

Promoting Community Conversations About Research to End Native Youth Suicide in Rural Alaska

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Aim 1: Track the effect of PC CARES on participants' knowledge, attitudes and behavior, and identify key factors influencing these outcomes over time.

Overview: Using a multi level growth model for participant outcomes, investigators will generate reliable estimates of changes in participants' knowledge, attitudes and behaviors before, over the 4-6-month intervention and follow-up: 6 occasions of measurement (pre-survey, 4 post surveys completed after each learning circle and a follow-up). Investigators will model variability in these changes over time, and identify key factors affecting these outcomes within the three domains identified through the investigators' pilot study: a) cross-sector participation on a community level; b) individual dosage, and c) participant social role: informal or formal.

Data collection and analysis for Aim 1. Aim 1 has four main activities: (1) track adaptations (2) facilitator recruitment and preparation, (3) document process indicators of implementation in villages, and (4) measure how the intervention affects participant knowledge, attitudes and behaviors over time, and utilizing insights from the pilot work, test how key factors affect these changes in participant outcomes over time.

(1) Track Adaptations. To document additional changes or unexpected issues related to content/format, the training of facilitators (TOF), and the ways in which PC CARES is delivered across BS villages, investigators will carefully document implementation issues and resolutions. These issues, discussions and modifications will be integrated into regular LSC reports. Once PC CARES is implemented in all 3 cohorts (year 5), the LSC and researchers will review changes in content, format or other areas. Researchers will add to the facilitators guide (and training) either the *rationale* for structuring PC CARES in a certain way *or* if options are equally viable, offer future facilitators a menu of choices they can use when implementing the model in their community. *Investigators expect there to be common areas for adaptation in terms of format across sites to fit the local culture (e.g. start with a prayer versus smudge) and content (e.g. lethal means restriction in NWA focuses on guns whereas BS might include reducing access to prescription pills). Adaptable areas will be highlighted in the facilitator manual to offer options for future Indigenous communities to effectively adapt PC CARES.*

(2) Facilitator recruitment and preparation. Recruitment for PC CARES facilitators is based on the local service ecology, LSC members' and other stakeholders' community knowledge, and what is feasible given the investigators' pilot study results. With preliminary approval from partnering tribal organizations--Kawerak and Norton Sound Health Corporation--investigators anticipate regional tribal organizations will encourage village-based workers to become PC CARES facilitators, similar to what happened in the pilot. Investigators aim to train 3-4 facilitators from each community, and will invite individuals who are in key positions (e.g. youth advisors, behavioral health aids, tribal leaders) *and* who are considered to be 'natural helpers' (according to the LSC). All villages will be invited to participate, and start dates will reflect local preferences and availability. Overall implementation will strive for a "stepped wedge" design¹¹¹. However, random *assignment* is not acceptable to the investigators' tribal partners. Instead, investigators will invite randomly selected village to participate in a roll-out design. Villages can, in this scenario, wait to join a different cohort if the timing better suits their needs. Investigators will travel with their tribal co-researchers to participating villages to inform village stakeholders about the opportunity, gain village-level tribal permissions (for regional tribal organizations permissions, see letters of support) and to personally invite people to be facilitators. If

interested, these persons will attend a week-long Training of Facilitators (TOF), with 9-12 learners per cohort, available with 3 University of Alaska Fairbanks Human Services college credits.

Training of Facilitators (TOF). Investigators will document the outcomes of the TOF to ensure a minimum level of readiness to facilitate. The TOF measures include a 10-item pre-post TOF survey measuring confidence, facilitator knowledge and skills. The survey describes specific tasks that are central to the facilitator role, such as recruiting participants, using the guidebook, answering participant questions about suicide prevention, and handling a dominating speaker during large group discussions. Response options range from 1 (not very sure of myself) to 4 (very sure I can do it), and facilitators scoring below a 3 on any item will receive extra support. To ensure facilitators understand the content, a *test of knowledge gains* will focus on the 8 areas of primary and secondary prevention information in 'What we Know'. The 'quiz' includes 16 questions with 2 items for each. TOF participants will get 2 large placards saying 'True' and 'False'. Participants will be seated in rows, facing the front of the room. Each of the questions is written on a large paper in the front of the room, and one-by-one, each is read and shown. Each participant responds to the statement by holding up the sign that corresponds to their understanding of that statement (True or False). The participants are asked to refrain from showing their signs to each other, so that they do not influence each other's answers. A research assistant, facing the group, documents the # correct and incorrect answers. In the pilot study, this method was fun and acceptable to participants, and allowed trainers to clear up any remaining areas of confusion. When piloting this method at the end of the TOF in November 2015, 100% of the 36 participants answered 11 questions correctly. The remaining 5 questions with mixed results (only about 70% or 25 people answering correctly) have been modified to be clearer to participants. Most of the confusion came from the need to preface statements with "Research shows..." rather than making blanket statements like, "suicidal behavior occurs more in the summer". Lastly, all TOF participants will demonstrate proficiency by facilitating one learning circle. TOF trainers (PI: Wexler, McEachern, Kirk, Moto, Pungowiyi) will assess facilitator fidelity (described in the next section). Only when facilitators have met a minimum proficiency level and have an accurate understanding of research—demonstrated in the training—will they be ready to facilitate PC CARES.

(3) Track process indicators of implementation in villages: Investigators will track facilitators' adherence to the curriculum and structured dialogue procedures, participation: across sectors of the community and the number of LC attended by each person, and qualitatively analyze the LC narratives. This data collection is feasible as investigators have demonstrated in their pilot study, with all LCs done so far in NWA, collecting and sending data to the research team by self-addressed stamped envelope. The procedure for audio recording LCs involves *a//* participants giving their permission, and in 60 of 61 LCs piloted, participants did. Audio files are recorded on a thumb drive and mailed to UMass along with participant surveys and attendance data. The research team will transcribe these LC audio recordings and put them into data analysis software¹¹² for coding by research team.

Assessing curriculum fidelity and accuracy of research interpretation. Two independent coders assess transcripts of recorded learning circles (LCs) for information accuracy and fidelity to the structured dialogue procedures described in the facilitators' guide. Areas of ambiguity are to the next LSC meeting to get guidance. The *information accuracy scale* rates both the concordance of information presented by facilitators with the research evidence itself, and the accuracy of participants' interpretation of the presented information (scoring facilitators and participants separately). Investigators use a three-point scale: 1 indicates facilitators interpreted research evidence accurately, leading to conclusions consistent with its intent; 2 means there are no direct misinterpretations of research, but the conversation focused primarily on issues of wellness or suicide prevention not directly related to (or showing

understanding of) the research; and a 3 indicates that research evidence was interpreted inaccurately, leading to conclusions inconsistent with its intent. A narrative rationale is provided for each score. Investigators use both reviewers' ratings to calculate an average. In the pilot, with 35 LCs currently coded, there is a high level of accuracy, average of 1.21. Investigators consider scores under 1.5 to be adequate accuracy. If scores fall above this threshold, investigators will work with facilitators to improve their understanding of the research. *Fidelity.* Investigators assess fidelity for each of the 6 standard elements of each LC: (1) agreements/safe talk, (2) small wins, (3) the LC activity, (4) what we know, (5) what we think, and (6) what we want to do. Each dimension gets 0-1 for procedural components (i.e., presenting specific data, giving clear instructions to the group) that are present (1), absent (0), or not captured (NA) on the recording when the audio clearly misses some of the LC. Investigators will average these scores across two raters for each element, consider scores for each LC, and for each community, providing clues to decipher issues in a particular element, LC, or village, so it can be addressed. Pilot fidelity scores average .79 across two raters, indicating acceptable adherence to the curriculum.

(4) Measure PC CARES participants' knowledge, attitudes and behavior over time.

Participant recruitment: Based on the pilot study, investigators estimate 15-20 participants in each of 4 LCs and at least 50 distinct participants per village. The pilot study shows this is feasible with 376 participants attending LCs in 8 villages with less follow-up and support. With 3 villages per cohort, investigators estimate 150 people per cohort who attended at least 1 LC, with a total number of participants across 3 cohorts at 450. Investigators believe that those who are eligible to receive CEUs will be most likely to come to 3+ LCs and conservatively estimate a third will attend most LCs. For follow-up, investigators will invite all participants to complete the follow-up survey 3 months after the last LC. The research team will stop trying to contact participants after three attempts, and aim to have at least 25 (50%) of the follow-up surveys completed per village (Total=225). This level of recruitment was more than achieved in the pilot (65%) by working with local facilitators to find individuals and send the surveys back to UMass.

Measures: PC CARES outcomes include a pre-, 4 post-, and follow-up assessment of participant learning. The 53-item **pre-survey** tracks participant characteristics (6 items), and uses a 7-point Likert scale from "Totally Agree" to "Do Not Agree at All" with a "Don't know" category. *All survey items were developed with the investigators' tribal partners and piloted with both youth and community members to ensure clarity and intended meaning.* Items assess suicide prevention knowledge (7 items), attitudes toward prevention (7 items), intent to act (7 items), engagement with others for prevention (6 items), and 7 items ask about attitudes toward using research to strengthen community prevention efforts. The survey also asks 12 items related to prevention and health promoting behaviors done within the past 3 months. There are 3 items for each of the constructs covered in PC CARES, and each uses a 7-point Likert scale to learn 'how often' they did particular actions from "a lot" to "not at all". The surveys ask about their actions to support wellness (e.g. intentionally spending time with youth), offering interpersonal care for someone who is down to give emotional support and encourage helpseeking, safety planning to reduce environmental risks (like removing guns), offering support after a suicide attempt, and postvention, which involves talking safely about suicide as well as taking precautions to reduce the risk of contagion if a suicide should occur. An open-ended item allows participants to write-in how they shared information or took action for wellness and prevention in the last 3 months. The 55-item **post-surveys** and **follow-up surveys** include the same items of the pre-survey minus the participant characteristics (items=47). Surveys ask for the participant's name, and take measures to protect their confidentiality. Each person puts their own survey into an envelope and seals it before turning it into local facilitators to send to UMass. The post survey has 4 satisfaction items, 4 items about

building relationships with others in the informal and formal village support systems. Please see vetted and piloted participant survey items in the appendices.

Reliability of measures. Participant surveys have inter-item correlations of .7 or greater using Cronbach's alpha, showing good reliability on 58 analyzed paired pilot surveys. These items also performed well and moved in the predicted positive direction comparing the mean of the pre-test to the mean of the post-test, with 80% of the items showing statistical significance at .05 using the Wilcoxon signed rank test using STATA 14.

Data Collection. Investigators anticipate that there will be approximately 50 unique participants in PC CARES per village (see Table 2); 150 per 3-village cohort and 450 unique participants in 3 cohorts. In the pilot, 376 people attended at least 1 LC. However, some participants chose not to fill out a survey, and less than 30% did pre-posts and follow-ups. The revised incentive process supported by additional facilitator training will make multiple surveys per participant more likely. Investigators' estimates of at least 15 participants per village with 4+ surveys (pre- and 2+ post and/or follow-up surveys) are conservative. With continuing education credits (CEUs), many health, mental health, and school counselors will come to multiple sessions. The researchers will administer the pre- survey before the first session, and post surveys after each LC. Some people will attend every LC while others will attend only one. Investigators will invite all who participated in any number of LCs to do a follow-up survey 3 months after the last LC.

Key factors affecting participant outcomes. Investigators expect particular factors, namely: (1) intervention participation on a community level (# sectors represented by participation in learning circles within each community), (2) participation on an individual level (dosage), and (3) participants' social role to influence changes in participant outcomes over time. These predictors are defined as follows: (1) *Cross-sector community participation* can be understood as a proxy for community capacity to collaborate for suicide prevention. Investigators found in the pilot research that if key stakeholders are able and willing to come together on a regular basis to learn about and make plans for how to prevent suicide, the community readiness for action is greater than in communities where this cross-sector participation does not occur. The attendance sheets will document sector participation using the 12 network positions described in SA2. For example, if LC1 in Village A has representatives from the school (+1), healthcare (+1), law enforcement (+1), and parents (+1) the cross-sector score for that LC = 4. Cross-sector participation for each community will be the average number of sectors in attendance over the 4 LCs. In the pilot work, these scores range from 2 to 8, and can be seen as a proxy for cohesion within village relationships and collaborative potential. These scores are also likely to vary across villages. (2.) *Individual participation:* Dosage: participants' attendance over time (1 to 4 LCs). (3) *Participant social role* will have two levels of categorization: "formal" meaning they are employed within the existing service ecology or "informal" in which they attend sessions based primarily on their social role as family member or friend. People will select one, primary role prompting their attendance. The specific subcategories under the "formal" or "informal" reflect the 12 *network positions* described in SA 2. Investigators hypothesize that social roles will shape one's level of change based on their previous suicide prevention training or lack of it. Investigators expect those without any training will gain the most knowledge, change of attitudes and prevention behavior.

Analytic Strategy. The broad goals of this aim describe trajectories of change over the 6-9 month course of the PC CARES intervention and identify predictors of differences in change.

Survey analysis. The research team will generate a measure of change or growth based on the 6 participant scores, calculating scores for each of the focus areas (knowledge, attitude, behavior), reverse scoring items if indicated. Using STATA 14, members of the research team will run the Wilcoxon signed rank test to determine the statistical significance of

the change from the mean of the presurvey to the mean of the post-surveys after LC 1, 2, 3 and 4 and the follow-up survey. If the data are not normally distributed, investigators will use a non-parametric test (e.g. the Friedman test) to assess change. Investigators expect to find positive gains in participants' suicide prevention knowledge, attitude toward prevention and intention to act, and prevention behavior over time with a linear or curvilinear dose-response. Investigators take advantage of recent developments in the statistical theory of multilevel models to generate reliable estimates of individual change^{113,114}. To apply this methodological approach to the data, investigators will use the hierarchical linear modeling program (HLM8)¹¹⁵. Researchers can estimate individual changes through longitudinal modeling of three or more waves of data, which are available from pre- (before attending PC CARES), post: after each of 4 learning circles, and 3-month follow-up surveys for a maximum of six. Investigators will test the hypotheses regarding important predictors of differences (variability) in change. These are (1) cross-sector participation within a community, which investigators believe will increase impact; (2) individual dosage, which investigators think will increase intervention outcomes, and the (3) participant's formal or informal social role will predict differences in change over time of their knowledge, attitudes and behaviors due to their previous exposure to prevention information. Those in formal roles typically have some suicide prevention training.

Preliminary analyses. HLM assumes multivariate normality of residuals at both within site: level-1 and between sites: level-2. During data cleaning, the univariate distribution of each variable will be examined using boxplots or stem-and-leaf displays to check for excessive skewness and outliers¹¹⁶. Tests of normality will be conducted for all interval-level variables using the Kolmogorov-Smirnov Z-test. Investigators will employ appropriate power transformations (e.g., natural log or square root) when needed to normalize distributions.

Missing data. While a distinct advantage of HLM is the ability to incorporate data missing at random in the outcome variables, it assumes complete data for the predictor variables. Investigators expect to have two levels of missing data. First, some items on survey questionnaires are likely to be missing although investigators will attempt to minimize this possibility. For this type of item missingness, investigators will substitute the respondent's average of all completed items. Second, investigators expect to have wave missingness, where scores are missing due to participant attrition or to not attending some learning circles. For these cases, investigators will use multiple imputation methods as implemented in STATA and in the HLM8 computer program.

Taxonomy of models. For each outcome (knowledge, attitudes and behaviors), investigators will estimate the following series of models to test the hypotheses concerning change over time. *Baseline models.* The first step in any growth analysis is to select an appropriate functional form to represent individual change over time. Given the number of waves (6 max: pre, post after each of 4 sessions and follow-up) and the short time frame (~9 months), investigators will test whether change is linear or curvilinear over time. *Main effects of predictor variables.* Investigators enter the predictors in sets to test investigators' conceptual models. Omnibus hypothesis tests will be used to assess the importance of each set in explaining variance in the outcomes, net of the other variables in the model. Variables in the three domains will be investigated to assess their impact. Investigators will test the impact of each set of variables within each domain.

Power. In repeated-measures studies, both the number of waves of data per individual (e.g. level-1 units) and the number of persons (i.e., level-2 units) affect the power to detect various effects. In cases where the within person variance is large relative to the between-person variance of the growth rate and persons vary in their growth rates, the total sample size is less important in determining power than either the

frequency of the observations or the duration of the study. Using a formula provided by Raudenbush and Bryk (2001), investigators calculate that, given 6 time points spaced over 9 months, a sample size of a minimum 135 will be large enough to detect moderate effects in change rates. Using Optimal Design, a program used for multilevel modeling, to get an unstandardized effect size of .035, investigators would need a conservative estimate of 135 repeated measures of participants in each condition to obtain .8 power to detect between person differences. The effect size is based on the difference in slopes (change rate) between differences (e.g., people with high versus low dose; level of cross-sector collaboration; formal or informal social role). The total sample is 450 participants (50 participants per villages, 9 villages), and a conservative estimate of 135 participants with high dosage (3+ LCs), half the villages with 3+ sectors attending, and a third of the participants in formal roles.

Aim 2: Document the community-level impact of PC CARES by tracking the number and type of interactions aimed at preventing youth suicide and promoting wellness in participating villages, and describe changes in the supportive social networks of young people before and after the intervention.

Overview. Investigators will record the effectiveness of the intervention at the network and community level by performing pre- and post-intervention network interviews documenting the level and type of institutional/role interactions that take place across those institutions and the social positions best able to meet the needs of young people at risk for suicide. Ideally, these communities could be sorted into case and control groups.

However, working with indigenous communities precludes random assignments where local participation could potentially benefit those involved. Importantly, from the investigators' previous work, investigators know that the differences between communities—even those with similar population demographics—are too great to support generic comparisons. These differences stem from disparate histories, family relationships and geographies (e.g. coastal versus river, nearness to commercial mineral resources). One example can easily be seen through the varied epidemics occurring in the early 1900's, in which some villages lost 80% of their population (and nearly 100% of the traditional knowledge holders: Elders); whereas, other villages remained relatively unaffected. Such varied experiences make comparisons across villages scientifically unsound. Instead, prepost intervention interviews and variation in initial rollout timing will create a natural stepped wedge design. To assess intervention outcomes investigators will collect bipartite (individual and institution/social role) interaction data in each participating community, under the direction of Dr. Dombrowski. This method builds on research previously undertaken by the research team in NWA in 2015-17 and Inuit communities in Arctic Canada in 2010-11, and will focus on levels of cross-sector collaboration (including informal supports) and youth support networks within each community. Rigorous network statistical analyses will focus on variation across participating communities, and before/after the community-level intervention. Structural clustering of 12 family and community level network roles and key organizations will be derived via the 1) transformation of bipartite graphs of individuals and organizations/roles in single-mode, sector-level social networks with interaction weighted links, 2) use of Generalized Ward's criterion^{117,118} to discover meaningful clusters of structured interaction, and 3) block modeling of these clusters (using standard density criterion) to create models of organizational structure^{45,46,119,120}. Analysis of the pre- and post-PC CARES structures will allow us to capture changes in suicide prevention and supportive practices within and between these network roles/organizations before and after each cohort wave of 3 villages, and across all the intervention villages.

Background for Aim 2. Measures of structural change are essential to understanding the impact and scalability of the PC CARES intervention. Prior research shows that social processes tend towards behavioral conformity among friends^{121,122}. A large and growing

literature on network diffusion describes these processes in detail¹²³⁻¹²⁵, highlighting the importance of network factors in effecting individual behavior change¹²⁶⁻¹²⁸. Such processes are critical to understanding the efficacy and scalability of the PC CARES intervention. The community education model aims to increase collaborative prevention practice among community members through new modes of interaction and ordinary process of behavioral diffusion. While not all instances of individual change provide evidence for the intervention's success^{129,130}, a person may change behavior through social influence of others who participated in PC CARES even when they did not. Providing a rigorous understanding of the social structural basis for this change is essential. Ethnographic network studies have pointed to the importance of local socio-cultural distinctions that influence a community's sense of its social structure^{44,131}. While difficult to document using conventional evaluation methods, social factors can significantly influence the impact of even the most thoroughly tested interventions. *Where PC CARES aims to strengthen community and institutional supports for vulnerable people by increasing collaboration among and within a variety of village institutions (e.g. tribal, health, school) and their links with informal village social networks (e.g. parents, Elders, youth), an evaluation of the success of an intervention must focus on the relations among these roles and its impact on the supportive social networks of vulnerable youth.* Informal networks in Arctic communities are often self-organizing and not the result of any particular government or social service agency program^{135,132,133} or externally imposed political structure (such as the ANCSA land settlement^{134,135}). Rather, the social networks in which peer effects take place are those that people form and reform around issues of food, economy, and health to assist one another in coping with the dramatic changes that have come about (repeatedly) in Arctic/sub-Arctic and Indigenous communities.^{42,136-138} While such networks are ordinarily unnamed and fluid, there is a general sense throughout Indigenous research that "connections matter". Research shows that culturally-appropriate Indigenous suicide prevention requires engagement of both formal and informal systems of support in order to intervene early in the risk trajectory of young people, and effectively increase youth access to social and emotional support, helping resources and wellness opportunities.^{139,140} Prior research by the PI (Wexler) emphasizes the importance of social relationships in suicide events,^{10,11,27,55,67,83,141} emphasizing the importance of informal helping and supportive relationships, and the central role these relations play in reducing suicide risk²⁷. An understanding of how these formal systems work together to identify, support and engage with vulnerable youth, and how informal systems interact with (or sometimes avoid) formal systems will offer insight into systems impact of PC CARES¹⁴². To document this, investigators track the changes in community-level systems to the number and variety of supportive interactions experienced by young people within the participating communities. A nuanced description of pre-post social networks will offer insights not only into the degree of community mobilization related to youth suicide prevention, but also how this mobilization is working to affect collaborative relationships within and between formal and informal systems to strengthen youth supportive social networks.

Data collection and analysis for Aim 2. Investigators use social network analysis methods that have been successfully used in rural Alaska by the investigators' team^{143,144}. *Employing a wait-list cohort strategy (3 villages per year over 3 years, 9 communities), investigators will measure prevailing suicide prevention support systems that influence (and thus potentially prevent) suicidal behavior.* Recent network studies have argued that while social networks are necessarily fluid and changing, underlying structural factors exist that lend them inherent characteristics of resiliency.^{131,145,146} It is equally true that these informal networks intersect at various points with more formal health structures.^{145,147-149} Investigators will document those important connections. The pilot research produced a roster

of 12 social ecological roles within the village (hereafter: “network positions”) that are relevant to suicide prevention. These network positions include 7 organizational affiliations: individuals interacting with young people on the basis of their position in schools, health care facilities, mental health services, social services, religious institutions, tribal governments, city governments (including law enforcement); and 5 family roles: Elder; parent or *anayakaak*; a sibling or cousin who is close: *aakauraga*; other adult family member who is also a mentor: *ilatka*, or friend. The final “network position” will be “Other”, with an open-ended response. Investigators will recruit all available people within the above-named village institutions, youth (ages 15-24), and others interested in participating. This recruitment is feasible: investigators recruited 75% of eligible village participants within 5 days in two villages in the pilot study. Random sampling is not feasible or required for members of formal organizations. Instead, investigators will perform a census of all individuals in formal network positions, which are limited in number (estimated $n \approx 35$ in each community). The possibility of data missing not-at-random will be assessed using reported demographics of non-responders from among all eligible participants. Random sampling of family roles in the community is hindered by the lack of a network sampling frame that could describe the distribution of ties across the communities. The investigators’ solution is to employ a rigorous respondent referral method to recruit a sample of informal/family respondents for reports on informal networks, supplemented by active solicitation of at least one member from each household in the community. Previous similar efforts in Arctic communities by the study team have shown high levels of representativeness while conforming to non-intrusive, community acceptable standards of research participation. There, recruitment was able to produce data from roughly 85% of all households and a close match to Census averages across a range of demographic categories^{44,143}.

Social Network Survey. Investigators developed the network survey items with AN community members, youth and local PC CARES facilitators in the pilot study. Items developed from these discussions were shared with a variety of community members (including youth) to get feedback to ensure that each question was clear, reflects local interactional patterns, and is culturally relevant. The resulting pre- and post- social network surveys ask respondents’ to look at a list of 43 likely interactions and indicate whether or not they have done them in the last 3 months. The specific kinds of interactions are associated with suicide risk reduction (e.g. lethal means restriction), interpersonal support (e.g. communicating openly), and wellbeing (e.g. showing positive regard). Respondents read through 10-11 short items on each screen, indicating either ‘YES,’ they have done it in the last 3 months or ‘NO’ they have not. Those ‘YES’ items will then have a series of follow-up questions that ask with whom they have done this interaction, and how often it has occurred. The ‘with whom’ screen lists the interaction and asks respondents to select from the 12 network positions all the people they interacted with in this way. For each ‘checked’ network position, investigators will also ask: how often, with 3 answer categories of “once” “a couple times” “many times”. See the Network Survey in the supplemental materials. The resulting network will be bipartite (or multi-mode) in the sense that people will provide connections among the list of network positions; and it will be multi-layered, due to the tracking of distinct types of intervention activity⁵² that relate to wellness, support, and suicide prevention, including referring to mental health services or calling the Alaska Careline. Answers will track their engagement in help seeking (and - giving), support, and wellness activities with different kinds of people, and will include over-sampling of youth in the highest-risk age group (ages 15-24). The intervention is aimed at activating these kinds of intergenerational, inter-sectoral interactions. If successful, investigators expect the young people in the pre-post network survey who are close to PC CARES participants to have more changes in supportive interactions, help-seeking and wellness options when compared to other youth who are not close to the participants. Investigators also expect to see differences between

villages based on moderating variables such as cross-sector and intergenerational interactions, and level of participation in each community (# participants/population).

Recruitment, sample and process: Because the main analytical approach will feature structural clustering and block modeling, enrolling different numbers of respondents across the various network positions will not significantly affect the pre-/post-intervention comparison, provided the differences are not very large, and similar numbers of individuals from each network position are interviewed at the pre- and post- stages. Investigators will recruit all those who attended PC CARES (approximately 50 participants per village), and will target all of those in the 7 formal network positions (n=14-35 formal supporters), whether or not they participated in the intervention. Investigators also aim to have, depending on population, 30-60 youth (ages 15-24), and 20-40 additional community members who fill informal support roles (e.g. Elders, *ilatka*, mentors) participate in each village, based on population (with eventual categorical weighting of results drawn from the 2010 US Census of the community). Each person filling out the network survey will report on a range of ties across all 12 network positions. The sample of 65 to 150 interviews per village (average of 108) and 3 villages per cohort (n=324), will result in a total number of participants n=~975 pre- and post- intervention. These numbers are feasible using the sampling method. In the pilot study, 304 surveys were done in just two small villages in 8 days. *To recruit community members, investigators will use a peer-referral system that tracks recruitment patterns via referral coupons.* Beginning with each of the network positions within each village (e.g. tribe, school, parent, Elder) investigators ask each of these individuals to recruit those with whom they are closest. Tracked referral chains will be analyzed for homophily and degree biases via methods developed for Respondent Driven Sampling (RDS), though the samples collected here will be too small and the referral chains too short for use of the full RDS framework¹⁵⁰⁻¹⁵⁴. Instead, local Census data will provide a frame for sample weighting, with homophily and degree estimates incorporated into existing weights in a hierarchical linear modeling (HLM) framework. The resulting individual weights serve as corrections on the numbers of reported ties of each person.

Social network analysis. The analysis of the pre- and post-intervention network data will involve reducing the bi-partite network into a single-mode graph of network positions. Here network edges (“links”) are composed of the weighted total of persons identifying meaningful contact with a network position in one of the network layers. Thus if a community member reports seeking advice with an Elder about someone in the community vulnerable to suicide, and later talking with a teacher about the same kind of issue, an edge is drawn between the network positions of Elder and Teacher (school), with a weight equivalent to corrected weight of the reporting individual. Where a participant is a member of an organization, all of his/her reported interactions in one or another of the network layers are counted as connections between his/her own organization, and other network positions. The resulting single mode graph of network positions will then be clustered using a structural positioning algorithm (structural equivalence clustered via Generalized Ward Distance Criterion^{118,155}). This process sorts the positions into groups of network positions whose patterns of connections with the members of other groups are most similar. *This analysis will be able to isolate PC CARES participants and help us understand how they—and the different sectors within the community— collaborate before and after the intervention.* These groups can then be block modeled to reveal the aggregate structure of the network (using density-based models¹⁵⁶) based on aggregate tie strength between the clustered network positions. The advantages to this strategy are: (1) this technique is amenable to the use of sampled data, even while targeting overall community structure; (2) goodness of fit and related tests of significance have been established for the density-based modeling that will be used¹⁵⁶; (3) comparison between pre- and post-intervention structures can be used in a null-model framework to evaluate the statistical significance of the changes resulting from the intervention; and (4) the results provide

succinct, visually intuitive representations of results that facilitate incorporation in local planning. Investigators expect that communities will show increased multi-sector collaboration focused on ‘upstream’ suicide prevention, safety and health promotion after PC CARES. By tracking changes across three distinct network layers (‘informal’ and ‘formal’ layers and type within), the results of this analysis can be used with the results of youth supportive interactions to better understand which aspects of community collaborations associated with the PC CARES most effectively increase youth access to and use of support, helping resources, and healthy/cultural activities in their villages, schools, families and peer groups. *The cohort-based, staged intervention format will provide data that ranges from a single village, to 3, three-village cohorts, and finally to the entire regional intervention group, allowing us to explore sources of variation among the structural outcomes. This analysis is possible because the network positions identified by the investigators’ prior research exist across the entire intervention area, allowing the merging of PC CARES outcomes at each of these levels.*

Overview

In this 10 page document, investigators describe what they plan to do in response to reviewers’ suggestions, including a description of the design to detect within-person change, the power analyses, and why they do not plan to do a multiple baseline design nor a comparison group. Both approaches are unacceptable to the investigators’ tribal partners. Investigators offer more information about the tribal research context and community-based participatory research (CBPR) process to support the rationale. Importantly, investigators emphasize the innovative social network analysis they will use to understand the intervention’s social efficacy. For this kind of community intervention in low-resourced settings, this last point—the diffusion of prevention knowledge, skills and behaviors—is most important. The innovative methods developed by Dr. Dombrowski and his team in the pilot allow us to do so, thus moving community evaluation methods forward in significant ways. Lastly, investigators share preliminary and promising results from the investigators’ pilot research.

Section A. Clarification that study design examines within-person change, and does not have a comparison group.

The investigators appreciate the opportunity to respond to the methodological concerns that have been raised regarding the study design as represented by Table 3 in the original proposal. They have revised Table 3^b to include only the repeated measures part of the design (below). The primary issue is investigators’ misuse of the term “stepped wedge” approach, as this design requires data collection strategies that are unacceptable to the investigators’ research partners. The investigators appreciate and acknowledge that such designs would be underpowered in the present proposal. However, the study is not conducting an intervention with a comparison group. As explained in details below (Section B) and in the original proposal, an intervention design involving extended wait times was not acceptable to the investigators’ community partners due to maltreatment of these communities in historical research efforts.

| Table 3 ^b : Overview of Staggered Data Collection for Measuring Within-Person Change | | | | | |
|---|--------|--|--|--|------------------------|
| Village Cohorts | Year 1 | Year 2 | Year 3 | Year 4 | Year 5 |
| Cohort 3 Randomly Invited 3 rd wave | | | | Pre- [^] Post ^{^^^^} | Follow-up [^] |
| | | | | Pre- [^] Post ^{^^^^} | Follow-up [^] |
| | | | | Pre- [^] Post ^{^^^^} | Follow-up [^] |
| Cohort 2 Randomly Invited 2 nd wave | | | Pre- [^] Post ^{^^^^} | Follow-up [^] | |
| | | | Pre- [^] Post ^{^^^^} | Follow-up [^] | |
| | | | Pre- [^] Post ^{^^^^} | Follow-up [^] | |
| Cohort 1 Randomly Invited 1 st wave | | Pre- [^] Post ^{^^^^} | Follow-up [^] | | |
| | | Pre- [^] Post ^{^^^^} | Follow-up [^] | | |
| | | Pre- [^] Post ^{^^^^} | Follow-up [^] | | |

[^] PC CARES Participant Survey documents participant knowledge, attitudes, & suicide prevention behaviors: 6 waves: pre-[^]; after 4 monthly learning circles (LCs): post-post-post-post-^{^^^^}; and follow-up[^] within 3 months of the last LC); 150 per cohort and a total 450 unique participants.

Given that this study will not have a comparison group, the study is designed to test for significant *within-person* changes in the study outcomes (PC CARES). Investigators will guard against potential ordering effects by randomly selecting which of the 15 eligible villages will be invited in Cohort 1, 2, or 3. The design choice to include staggered starts for the 3 intervention cohorts allows us to guard against the possibility that a single external event could explain the results (Rhoda, Murray, Andridge, Pennell, & Hade, 2011).

In addition, the staggered treatment starts will provide three distinct replications of the intervention. Based on the methodological suggestions provided, investigators will examine within-person effects in each of the 3 cohorts of 150 participants, rather than pooling across the 3 cohorts as proposed in the initial analysis plan.

Section B. History and rationale for not including a comparison group.

Doing research in rural Alaska Native communities, particularly about the significant health disparity of youth suicide, requires a high level of sensitivity, community involvement and shared decision-making. These research values are particularly important given the history of maltreatment and exploitation related to research in many tribal communities (Wallerstein & Duran, 2010; Caldwell, et. al., 2005; Norton, & Manson, 1996), and specifically in Alaska Native communities (Rivkin, et. al., 2013; Foulks, 1989). Consequently, there is a pervasive mistrust and suspicion of research in Indigenous communities across the U.S. Having done tribal community-based participatory research in rural Alaska Native communities for over 20 years, the PI (Wexler) understands how to balance scientific and community priorities in respectful ways.

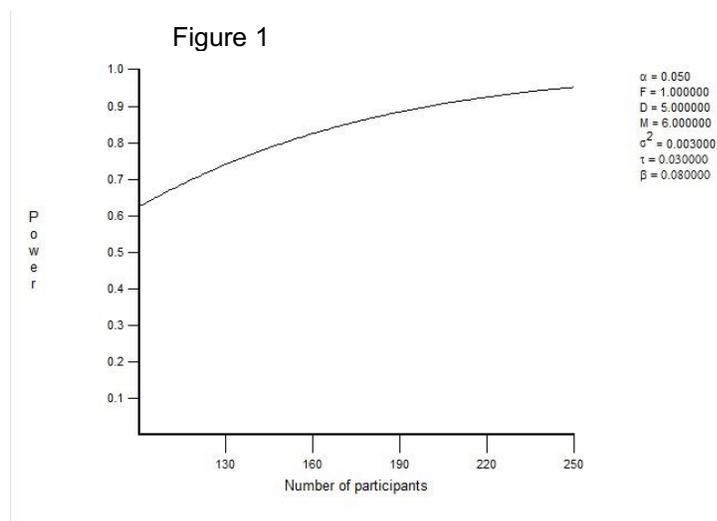
In this context, the Bering Strait Community Planning Committee met several times to develop this proposal (R01 MH112458). Researchers and tribal partners discussed the scientific priorities and considered them in light of community values, preferences and wants. After multiple discussions, it is clear that randomization is not acceptable to tribal partners. They believe that the intervention needs to be made available to all villages in the Bering Strait Region, especially since it is aimed at preventing youth suicide. Tribal partners emphasize that self-determination and reciprocal benefits in research partnerships are paramount. Since the intervention could benefit communities, all villages need to be offered the chance to participate. Thus, in the design, investigators plan to *invite* 5 communities (of the

total 15) for a total of 3 villages to participate in PC CARES per cohort, at random, for a total of 9 villages. The investigators' plans reflect the possibility that some communities (6 of 15) will not want to participate. Given the community preferences, investigators arrived at the stratified sampling strategy presented in the proposal.

Tribal partners want data collection to occur in communities only just before they participate in the intervention in order to reduce their measurement burden. To elaborate on their perspective, the investigators' partners did not feel that it was fair and reasonable to ask community members to complete surveys, and then wait up to 3 years to receive the intervention. Suicide is a very real tragedy that affects entire communities. Filling out surveys for a suicide prevention project that may not occur for years is a way to engender mistrust of the people involved. The investigators' tribal partners think that a long wait between data collection and intervention start could inadvertently create mistrust of community members (e.g. "You people came and asked us to tell you things on surveys, and didn't do anything to help for years."). Additionally, the wait was seen to put the researcher needs and desires above those of the participating communities. This perceived imbalance—weighing the needs of research over that of community-- is unacceptable to tribal partners. Working within these meaning systems and power dynamics, investigators propose to conduct a staggered start intervention that guards against ordering effects.

Section C. Updated power analysis by cohort.

Though the original study design was powered to examine within-person change in study outcomes, the initial analysis plan proposed to examine within-person change pooling participants across the 3 study cohorts (150 participants each, for a total sample size of 450). In the original power analysis using Optimal Design software (Raudenbush et Liu, 2001), investigators chose to model a moderate effect size, which was the unstandardized coefficient relating time to the outcome. In other words, it is the expected rate of linear change per unit time in an unstandardized metric. So, an effect size of .08 means that the study is powered to find a positive growth rate of .08 units per session. This curve (Figure 1) is generated by the multilevel power program Optimal Design (Raudenbush et Liu, 2001) and shows the power curve to detect linear change. It is based on the following parameters: 6 repeated measures, alpha set to .05, a ratio of between-person to within-person variance of 3:1, and an unstandardized effect size of .08.



Given these parameters, the study would need 150 participants to detect an individual difference (such as hypothesized dosage or role effects) of .08 on the slope of linear change in the study outcomes. Thus, with cohorts of 150 participants each, the study is sufficiently powered to detect the expected effects on within-person change for each cohort. In addition to analyzing change within each cohort separately, investigators will use descriptive statistics to compare the findings across the 3 cohorts to

explore whether consistencies are present that might be indicative of a treatment effect.

Investigators have re-done the power analysis using a Monte Carlo simulation method with the MPlus software package (Muthén & Muthén, 2002). This calculation is based on pilot data from the R34 MH096884, which was unavailable at the time of the original submission. The effect sizes for within-person change have been estimated from the pilot data rather than assumed (as was the case with Optimal Design). The investigators acknowledge that the original analysis plan neglected potential cohort effects of time (e.g., historical external events), and agree with the reviewer's suggestion that the study focus on each cohort separately in all analyses. In response to these methodological concerns about pooling the groups, investigators have conducted power analyses at the cohort level to test for within-person change. As results indicate, the analyses showed that the study will be sufficiently powered to detect effects at the cohort level (per cohort of 150 participants).

As described, since the time of the original proposal investigators have completed data collection and cleaning, thus enabling us to use pilot data in the power analysis calculations. This pilot study used the same outcome measures and the same intervention as proposed in the present work. Using pilot data in power calculations provides more accurate estimates for statistical power than formula-based approaches; this issue is particularly important in estimating power for multilevel models, where multiple parameter estimates for both fixed and random effects must be known in order to make obtain power calculations (Bolger & Laurenceau, 2013).

As a reference point, investigators will first report the analyses from the pilot data collection for each of the primary outcome scales of the PC CARES measure related to the primary and secondary suicide prevention intervention. These 4 outcome scales are: suicidal prevention *knowledge, skills* related to handling suicidal behavior, *community of practice* to respond to suicide risk and suicidal behavior, and *intention to act* regarding suicide prevention. In the pilot data collection, only 2 assessments were collected for most participants: pre-intervention and follow-up. In order to provide a sense of the magnitude of the intervention effects, paired samples *t* tests were conducted to estimate within-person change as a difference score in these outcomes pre- to post-intervention. Results showed statistically significant increases of moderate effect size in three of the four primary outcome measures. This difference can be converted to an effect size (Morris & DeShon, 2002), which account for the correlation between the two scores. For the 82 participants with complete pilot data pre- and post- intervention: significant increases were found in the suicide prevention knowledge scale (increase from 3.27 (.77) to 3.84 (.61), ES: .69) (Morris & DeShon, 2002), the skills to handle risk and suicidal behavior scale (increase from 3.64(.80) to 4.05(.68), ES= .49), and the community of practice in response to suicidality scale (increase from 3.52(.81) to 3.90(.78), ES= .43). The increase in the intention to act subscale was not statistically significant and much smaller in effect (increase from 3.84(.59) to 3.91(.60), ES = .12). Investigators believe that the within-person changes to the 'intention to act' is not significantly higher in PC CARES participants because those who voluntarily participate in the intervention are already highly motivated to take prevention action. Indeed, by attending the PC CARES intervention, they are taking action for prevention in their home communities. The ceiling effect is expected.

The investigators' use of social network analyses (Specific Aim 2), however, will track the diffusion effects of the intervention, which is paramount to the success of the community intervention. Briefly, the innovative social network methods enable us to document the behavior of people did *not* attend the intervention, but who are socially "close to" someone who did. The pilot study offers evidence that the PC CARES may induce significant, positive changes in people close to PC CARES participants in terms of increasing their health promotion and prevention behaviors. Investigators believe the potential for

change is greater in these ‘close associates’ because they may not necessarily be motivated toward prevention (since they did not attend the intervention), but can be socially influenced by those PC CARES participants who are learning about how to engage in primary and secondary prevention (See Section D).

For Specific Aim 1 in the proposed study, investigators will use a more complex analytic design using multilevel modeling with random effects to test for change in these outcomes over six repeated measurements rescaled to reflect change from baseline through follow-up to match the scaling of the pilot data which was collected in fewer intervals than the proposed design. The pilot data provided limited (highly unbalanced) but sufficient information to estimate expected linear change in the outcomes over the course of the PC CARES intervention in a multilevel modeling framework (total $N = 94$. 82 participants has 2 repeated measures, 12 participants had 3 or 4 repeated measures). With more data collection points required in the proposed intervention, investigators expect to be capturing change more reliably than the estimates obtained from the pilot data. Thus the following analyses are conservative in their estimation of within-person linear change. The equation for the analyses are as follows:

Level-1 Model

$$Outcome_{ij} = \beta_{0j} + \beta_{1j} * (Time0to1_{ij}) + r_{ij}$$

Level-2 Model

$$\beta_{0j} = \gamma_{00} + u_{0j}$$

$$\beta_{1j} = \gamma_{10} + u_{1j}$$

Where *Outcome* is one of the primary suicidality knowledge outcomes: suicidal prevention *knowledge*, *skills* related to handling suicidal behavior, *community of practice* to respond to suicide risk and suicidal behavior, and *intention to act* regarding suicide prevention. *Time0to1* represents the repeated measures within individuals over the course of the intervention, rescaled so that a one unit change in *Time0to1* represents participant linear change in the outcome from baseline to follow-up.

Investigators used guidelines for conducting power analyses via Monte Carlo simulations for multilevel models (Bolger & Laurenceau, 2013, Gelman & Hill, 2006) with Mplus statistical software (Muthén & Muthén, 1998–2012, 2002). In this technique, parameter estimates obtained from a linear growth curve model of the pilot data are used as population estimates, and multiple simulation studies for the specified sample size are conducted from this population. Across simulations, investigators calculate the percentage of times any given parameter estimate was statistically significant. In general, 80% power to detect effects is considered to demonstrate sufficient power.

Recently-completed analyses from the pilot study are the basis for the current calculations. Investigators conducted post hoc power analyses using the same sample size and missingness patterns as the pilot sample, in order to obtain an estimate of the fixed and random effects for change in each outcome found in the pilot study. Next, investigators conducted power analyses for the expected size of each cohort in the new study and the proposed number of repeated measurements: 150 individuals with 6 repeated measures, assuming 30% missing data. Following recommendations, 1,000 simulations were conducted to obtain estimated power to detect effects (Bolger & Laurenceau, 2013). Results are summarized in Table 1, next page. Within-person linear change was found to be sufficiently powered, even in the pilot sample, for the first three outcomes. Consistent with pre-post analyses of the pilot data, the smaller sized within-person change in the intention to act scale was underpowered in the original sample size, as was its random effect. Thus, investigators find evidence that the primary

analyses in Aim 1 are more than sufficiently powered to detect significant within-person change on the suicide prevention knowledge, skills, and community of practice scales. The moderator analyses will focus on which person-level characteristics may modify the degree of linear change in the knowledge and skills variables only, as these showed sufficiently powered variability estimates around the average change necessary to test for moderation effects.

Table 1: Power Analysis Results Based on Monte Carlo Simulation

| | <i>Post hoc power from pilot data (N=94)</i> | | <i>Power for proposed sample size (N=150)</i> | |
|---|--|--|---|--|
| Outcome | Average Linear Change (Fixed Effect) | Variability around linear change (Random Effect) | Average Linear Change (Fixed Effect) | Variability around linear change (Random Effect) |
| <u>Knowledge</u> of suicide prevention | >99% | 44.4% | >99% | 99.5% |
| <u>Skills</u> to handle risk & suicidal behavior | 94.7% | 48.3% | >99% | 99.6% |
| <u>Community of Practice</u> supporting suicide prevention | 91.3% | 0.04% | >99% | 0.13% |
| <u>Intention</u> to act | 21.6% | 2.0% | 59.4% | 2.5% |

Note: Percentages represent percent of 1,000 simulations in which the linear change fixed effect and random effect obtained statistical significance at the $\alpha < .05$ level. Effects above 80% are considered sufficiently powered for that sample size.

Thus, with cohorts of 150 participants each, the study is sufficiently powered to detect the expected effects on within-person change for each cohort. Investigators will test whether the three villages in each cohort differ by including indicators for two of the three villages representing the main effect of village and time-by-village interactions. This will test whether either the intercept or the linear slope in the growth curve model differs in one village versus another. With the proposed design investigators will also examine whether change is curvilinear by adding quadratic terms to the polynomial function for time, but pilot data did not have enough timepoints for us to include them in the primary power analysis simulations. In addition to analyzing change within each cohort separately, investigators will qualitatively compare the findings across the 3 cohorts to explore whether consistencies are present that might be indicative of a treatment effect.

Section D. Innovative methods to examine within-community change due to intervention.

Importantly, investigators are using innovative social network methods to track the social effects of the intervention, which has great potential for use in other community-based research. The pioneering techniques developed in the pilot study allow us to leverage social network data and analysis techniques to identify and measure the social forces that result in correlations among respondents in the same village. This new kind of analysis will enable us to understand and quantify the “social efficacy” of the PC CARES intervention. As the most recent NIMH reviewer’s comment reflects, social relationships that exist prior to the intervention are likely to influence the post-intervention outcomes of both participants (through reinforcement) and non-participants (through diffusion/contamination) in community settings. In small, close-knit Alaska Native villages, these social influences are virtually certain, and in the case of PC CARES, they are actually essential to the success of the investigators’ community education model.

Close social ties between participants and non-participants create the possibility of information transfer and/or behavioral influences between them, which investigators successfully documented.

In a paper that will be submitted shortly, investigators describe this process formally and apply it to the pilot study of the PC CARES intervention. In that analysis they found that PC CARES had its largest impact via the social effects of prevention and the diffusion of behavior change. Across all 39 items on the behavioral survey, 26 (66.7%) items showed significant positive change in the close associates of participants; a much higher level of change than in the participants themselves. The reason for this is typical of many community interventions. Where community-based, participatory research in tribal communities calls for open enrollment and participation, many of those who attend the intervention are members of that small community minority that are already engaged in the sorts of prevention behaviors and activities suggested by the intervention. In their case, looking for behavior change as a measure of intervention success among those who attend the training is likely to be limited. These self-selected participants tend to be highly motivated and there are therefore ceiling effects. Where investigators expect to see the biggest change is actually in the behavior of those who did not attend the intervention, but who are socially “close to” someone who did. In this case, measuring the *social efficacy* is more indicative of the overall impact of the community intervention than is the *individual efficacy*. In the pilot work, investigators were able to show that the PC CARES induced positive change most effectively in people close to PC CARES participants in terms of increasing their health promotion behavior and inducing positive behavior changes relating to prevention (#’s 2 and 4 below).

The technique investigators have developed, which is also being used in two other NIH-supported intervention evaluations—Les Whitbeck’s Bi-Zin-Da-Di-Da project and Stacy Rasmus/Jim Allen’s Qungasvik Toolbox project—measures both behavior change and the retention of protective behaviors across four social axes: 1) individual effects given all social inputs, 2) social effects in the non-participating friends of intervention participants, 3) social reinforcement effects that come from the co-participation of close friends, and 4) general diffusion effects of the intervention on non-participants. A paper describing the formal means for this evaluation, developed by project co-Investigators Dombrowski and Khan, is available and will be submitted, along with the PC CARES results, to PLoS Medicine in the next week or so. A draft of the paper is available now upon request.

Section E. Preliminary Findings from Pilot Research (R34 MH096884)

The investigators’ pilot research (R34 MH096884) preliminary findings are promising! Comparing data from the pre – follow up survey items from PC CARES participants, the investigators found significant positive changes in 12 of the 19 items. More specifically, they compared approximately 80 participants’ pre-surveys with their responses to items at the follow up, which was done between 3-6 months after the last learning circle in their community (see Table 2, next page). When combining items related to 4 constructs, perceived knowledge, skills, attitudes toward prevention, and engagement with others for prevention (community of practice), investigators found that each construct moved in the predicted positive direction, and even with only 83 paired surveys, they found statistically significant results for 3 of the 4 constructs (Chart 1, page 7). Additionally, they found a basic dose-response when they compared PC CARES facilitators, people attending more and less learning circles and those who did not participate in the intervention. There were significant differences among these different groups in their perceived knowledge, and attitudes toward prevention (see Chart 2, page 8). Lastly, PC CARES participants are statistically significantly more likely to do specific preventative actions when compared with people who did not participate in PC CARES (Table 3, page 9). Although this cross-sectional data does not demonstrate cause and effect, it is encouraging. These preliminary findings, in combination

with the demonstration of the intervention’s feasibility in the pilot study, supports further research on the efficacy of PC CARES as proposed in R01MH112458.

Table 2: Comparing PC CARES Participants’ Pre and Follow-up Survey Items

| Question | Mean at Pre | Follow-up Mean, 3 months after | P value (<.05 is significant) | Number of observations |
|--|--------------------|---------------------------------------|---|-------------------------------|
| I know how I can make positive changes for community wellness | 3.79 | 4.18 | 0.0042* | 80 |
| I have clear ideas for safety planning if someone is feeling suicidal | 3.38 | 3.86 | 0.0008* | 80 |
| I feel confident that I can do things to prevent suicide | 3.8 | 4.09 | 0.0341* | 79 |
| I know what time of year young people are most likely to attempt suicide | 2.78 | 3.61 | 0* | 79 |
| I understand what kinds of community level factors can protect against suicide | 3.42 | 3.96 | 0.0001* | 74 |
| Many people in this community work together for suicide prevention | 3.06 | 3.64 | 0.0001* | 77 |
| I know how to talk safely about suicide in ways that discourage others from attempting | 3.29 | 3.84 | 0.0001* | 80 |
| I know what young people around here think will prevent suicide | 3.04 | 3.62 | 0.0001* | 79 |
| I know small ways to support someone after she or he attempts suicide, whether or not I am close to them | 3.65 | 4.08 | 0.0015* | 80 |
| I know how to support and listen to someone who might be feeling suicidal | 3.73 | 4.21 | 0* | 78 |
| I have regular opportunities to make plans to increase wellness and prevent suicide | 3.45 | 3.82 | 0.004* | 78 |
| I have many people to work with in my community to prevent suicide. | 3.53 | 3.9 | 0.0044* | 78 |

Chart 1: Positive Change from Pre-Survey to Follow Up-Survey (n=83)

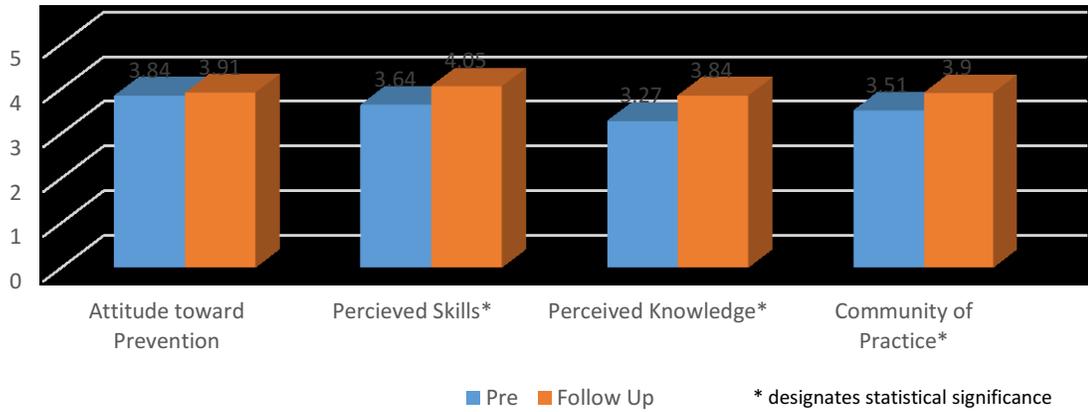


Chart 2: Comparison of Scores among Attendance Groups

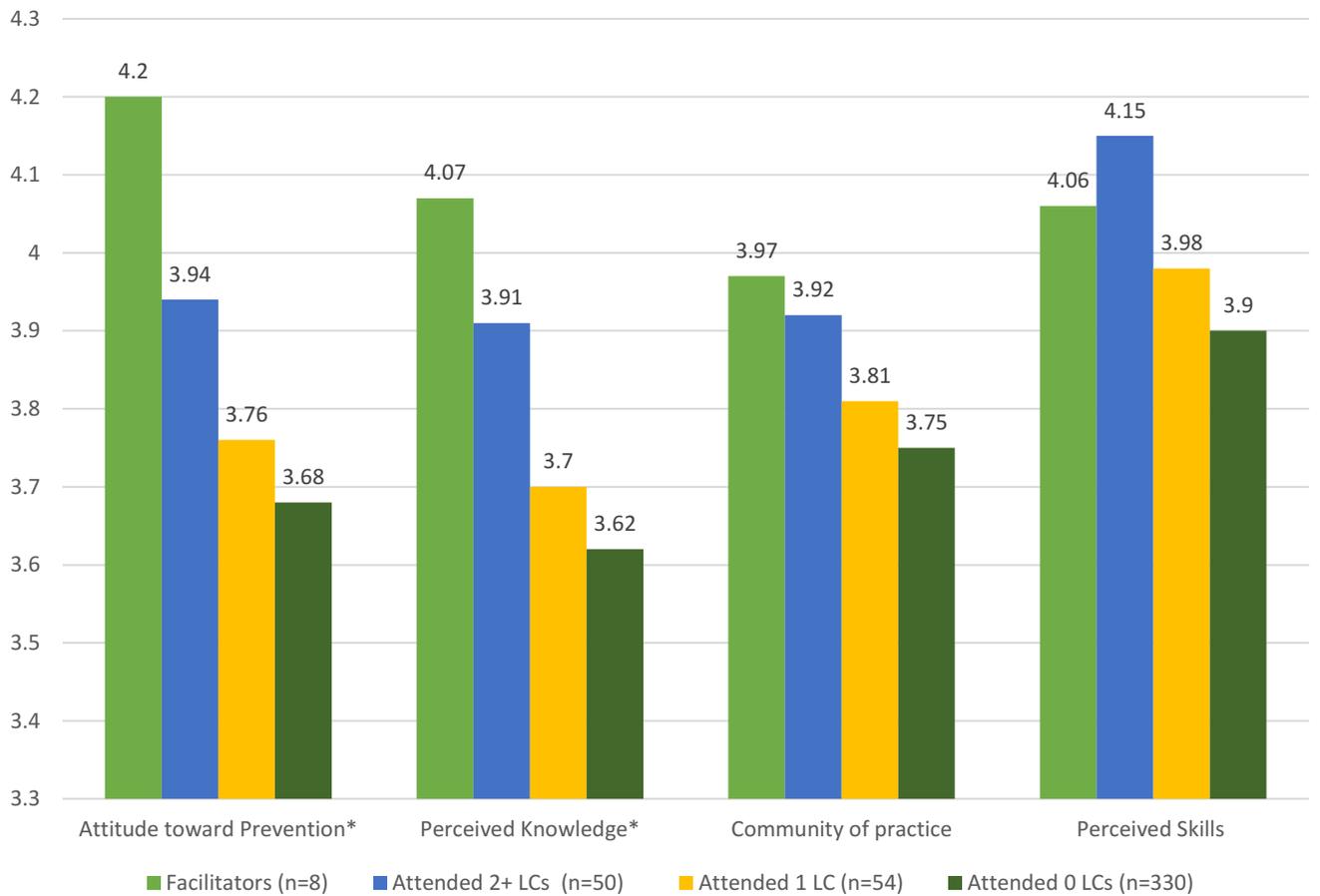


Table 3: Prevention Behaviors of PC CARES Participants and Others

| Survey conducted in the communities at least 3 months after the last PC CARES learning circles | % of PC CARES attendees (2+ sessions) who took action (n=63) | % of non-PC CARES attendees who took action(n=384) | p value |
|--|--|--|---------|
| Prevention Behaviors taken in the last 3 months | | | |
| Talked with community members about wellness | 81.8% | 39.9% | 0.0001* |
| Done something for prevention when worried about someone's risk of suicide | 85.5% | 72.4% | 0.0408* |
| Had conversations about making it harder for an 'at risk' person to get a loaded gun | 48.1% | 31.8% | 0.0189* |
| Let people know what resources are available for prevention | 73.2% | 54.7% | 0.0097* |
| Opened up to share your thoughts | 96.4% | 68.6% | 0.0001* |
| Participated in wellness activities (activity nights, talking circles, community events) | 94.9% | 49.0% | 0.0001* |
| Spoken up about community protective factors | 57.4% | 25.3% | 0.0001* |
| Spoken up on what community organizations can do to reduce risk of youth suicide | 67.2% | 36.5% | 0.0001* |
| Suggested ways community organizations could work together to increase wellness | 86.0% | 42.2% | 0.0001* |
| Talked about how youth suicide attempts happen more often in the summer | 59.6% | 32.3% | 0.0001* |
| Talked about the impact of culture loss on the lives of young people in your community | 80.7% | 56.8% | 0.0006* |
| Talked to others in your family about wellness | 83.1% | 68.8% | 0.0266* |
| Talked with others about history and suicide | 73.2% | 59.3% | 0.0478* |
| Talked with others about how showing you care can reduce the risk of suicide | 93.1% | 82.2% | 0.0375* |
| Talked with others about how to prevent suicide | 86.4% | 70.4% | 0.01* |
| Talked with someone about how culture can promote youth wellness | 78.0% | 49.7% | 0.0001* |
| Tried to listen more to a young person that you know | 96.5% | 86.2% | 0.0289* |
| Trusted others in the community to hear what you have to say | 83.9% | 68.1% | 0.0162* |
| Worked with others to increase wellness in the village | 83.9% | 49.4% | 0.0001* |

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