the shape of the wavefront is not correct owing to a faulty velocity field, then we certainly would not attain the ideal output. But this still does not suggest the presence of aliasing - only more noise. Aliasing and noise are two distinct concepts that should not be confused as we have noted before. The aliasing, as presently conceived, is largely a method-dependent effect.

Again, the argument concerning multifield data, in my view, relies also on the same principles I have already refuted. Multifield data is important for two reasons:

1) It permits computation of accurate velocities which are needed to define wavefronts, diffraction curves, or other imaging curves or surfaces.
2) It suppresses noise and computational artifacts by simply having more soundings or better statistical resolving power.

So long as industry relies on the equation cited by Vermeer, which I maintain is not valid, we will not “unlock” the full information content of the seismic data.

In addressing statement 3 (using Vermeer’s definitions), we agree fully and I will admit that perhaps my position was not fully clear. There are, in fact, two criteria with regard to sampling: one is distribution of parameters within each bin and the second is homogeneity of sampling from bin to bin. While I believe that distribution is important, I agree that homogeneity is essential.

The final arguments of Vermeer’s critique ask whether my methods will work. Preliminary testing on proprietary data sets, both at seismic and ultrasonic frequencies, indicates they work remarkably well. As for the results of Marianeschi and Tili, I must disagree with Vermeer yet again. These authors, by their detection scheme, make it possible using minimal additional information to develop detail below any given resolution limit (see “Resolving reservoir complexes below seismic resolution using color displays and model studies,” by Neidell and Smith in SEG Expanded Abstracts, 1990.)

I thank and compliment Vermeer for his well written and careful analysis of the subject matter. It is a pleasure to have ideas reviewed so professionally and logically. While we do not agree and probably are both wrong about some of the elements considered, the dialog should alert us that no aspect of our science, or any science for that matter, will ever be “closed.”